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A POST-GRADUATE LECTURE ON OCULAR PAIN.

By PERCY DUNN, F.R.C.S.,

Senior Ophthalmic Surgeon, West London Hospital; Lecturer on Ophthalmology, West London Post-Graduate College.

IN the 'Lancet' for 1901 I published some remarks upon ocular pain, which I now propose to amplify. It is natural that an organ like the eye should, in the course of the disturbance of its nutrition, especially exhibit the symptom of pain. Reflect a moment upon the extent to which the eye has been endowed with common sensation. In acuteness of sensitiveness the only organ in the body which can compare with the eye is the testis. But the sensitiveness of the testis is confined to its parenchyma, while the eye has not only an elaborate distribution of sensory nerves within its capsule, but it also possesses a sensitiveness upon its surface, in comparison with which there is nothing in the body to equal it. We may rightly assume that this special endowment of Nature is mainly for defensive purposes—in other words, to enable the eye to protect itself against the effect of injury. For example, the acute suffering caused by a foreign body beneath the upper lid compels instantly steps to be taken for its removal. By this necessary procedure, not only is the pain relieved, but the cornea is protected from harm, which the friction of the foreign body upon its surface, with the movement of the lid, would create.

Then pain is a valuable index of the onset and progress of diseased ocular processes; moreover, its presence is especially useful as a guide to treatment. Some of the inflammatory states of the eye are unaccompanied by pain when the attack is of limited degree; on the other hand, aggravated forms of such attacks are commonly acutely painful. Take, for example, iritis; although more or less

usually painful, a case of this disease is not necessarily so when of mild form, but a severe attack of iritis is always distinguished by a painfulness which is very acute, demanding at once active treatment if the case is to terminate favourably. Furthermore, not only is the presence or absence of ocular pain useful as a guide to treatment, but it is also of value in a diagnostic sense. A good illustration of this may be given in respect to the differential diagnosis between iritis and cyclitis An uncomplicated iritis is not painful when the eyeball is palpated, but palpation is always painful when an iritis is associated with cyclitis, or when, as in acute glaucoma, disturbance of the nutrition of the ciliary processes is also present.

Ocular pain differs in its character, its origin, its progress. Taking a broad classification, it may be described as—(1) extra-ocular, (2) intra-ocular. Extra-ocular is that which arises from lesions of the conjunctiva and cornea.

As a mucous membrane the conjunctiva differs from other tissues of the same nature in the body: first, because of its extreme laxity, in order to allow of freedom of movement of the eyeball, and secondly, because of the higher degree of its sensitiveness. The nerve-supply of the conjunctiva, derived from the lacrimal and the supra- and infra-trochlear branches of the nasal nerve, is especially abundant. By this is explained the intolerance which the conjunctiva exhibits to irritants, whether mechanical, chemical, or micro-organic. Under normal circumstances, however, pain, as such, is not a feature of conjunctival lesions; the discomfort so caused scarcely amounts to pain—"soreness" or "smarting" are the terms usually employed to describe it. On the other hand, we are familiar with the complaint of actual pain, even causing photophobia, in the conjunctival inflammations of neurotic patients.

Of the nerve supply of the cornea, upon which its acute sensitiveness depends, many interesting details could be cited. Not only has provision been made for an excess of this supply, but also that the excessive nerve distribution shall not interfere with the special feature of the corneal structure, namely, its transparency. The nerve branches to the cornea are derived from the anterior ciliary nerves. As many as from sixty to eighty of these branches from the plexus annularis at the sclero-corneal junction pass into the corneal substance. Soon after entering the cornea they lose their medullary sheaths, thus becoming transparent, just as is the case with the fibres of the optic nerve in the formation of the retina. However, for our purpose here it is not necessary to discuss further the minute and special distribution of the nerve supply of the cornea. Nevertheless, this reference, brief as it is, will suffice to provide us with the text upon which to pass under review the various features of ocular pain associated with corneal lesions. The simplest form of ocular pain in this connection I have already referred to, namely, that caused by a foreign body beneath the upper lid. The matter is worthy, however, of further mention, inasmuch, simple as it may be, it sometimes leads to an error in diagnosis. Some particle of a foreign substance, such as, for example, coal-dust, is blown by the wind into the conjunctival sac. The patient at the time is unaware of this fact, and so has no history to give of its incidence. Meanwhile, after some hours, with the movements of the lids, the foreign body may gradually be worked beneath the surface of the upper lid, and then begins the irritation of the cornea. By this time the eyeball has become injected, and the redness exhibited is attributed to exposure to a draught or cold wind. So it is that one meets occasionally with cases of this description which are being treated for conjunctivitis. And yet a mistake in diagnosis can be very readily avoided. The common so-called catarrhal conjunctivitis is usually binocular; when, therefore, the symptoms are confined to one eye, and pain in addition is complained of, as a matter of routine the upper lid should always be everted and examination made for a foreign body. Allied closely with these cases are the frequent ones in which a foreign body has become impacted upon the corneal surface. There, again, the cause of the trouble may be overlooked. Sometimes the foreign substance is of so minute a description as to require very careful examination by focal illumination for its detection. Again the signs are monocular, and again the injection of the eyeball which is present may suggest a conjunctivitis. In the matter of a differential diagnosis it should be remembered that the character of the injection of the eyeball differs in a case of conjunctivitis from that due to a corneal lesion. In the former the injection of the vessels is most marked at the fornix, while in

the latter the vessels involved are scleral vessels surrounding the cornea. The so-called circumcorneal zone of the older writers indicates injection of these latter, arising from lesions of the cornea, iris, and ciliary body. Another diagnostic point is the difference in colour between the redness due to ciliary inflammation and that associated with conjunctival injection. The former is always represented as a diffused pink circle of injected vessels, the latter as of a brick-red colour, with the vessels freely movable over the surface of the sclerotic and capable of being emptied by pressure with a finger. Such pressure, of course, has no effect upon the deeper seated scleral vessels, whose injection gives rise to the so-called circumcorneal zone.

For the most part, the simple procedure involved in the removal of a foreign body from the corneal surface demands the observance of a principle of treatment which should never be overlooked. There is always, consequent upon this small operation, some abrasion and loss of the corneal epithelium; implied in this condition of the cornea is that which follows, say, a wound of the skin; in each instance the means is provided for the inoculation of septic organisms, and in each case the wound may be infected with septic matter at the moment of its infliction. Obviously, therefore, it is sound practice to provide against the results of such a contingency. In all such cases of corneal injury, after the instillation of cocaine, both before and after the removal of the foreign body, the conjunctival sac and cornea should be thoroughly douched with a warm solution of 1 in 4000 chinosol. By this small, though necessary precaution, consequences may be safely averted which otherwise would be quickly prone to declare themselves. But so it happens, that not only the cornea, but even the integrity of the eye itself, may be seriously imperilled by septic invasion following a corneal injury, in which neglect of this important detail has been allowed to occur. Let me cite one or two instances by way of illustration. A common example of this form of injury is met with among nursing women. The infant in arms scratches its mother's cornea with the nail of one of its fingers; with the septic matter accumulated in the furrow of the nail the abrasion is infected, and long before the necessary treatment is available a severe form of septic ulceration ensues. One of the worst cases of the kind which has come under my observation occurred in a man; in him, however, the injury was inflicted, not with the nail of a baby's finger, but with that of the landlady of the house in which he resided. A lurid scene of domestic infelicity can thus be imagined by the recital of this history. A characteristic of these septic ulcers is that they are extremely painful. Frequently they are accompanied, even in adults, by so much blepharospasm as to render it a difficult matter to examine the eye. The explanation probably lies in the fact that the toxin created by the infecting micro-organism acts as a powerful irritant to the nerve-endings of the cornea exposed in the wound. In children this blepharospasm occurs in a form so acute as to make the examination of the eyes impossible without general anæsthesia. Let me now add a few words in regard to those cases in which blepharospasm, or blepharophimosis, as it is otherwise called, is manifested as the most prominent symptom. The cases are confined to the children of the poor, in whom a history can be obtained of recent recovery from an attack of measles, whooping-cough, or other exanthem. The general systemic condition is one of malnutrition, precisely that in which an ulcer of the cornea is most prone to occur. When the child is brought to the hospital for treatment, apart from the blepharospasm, it is observable that the lids and the skin of the face are conspicuously filthy and septic. Any attempt to examine the eyes is followed by screaming and forcible resistance on the part of the child. And yet such examination is very necessary for the purpose of ascertaining the condition of the cornea. A general anæsthetic is according employed. The filthy state of the eyelids and surrounding skin is partly the result of neglect, partly the fault of the child rubbing its eyes and face with its dirty hands to obtain relief from the pain from which it is suffering. Then, after thorough cleansing of the parts with a warm solution of chinosol, the lids are separated, either with the surgeon's fingers, or with a retractor, care being employed in either case to avoid causing any pressure upon the eyeball. On exposing the cornea it is seldom that we find anything more than one or two small superficial ulcers, these being out of all proportion to the severity of the symptoms presented. Whence, then, is the source of this acute photophobia and blepharospasm. The probable explanation is the following: There is

first a small corneal ulcer ; the discomfort to which this gives rise causes the child to rub the eyelids with its dirty hands. In time, by this means a septic conjunctivitis is excited ; the toxins thus created reach the ulcerated surface of the cornea, and acting as a powerful irritant upon the nerve-endings, give rise to extreme pain, which reflexly excites the acute blepharospasm. In all such cases the child should be admitted into hospital. To attempt any treatment in the out-patient department is futile, more especially as the first principle thereof, namely, cleanliness, is not one which lies within the power of the mother to enforce. For the first few days chinosol fomentations, two-hourly, are applied, and irrigation of the lids and, as far as the blepharospasm will admit, of the conjunctival sac, with a warm solution of chinosol twice daily ; in addition, 4 per cent. cocaine drops are instilled ; the child is also placed on a generous diet, and a mixture of iron and quinine administered. As a rule rapid improvement follows this plan of treatment. The photophobia and tonic contraction of the orbicularis diminishes ; the child ceases to bury its face in the pillow, and makes furtive attempts to open its lids and use its eyes when the dressings are removed. In passing, it may be pointed out that in earlier days heroic measures were deemed to be necessary in order to overcome this reflex spasm of the orbicularis. A common practice was to suspend the poor creature by the legs, and plunge it head downwards into a pail of cold water. This primitive procedure, as far as can be judged, was in many cases effectual ; the spasm was certainly subdued. But nowadays such treatment must only be regarded as unnecessary and barbarous. The treatment of traumatic ulcers of the cornea in the adult scarcely belongs to the subject-matter of this lecture ; nevertheless, inasmuch as reference has been made to this form of corneal lesion, by reason of the character of the pain with which it is associated, I may usefully, I hope, direct attention to the means of alleviating this symptom. Just as in the case of the child, so with the adult, thorough antisepticism is essential. After the instillation of cocaine douche the conjunctival sac and cornea with a warm chinosol solution, and afterwards apply moist heat in the form of chinosol fomentations every two hours. In the severest cases, by this means, relief from all pain speedily follows.

I have now to deal with the second class of cases of ocular pain, namely, that associated with intra-ocular causes. Pain is here a valuable symptom as a guide to treatment. It is an unfailing index of the progress of the case ; it indicates a variety of conditions associated with its onset, and just as the conditions vary which cause it, so does the treatment necessary for its relief. Let us first consider pain accompanying iritis. The pain of an iritis varies with the intensity of the attack ; it differs also in character, according to the cause of the iritis. In iritis due to syphilis it may be scarcely a noticeable symptom ; on the other hand, in this disease it may be of the acutest description. Again, the pain of a syphilitic iritis is continuous, until relieved by treatment. Moreover, after having been once relieved it does not recur. The acute character of the pain which occasionally supervenes in syphilitic iritis is due to the extreme vascularity of the eye which characterises the attack. The whole organ is in a highly vascular state, and the compression of the sensory nerve branches, especially those of the iris by the blood stasis, is probably accountable for the acute suffering present. Then, in so-called rheumatic iritis the pain is seldom continuous, nor does it ever reach the extreme degree of acuity which is occasionally met with in iritis due to syphilis ; it has periods of remission and of exacerbation ; in the daytime, for example, it may be scarcely troublesome, but for the most part at night-time it is always present to a disturbing degree. Nocturnal pain, then, of a rheumatic iritis is usually the rule. Again, the pain is subject to recurrence after having been relieved by treatment. In the course of an attack, while the eye is under treatment, there may be relapses of pain—relapses which indicate a recrudescence of the disease generally. With rheumatic iritis that is commonly the sequence. Recurrent attacks, at intervals, distinguishes this disease from other forms of iritis. There is no certain knowledge in respect to the precise dyscrasia upon which the disease depends ; common experience, however, teaches that in men a history is obtainable of a previous gonorrhœal attack, and chronic pain in muscles and joints may also be present at the time of the attack of iritis. With regard to treatment, the pain of an acute iritis in syphilis demands local depletion. Two leeches should be applied to the temporal region, and by

this means relief is usually speedily obtained. Here, however, it may also be pointed out that another indication for local depletion in cases of syphilitic iritis is when, in the early stage, the iris fails to respond to the instillation of atropine owing to the engorgement of the iris vessels with blood. In rheumatic iritis pain is relieved by a different method. Dry heat is that which is most useful. A large bran poultice as hot as it can be made is placed over the eye and forehead at bedtime and secured in position by a flannel bandage. In a short time the pain ceases and the patient falls asleep, without any further treatment being necessary.

The next class of cases to be considered in which ocular pain is a prominent symptom is that of acute glaucoma. The pain here is almost entirely of ciliary origin. It is continuous, throbbing, and generally intense. Its ciliary origin is proved by the extreme tenderness of the eyeball, palpation of the organ in order to ascertain the tension being a procedure from which the patient involuntarily shrinks. The cause of the pain is readily explained when reflection recalls the character of the changes of which a glaucomatous eye is the seat. In this regard, however, it is unnecessary to discuss the whole question of the pathology of the disease; it will suffice to say that owing to blocking of the angle of the anterior chamber the tension of the globe rises. As this becomes greater so the intra-ocular blood-pressure increases. With this increased blood-pressure and raised tension the ciliary processes become engorged with blood, and compressed, and any such disturbance of the nutrition of these delicate structures is provocative of pain in greater or less degree. The intensity of an attack of glaucoma varies with the degree of vascularity present, and for the same reason the amount of pain varies. For example, in a case of subacute glaucoma the pain complained of is generally insignificant; on the other hand, in a so-called fulminating attack of glaucoma the pain may be so intense as to cause vomiting. For the relief of pain in glaucoma an immediate iridectomy is the only remedy.

I have now to speak of another class of cases in which pain is present as the result of disturbance of the ciliary body. A case by way of illustration may be cited. A child inflicts a penetrating wound upon one of its eyes with the point of a pair of scissors. Under treatment the would heals, and the eye for the time being recovers. But it subsequently begins to soften, and in time passes into a shrunken, atrophic condition. Nevertheless, it causes no discomfort, shows no sign of reaction, and in its present state calls for no immediate treatment. Some years may elapse before anything further becomes noticeable in the eye. Then, however, it happens that the patient begins to feel a frequent impulse to rub it. The eye looks and feels irritable, and ultimately it becomes the seat of a dull, aching pain. On examination the atrophied globe is found to be injected, tender on palpation, and lacrimating. The cause of the onset of these symptoms is due to secondary degenerative changes ensuing in the atrophied ocular tissues. Calcification, for example, of the choroid is a common result of such changes. As soon as this begins to extend to and involve the ciliary processes, or what remains of them, there is excited a passive form of cyclitis, the development of which is manifested by the symptoms described above. The only remedy under such circumstances is enucleation of the atrophied eyeball. A lost eye, from whatever cause arising, which is painful on palpation, should always be removed, not only for the purpose of relieving the local discomfort, but also because of the possible risk of the atrophied remains exciting what the old writers called "sympathetic ophthalmia," that is to say, infective cyclitis.

The last class of cases to which I may allude is that in which pain is a symptom of panophthalmitis. Suppuration of the eyeball is usually the sequel of a penetrating septic wound, either with or without the retention within the eye of a foreign body. The signs present in either case are œdema and redness of the lids, especially the upper one, chemosis of the conjunctiva or extreme vascularity of the eyeball, increased tension, continuous pain of an acute, often agonising degree, and more or less proptosis. While panophthalmitis is usually traumatic in origin, occasionally a case is met with in which the infection is endogenous; for example, a woman was admitted into the wards some months ago from a cottage hospital in the suburbs. The left eye had been for some days acutely painful; there was some proptosis, the tension was + 2, the cornea steamy. The patient was suffering from pyæmia, following puerperal sepsis; large collections of pus were present over the buttocks, the right forearm, the left thigh, and left elbow. The ocular signs were due to septic embolism, causing panophthalmitis.

The case was not one for enucleation, owing to the desperate condition of the patient. A general anæsthetic was administered and the eyelids separated with a spring-stop speculum; then a Graefe's knife was taken, and the globe was transfixed on the outer side midway between the poles, and the knife carried backwards for half an inch parallel with the axial line. The purulent contents then escaped from the globe, and the cavity of the eyeball was douched out with a warm solution of 1 : 2000 chinosol. The numerous purulent collections were evacuated, after the eye had been attended to, and the patient was removed to bed. She lived for some days in comparative comfort, and ultimately died of a septic pneumonia. There are two methods of relieving the pain accompanying panophthalmitis: First, by enucleating the eyeball, and secondly, by evacuating the pus by an incision through the ocular tunics, as was done in the case above mentioned. The belief was current some years ago that the enucleation of a suppurating eye was an operation attended with some risk. Cases of the kind were recorded in which death followed as a result of purulent meningitis. It was held that the septic meningitis was due to infection from the operation wound; hence the lesser alternative, namely, incising the eyeball, was regarded as the better method of dealing with these cases. Personally, however, I am doubtful whether the facts have been sufficiently proved against enucleation in panophthalmitis. In the first place, with the single exception above narrated, I have always removed suppurating eyeballs, and in no case has any ill-result occurred. In one case a highly septic condition was present, following the perforation of the eyeball with the knot of a whip, which was afterwards found inside the globe. Moreover, fatal meningitis has followed panophthalmitis in which no operative procedure has been undertaken. We may reasonably, therefore, conclude that as it is impossible to determine the precise time at which the meningitis begins, it is quite possible that in the fatal cases of enucleation for panophthalmitis the disease had begun before the operation was performed. No actual contact with the septic organisms causing the panophthalmitis takes place at the wound resulting from the enucleation; consequently, in these days of aseptic and antiseptic methods of operating, it is difficult to accept the theory that the fatal meningitis arises from the infection of the enucleation wound by the suppurating and septic eyeball. Meanwhile it is obviously sound practice to remove at the earliest moment an eye in a condition of active suppuration, not only on account of its proximity to the meninges, but because of the risk of the extension of the inflammation backwards along the lymph-channels of the optic nerve-sheath.

October 10th, 1910.

A CLINICAL LECTURE

ON

MALIGNANT DISEASE OF THE TESTIS.

Delivered at the London Hospital.

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GENTLEMEN,—I have taken for to-day's lecture the subject of malignant disease of the testis for two reasons: first, because in the London Hospital we have, I believe, the finest collection of specimens of malignant disease of the testis which exists. This collection was started by Mr. Curling and has been added to considerably of late years. Secondly, I chose this subject because I have been paying some attention to it myself, and I want to put forward one or two views which are perhaps different from those which are generally accepted and which are found in most text-books. I use the term "malignant disease of the testis," rather than "sarcoma" or "carcinoma of the testis" for a special reason. One might have called the lecture "Tumours of the Testis," or "Neoplasms of the Testis," but I took the term "Malignant disease of the Testis" for the purpose of impressing upon you that there are practically no innocent tumours of the testis. Cases of innocent tumours of the testis have been described, such as osteoma, myoma, and fibroma, but if you look into these descriptions carefully—and such descriptions are very few—you will find practically all of them are really examples of inflammatory conditions, or tumours of the tissues surrounding the testis, rather than of the testis itself; scarcely any of them will be found to be actual tumour-formations in the organ. The only case of osteoma of the testis described in the 'Transactions of the Pathological Society' was a piece of bone found in the epididymis of a person who had had gonorrhœal epididymitis and a urethral stricture for many years, and who died of renal insufficiency and the other symptoms which are the general sequelæ of stricture. The osteoma was in the epididymis, and was undoubtedly inflammatory in origin.

One can say that for practical purposes an innocent tumour of the testis does not exist, and

the cases of the kind which have been described, if they are true neoplasms, are simply pathological curiosities. Therefore when one deals with a tumour of the testis one knows at the outset that it is a malignant tumour and not an innocent one, and hence you will see it is desirable that in regard to the testis such terms as "adenomata of the testis" and "chondromata of the testis" should be dropped. An apparent exception to what I have just said may be regarded to obtain in the case of dermoid tumours. Dermoid tumours of this part certainly do exist, but I think only four cases have been described in the last twenty-five years in England, so that again the exception is a very, very rare matter, but that is a point I want to touch on later, if I have time.

I do not want to occupy your time in this lecture by giving you the text-book descriptions of the differential diagnosis between sarcoma and carcinoma of the testis, or even between these tumours and the so-called fibro-cystic disease. These conditions exist pathologically, but it is impossible to make clinical distinctions between them. The differential recognition of different growths cannot be made clinically in regard to the testis, and when surgeons speak of them clinically as sarcoma or carcinoma, they are going beyond what they can legitimately do; they cannot make a clinical distinction between sarcoma, carcinoma, and fibro-cystic disease. I will take some specimens from the museum. Here is one showing malignant disease of the testis. It happens, on microscopical examination, to be a sarcoma, but it would be impossible to say that it is sarcoma on examining it clinically, or even looking at the specimen removed from the body. Here is a second specimen, of the same uniform appearance, and this is a carcinoma, and I think you will agree that no naked-eye distinction between them is possible. Here is another form of growth, which also it is impossible to distinguish from the others clinically, and is a case of endothelioma of the testis. The nearest we can get to a diagnosis clinically is, that there is malignant disease of the testis. From fibro-cystic disease, again, the distinction is impossible from the clinical point of view. In this specimen there are several large cysts, which possibly you might have been able to feel clinically, but even then you could not distinguish them from the localised collections of fluid in the tunica vaginalis which are common in association with other forms of malignant disease. In this next specimen of fibro-cystic disease there are no obvious cysts present in the clinical sense at all; none can be found except microscopically. There would be in such a case, a large solid tumour of the testis, fibro-cystic disease, clinically indistinguishable from sarcoma and carcinoma. Therefore one arrives at the conclusion that clinically we can only diagnose malignant disease of the testis, and the microscope is necessary to arrive at a pathological diagnosis.

It happens that one is lucky enough to have a case of malignant disease of the testis in the hospital which came up quite by accident, and that is the case of this boy. This enables me to show you the various points of diagnosis, from the clinical standpoint, better than if one simply spoke about them without the case in front of you.

The cause of malignant disease is unknown, and it occurs at all ages of life. This boy is an example of malignant disease of the testis occurring in a young subject. I collected eighty cases of malignant disease of the testis, and five of them were in children under twelve years of age. I have also seen the condition in a man æt. 80 years, but it is most common in subjects who are rather young for malignant disease in other parts of the body. If you even exclude the cases which occur in infants and take the average age of men who suffer from malignant disease of the testis, you will find it comes out at something between thirty and forty years of age, which of course is rather young for malignant disease in general. The only other ætiological factor which we know of in connection with this disease is injury. You find it stated that injury precedes malignant disease of the testis in a large number of cases; one writer says that no less than 50 per cent. of cases give a definite history of injury. I do not think that is correct. In the fifty-nine cases which I investigated closely, there were only three in which there was a definite history of injury preceding the disease, and of these there was only one case in which the patient had an injury of the testis, and immediately after the injury the testis became swollen and shortly afterwards was the seat of malignant disease. To find definite conclusive history of injury in malignant disease of the testis is not common, but to get a vague history of some accident to the organ some time before is common.

That question of injury is at the present time of the utmost importance, because from the point of view of the Workmen's Compensation Act, if a person receives an injury to the testis in the course of his employment, and on that there is the development of malignant disease, and the patient dies, the employers of that man are liable to compensate his widow and children for his death. Therefore you will see it is very important for every medical man in these cases to get a clear idea of whether injury is likely to cause a certain disease or not. Personally I would say that malignant disease of the testis directly following an injury is exceedingly rare, and whether it is ever due to injury, very doubtful. That question of injury and disease concerning the Workmen's Compensation Act is extraordinary, and there may be very strange results. I know one case in which a medical man swore in the witness-box that a perforated duodenal ulcer was the direct result of an injury to the knee the patient had received two and a half years before, and the jury brought in a verdict of death due to the accident. Another case I came across was that of a man who had ordinary chronic hydrocele, and he claimed compensation because he saw a man lifting a bag of flour. The man, it was proved, looked on while another man lifted the flour, and when he went home he found a swelling of his testicle, a chronic hydrocele, and his medical man gave him a certificate saying it was due to injury. I use these illustrations to impress on you that the usual statement that malignant disease of the testis frequently follows direct injury, is not, I think, in accordance with facts.

Another question of importance is whether the disease is frequently bilateral, as that statement has been made by French writers. English writers have not discovered this particular form of malignant disease of the testis, and in the cases which I obtained there was no case of bilateral disease, and it is probable those cases described by French writers were secondary growths in the testis and not primary neoplasms at all. A very important ætiological question is whether malignant disease of testis is more likely to occur in the undescended or imperfectly descended testis than in the normally placed organ. There has been a good deal of controversy on this point. A writer on imperfectly descended testis came to the conclusion definitely that malignant disease was not more common in the imperfectly descended testis. That has been disputed. Jacobson also wrote on the subject and decided that it was. From the point of view of the investigation which I have made in the records of this hospital there is no doubt, as far as the London Hospital is concerned, that malignant disease of the testis is considerably more common in the imperfectly descended organ than it is in the properly descended, and if this is universally correct it is a very strong argument for the removal of all imperfectly descended testes. One case which is worthy of mention was that of a man who had an imperfectly descended testis on one side, and malignant disease of the testis on the other side. That, of course, was a mere peculiar coincidence. With regard to ætiological factors, therefore, beyond saying that it occurs at all ages and that there is no reason for its onset, that it has a definite relation to imperfect descent of the testes, we know nothing about the cause of malignant disease of the testis.

What are the clinical features of this disease? The most striking are its entire painlessness and the absence of symptoms. As a consequence, one rarely sees malignant disease of the testis until it is well advanced, and one will not find the physical signs of malignant disease, as generally described. Another reason why the disease is seen late is that the organ retains for a long time its natural shape. This boy gives a history of only ten weeks' duration, but there is everything to make us believe that it is malignant disease of the testis. It is painless and causes him no inconvenience, and it has probably been growing much longer than ten weeks. Pain is absent from the disease until there are secondary growths in the glands, the glands affected being the lumbar. The secondary growths exert pressure on the cœliac plexus of nerves, and cause neuralgic pains in the abdomen, and pressure on the vena cava causes secondary symptoms such as ascites and œdema of the legs. In the early stages, however, there is nothing but slight inconvenience due to the size of the testicle. On the skin all that you notice is the presence of big veins crossing all over the testicular area, and this enlargement of the veins is due to the congestion caused by the presence of the tumour. The skin is freely movable over the tumour until quite a late stage, because these tumours, as you can see in these specimens, remain encapsulated by the tunica albuginea for a long time, and so there is no adherence to the skin till

late. Frequently, however, there are a certain number of adhesions between the layers of tunica vaginalis which make the skin slightly less freely movable over the organ than normal, but there is no fixity in one place, such as occurs in carcinoma of the breast. Hydrocele is present in a large number of cases, but is generally a small localised hydrocele, with some adhesions, and some fluid in the tunica albuginea. A large hydrocele is distinctly uncommon. Another statement which is quite correct but is exaggerated is that of the presence of a blood-stained hydrocele. You will hear it stated that in malignant disease of the testis there is a blood-stained hydrocele, but as a regular indication of malignant disease of the testis that is an error. It does, however occasionally occur. Mr. Jonathan Hutchinson has figured one specimen in the New Sydenham Society's 'Atlas of Diseases of the Testis,' and in that case there is one blood-stained hydrocele. But in eighty cases which I examined not one of them showed this condition. When it does occur it is an important sign, but it is very rare. Why it is considered to be more common is, that these tumours nearly all give a sense of fluctuation because they are very soft growths, encapsulated in the strong tunica albuginea. I think the tumour in this boy is a solid growth, although there is fluctuation here. If you puncture one of these soft vascular growths, you can frequently withdraw a syringe-full of blood. There was one tapped in this hospital from which two drachms of blood were withdrawn, and because of this it was diagnosed as a hæmatocele. It was found afterwards to be sarcoma of the testis, and the blood which was obtained had been drawn out of the tumour itself, not from a blood-stained hydrocele. These tumours are very soft and vascular, and the slightest injury will make them bleed, and you can, as I say, get up to two drachms from them by simply tapping the solid growth.

The disease affects the body of the testis. Sarcoma of the epididymis does occur, but it is rare. I was able to find in the whole of the London Hospital Museum five cases only, and they were not altogether satisfactory examples. In malignant disease, the differentiation between body and epididymis does not help much in the diagnosis. The reason for that is explained by looking at these specimens. The whole body of the testis becomes enormously enlarged, and the epididymis is so spread out over the surface of the tumour, that one cannot distinguish it, even after the organ has been removed from the body. You see how the epididymis is crowded away at the top, and it is practically impossible to distinguish it, even when you have removed the tumour and bisected it. In this boy one cannot feel anything like an epididymis; you cannot differentiate the body of the testis from the epididymis. In the only case I saw in which it could be distinguished, it led to an error of diagnosis. It was the case of a man, æt. 60 years, who came into the hospital, and clinically this is what was felt. At the lower end of the epididymis was apparently a large nodule. The obvious diagnosis was tubercle of the epididymis—tubercular epididymo-orchitis—and the organ was removed. On examination the diagnosis was found to be incorrect; the nodule was a small sarcoma in the body of the testis. It is the earliest case I have ever seen, and owing to an error in diagnosis one was able to remove from that patient, quite by accident, a very early sarcoma. So remember that when the tumour is of any size, the epididymis cannot be distinguished from the tumour mass.

The cord becomes generally thickened in the early stages by hypertrophy of the cremaster muscle, and an increase of the blood-supply, the vessels becoming enlarged. In late cases one gets big secondary nodules running up the cord, in some instances the nodules being equal in size to a normal testis. That is a late condition as a rule. Examination *per rectum*, which, of course, should always be carried out in cases of enlarged testis, shows no changes in the prostate or vesiculæ. The glands affected are the lumbar glands, and one not infrequently finds a mistake made in the position of the lumbar glands. The aorta comes down and bifurcates just below and to the left of the umbilicus. The renal artery comes off the aorta above the umbilicus, and the lumbar glands lie in the region between the bifurcation of the aorta and the renal artery, and that is where the mass of glands occur in malignant disease of the testis. I frequently find that in the cases examined by students the glands, even when much enlarged, are not felt, owing to the lower part only of the abdomen being examined. In this case I cannot feel enlarged glands, but in a large number of cases, particularly in young subjects, the lumbar glands are affected early. In five

cases of children under twelve years of age in whom I found malignant disease of the testis, the lumbar glands were already affected when the patients were first seen, but in this boy, who is the sixth case, I cannot feel them. Occasionally one finds the inguinal glands affected. That occurs in two ways: When the growth fungates and infects the skin the glands are secondarily infected from the skin; in other cases a secondary mass occurs in the groin after the lumbar glands are involved, due to blockage of the normal lymph-path. So remember the inguinal glands may be affected. The vas is unaffected. A hydrocele is not to be distinguished, as a rule. Occasionally it can be, and then it may be blood-stained. To sum up: the organ is enlarged as a whole, retaining the shape of the testis, feeling more or less uniform all over; epididymis not distinguished; cord may be nodular with big masses. Vas unaffected; rectal examination reveals nothing; lumbar glands may be enlarged. On these points one diagnosed in this boy malignant disease of the testis.

But there are one or two errors which may be made in the diagnosis, and they are such important errors that we must give them a little notice. The most common cause of mistake is a hæmatocele. Here is an example of hæmatocele which was removed because it was thought to be malignant disease, and this specimen has a curious history. The patient came and saw me in the out-patient department, and from certain statements in the history I diagnosed syphilitic orchitis. I gave him iodide of potassium and mercury, and showed him at the Medical Society here as a case of syphilitic testis. Everybody agreed. As he did not improve on the treatment I was giving him, I altered my diagnosis to malignant disease of the testis. Again I showed him to the Medical Society. Members did not recognise him as the same man, and again everybody agreed with the diagnosis. The patient was taken into the hospital, and the surgeon under whose care he was removed the testis, and after removal found it was chronic hæmatocele. I have a second and a third case here. Hæmatocele is fairly easy to diagnose if you have a history, but in its absence differential diagnosis is often impossible There are the same clinical features—a big, soft mass, quite painless, occurring in the scrotum. You cannot distinguish the epididymis, the vas is unaffected, there is nothing abnormal about the cord. The two conditions are precisely similar and differential diagnosis between the two is impossible. It follows that you should always explore a case of malignant disease of the testis before actually deciding on removal. If you make an incision into the mass you will see what you are dealing with, and if you are dealing merely with chronic hæmatocele the testis need not be removed. If the glands are affected the diagnosis is certain, but in all other cases explore the malignant disease before actually performing castration. Hæmatocele has been found to lead to more mistakes than any other condition.

The second cause of error in diagnosis is syphilis. You will see the reason of that from this specimen. Here is a testis with a big gumma occurring in it, and with a blood-stained hydrocele of the tunica vaginalis. The body of the testis is enlarged by the gumma, the epididymis is stretched over the tumour, the vas and cord are unaffected. The diagnosis from malignant disease is very difficult. One has to take the history and evidence of syphilis elsewhere, to do the Wassermann reaction, etc., and in many cases the diagnosis is made by seeing the results of treatment. The treatment by medicines to discover whether a lump is syphilitic should not be too prolonged, and if the mass does not rapidly get smaller under iodide of potassium you should explore, otherwise you will be too late, for the disease will have made too great progress. Another condition with which malignant disease of the testis is confused, is tubercle. In the form of disease which I show you here, which is one of the kinds not generally recognised, the diagnosis from malignant disease is extraordinarily difficult. In this form of tuberculosis of the testis, the disease does not start in and remain limited to the epididymis as it usually does, but spreads into the body, and from the body spreads into the cellular tissue, so that there is a large mass of fibrous tissue filling up the scrotum. The body cannot be distinguished from the epididymis; the whole is one solid mass of fibrous tissue, with the remains of the testis buried in the middle. Thus you see there is apparently a solid tumour of the testis indistinguishable from malignant disease; it is a chronic diffuse tuberculosis. It is very essential in the majority of cases to explore suspected malignant disease of the testis before removal.

Now, before we come to consider the pathology, let me say a few words about treatment of this condition. The modern treatment of any malignant disease, as you know, is free removal of the tumour, with as much as possible of surrounding healthy tissue, and complete removal of the nearest or most directly communicating lymphatic glands. This modern treatment of malignant disease is carried out, as far as possible, in every case. But in treating malignant disease of the testicle this is a difficult matter. One can remove the testis and remove the cord up to the abdomen, but can one remove the enlarged lumbar glands? Is it justifiable to open up the abdomen and remove all the cellular tissue from the bifurcation of the aorta to the renal artery, with the vena cava on the right side, the aortic plexus of the sympathetic surrounding the artery, and on the left side the inferior mesenteric? It is a very difficult and dangerous operation, for at the same time you must remove the whole spermatic vein so as to prevent spread by the venous system. Is one justified in performing such a big operation? It has been done. The first man who did it, I think, was an American named Roberts. He attempted this operation on a case, but the patient developed intestinal obstruction, had a fæcal fistula, and ultimately died. That, of course, is a common history in all cases of pioneer surgery, the early patients succumbing to the operation. This operation, which may have been done many times since the date of which I am speaking, 1902, has been advocated recently by Mr. Bland-Sutton, who described a successful case. The operation is feasible from an anatomical point of view, and it is the only really satisfactory operation that one can do, because we shall see that the prognosis in these cases is very bad indeed. Very few cases survive, and they practically all die with recurrence in the lumbar glands. Local occurrence is rare, but soon after the operation these patients develop a mass of glands in the abdomen, ascites, œdema of the legs, neuralgic pain, and secondary growths in other parts follow, and the patients die. Although one has had several cases of recovery, the prognosis on the whole is very bad, much worse than that of malignant disease generally, so that some drastic operation is necessary if you are going to have recoveries, and prevent recurrences in malignant disease of the testis. I think the operation is justifiable and legitimate, and therefore I shall do it on this boy. I shall first examine the testis, and if it is the seat of malignant disease I shall attempt to remove the testis, the skin of the scrotum, all the spermatic vein, the spermatic cord, and the lumbar glands.* Otherwise the boy is inevitably doomed to death.

Now I shall turn to what to me is more interesting even than the clinical side, and that is the question of the pathology. What is the pathology of these growths? Is there any means of splitting them up into classifications? Anyone who sets to work to read the literature of malignant disease of the testis, rapidly gets into a state of hopeless confusion. The classification is so difficult that Mr. Bland-Sutton in his book on tumours does not talk about carcinoma or sarcoma in the testis, but talks about multiple cystic disease. In the course of some investigations and by reading other people's papers, one has been able to evolve some sort of regularity out of the chaos which exists concerning malignant disease of the testis, and very briefly I shall put before you the results I have reached by the examination of a large number of cases. I started with a large number of tumours, and tried to divest myself of any preconceived notions. The first thing I was able to find out was that there were in the eighty cases which I investigated, thirty which showed a uniform structure, and which were easily described as sarcomata. These specimens were simply round-celled sarcomata, such as occur in other organs, tumours consisting of masses of small round cells invading the testis, and consisting of nothing but small round cells, none of them showing cartilage or cysts. They want no further description; they are simply small, round-celled sarcomata, without any other admixture of other elements. Here is a micro-photograph of a small, round-celled sarcoma invading the body of the testis; there is nothing further to be said about it. Spindle-celled sarcomata are rarer. I was able to find only one in my series. Dr. Lazarus Barlow, of the Cancer Research Laboratory at Middlesex Hospital, gave me another, and I obtained a third, and also I have seen descriptions of other specimens. They resemble spindle-celled sarcoma in general, and are perhaps a little less malignant than the small, round-celled variety. A third variety was that certain of these round-celled growths had a stroma which resembled

* This operation was successfully performed two days later.

that of lymph-glands, and if you like to call them lympho-sarcoma there is no objection to that. Thus thirty out of eighty were sarcomata. Myxo-sarcoma has been described, but this simply means myxomatous degeneration of the tumour. Tumours which have been described as chondro-sarcoma and adeno-sarcoma of testis I think belong to an entirely different group.

There were six cases, which can be labelled columnar-celled carcinomata, and they require no special description. They are exactly the same as columnar-celled carcinomata occurring in the breast —masses of epithelial cells arranged in a certain way; they contained no cartilage nor cysts, and were simply plain, straightforward carcinoma of the testis. Here I show you a micro photograph of one of those specimens—ordinary carcinoma of the testis. There was another straightforward case, one of endothelioma of the testis, which showed the ordinary structure of endotheliomata. It contained blood-vessels, and one saw endothelial cells spreading out from them in all directions. These three varieties are undisputed.

Now come the two varieties of growths of the testis which are the source of the trouble in classification. There were five cases of what we can class as large round-celled growths. They are quite simple in structure, and the trouble is to know where to put them. From whatever part of the tumour you cut the sections the structure remains the same. There are large round-cells with a nucleus, generally with clear cell substance, and the cells are arranged in columns. You can see the tendency for the cells to be arranged in that way in this micro-photograph, and so much is this the case that the sections somewhat resemble the structure of the supra-renal glands. It is suggested that these tumours are really not tumours of the testis at all, but tumours arising in remnants of supra-renal tissue, which have been found in the spermatic cord and in the testes themselves. These tumours have been described by various writers, and various views have been put forward. They have been thought to be tumours of the supporting cells of the testis. Others think that they arise in the supra-renal remnants, others that they are cases of large round-celled sarcomata, and others, again, that they are endotheliomata. My belief is that they are carcinomata arising in the glandular substance of the testes, similar to the glandular carcinomata such as occur in the breast, and if this is agreed to we have two varieties of carcinoma in the testis—columnar-celled carcinomata arising in the cells of the ducts, and glandular carcinoma arising in the essential cells of the organ. There were five of these in the eighty cases. This last classification is disputed, but I put it forward tentatively, and to say the least of it, it is plausible.

Lastly, we come to the group which has given rise to endless confusion—the so-called fibro-cystic disease of the testicle. To enable you to follow what I am now going to say, I must review the work of other writers. These tumours have received all sorts of names. Cooper and others described them under the name of "hydatid disease of the testes," because in them one found cysts somewhat resembling hydatid cysts. Then they have been called adenomata of the testes, because they are in many cases apparently innocent; they are apparently encapsulated, and they are in part composed of non-malignant looking cysts. They resembled cysto-adenoma, for example, in the breast. Adenoma is an unfortunate name, because an adenoma is a simple tumour, and every one of these is essentially a malignant tumour. Mr. Eve described them twenty years ago, and called them cysto-adenomata or cysto-sarcomata. He recognised their malignant nature in most cases, traced them to the pro-nephron, and thought they were malignant tumours arising in embryonic remnants. Many other names have been given to them, and at last, by common consent apparently, fibro-cystic disease is the term most commonly used, and under this term you will find them described in the text-books. The reason is that they all have cysts and they all have fibrous tissue, so fibro-cystic disease certainly describes one of their characteristics. On further examination one finds many other structures. This specimen I show you is Sir James Paget's original specimen of so-called chondroma of the testis. The tumour is composed of ordinary solid cartilage with some cysts. He described it as chondroma of the testis, and thought it was an innocent growth, and after this specimen, a large group of cases was described, chondro-adenoma of the testis, chondro-sarcoma of testis, and so on, and pathologists thought that for some reason tumours of the testis had a habit of growing cartilage. This specimen appeared and

was described as an innocent tumour. Unfortunately some years later this patient died of recurrences in the heart and lungs, and it was therefore concluded it could not have been innocent tumour, and it must have been malignant, or if it were innocent, it caused metastases, which is opposed to modern ideas. Some years ago Kanthack and Pigg obtained the specimen and re-described it, or, at any rate, the half of the specimen which is in St. Bartholomew's Hospital. They described it before the Pathological Society of London as a carcinoma, which, for some reason which they could not explain, developed cartilage. Another gentleman, interested in the question, obtained the description of the specimen by Kanthack and Pigg, and classified it as something different. He said it was neither carcinoma, or sarcoma, or chondroma, but a chorion-epithelioma. I obtained this half of the specimen and cut sections of it, and found it allied to the group of cases under discussion. The question now arises as to what view one is to take of these specimens, of which this is a classical one. On taking a large number of specimens, one finds that they all contain in some part of the tumour similar elements, but if you cut sections of various parts of the tumour you find an extraordinarily diverse structure. This specimen is a tumour which grew very slowly for years, and only caused secondary recurrences after a long time. On examination one finds fibrous tissue, cartilage, and cysts lined with columnar epithelium. Here is a specimen which killed the patient in eight weeks exactly, from the alleged onset of the disease, from secondary growths of the same nature all over the body. It also contains fibrous tissue, cartilage, and cysts, and the sections are so much like the previous tumour that you cannot tell the difference between them. I show you the specimens under the microscope. One was slowly growing for years, and only after many years did it prove to be malignant; the other was highly malignant. Here is another specimen with the same elements in it, but if you cut other parts of the tumour you find typical carcinoma. If you look at such a piece under the microscope you would say here is a typical carcinoma of the testis, similar to those which I have described previously. Take another part and cut a section, and you say it is typical sarcoma. Take another part and cut a section, and you say typical endothelioma. Cut another part of the tumour and look at it and you see it is typical chorion-epithelioma. If you look at this part of the tumour with the description of deciduoma malignum of the uterus in your mind you will see precisely the same sort of structure. So you have in the testis a series of tumours which on microscopical examination show in one part cartilage, in another part cysts, in another part squamous epithelium resembling skin, and even epithelial pearls, as in squamous-celled carcinoma. In another part there is a columnar-celled carcinoma, in another sarcoma, in another endothelioma, and, lastly, chorion-epithelioma. It depends on which part of the growth you cut as to which structure you come upon, so that we can only get an opinion of these tumours by cutting sections from all parts. All the tumours show the cystic arrangement, but the number of cysts varies enormously.

What shall we call these tumours? From a pathological point of view there is only one way in which one can look at these growths, and that is that they arise in embryonic tissue occurring in the testes. Here are a series of tumours containing cartilage, bone, sarcomatous tissue, carcinomatous tissue, etc., closely resembling the teratoid tumours of the ovary, and one can imagine these tumours arising in a sex-cell developing irregularly under some abnormal stimulus. It is a mistake to label such growths carcinoma, sarcoma, endothelioma, or chorion-epithelioma; they are really tumours containing all these elements, and arise in cells which are capable of growing every kind of tissue. Let me put this view in a diagrammatic way. Take a sex-cell, an ovum developing under normal stimulation. One starts off with the idea that you have an ovum splitting into two. Both of those portions develop, and there results a twin birth. Two portions of ovum or two ova have been fertilised at the same time, and the mother gives birth to twins. But supposing one twin develops well and the other twin is overwhelmed *in utero*, in that case you have born a living child and a mass of legs and arms and internal organs, an amorphous mass, which goes by the name of an acardiac monster. Go further and you get a living child with, attached to it, part of another child. You will remember the Hindoo boy whom Mr. Keith brought here, a normally formed boy, æt. 18 years, with a second individual attached to his chest, with legs, penis, etc., obviously his twin brother, an autosite and a parasite. This parasite can, of course, suffer from tumour formation, innocent or malignant. Go further still and you get another class of case, in which the parasite is fixed to the autosite as an amorphous mass, generally in the sacro-coccygeal region. In these sacro-coccygeal tumours you will find squamous epithelium, columnar epithelium, nervous tissue, bone, and cartilage. You can go one step further still and put one being inside the other, so that it is hidden. It is

now termed a "teratoma," a "*fœtus in fœtu.*" These teratomata have been found in various parts of the body, are capable of undergoing malignant change, and not only do they contain the elements of a second individual, but they contain the elements of the fœtal structure, the chorion as well, and from this trophoblast tumours may arise. Therefore when these structures undergo malignant change, one is not surprised to find chorion-epithelioma present, which is malignant disease arising in the trophoblast. If you look on these tumours in this way you can understand why there are so many diverse structures in them.

The secondary growths which occur in connection with these tumours vary enormously. The secondary growths of this specimen were practically chorion-epitheliomata. It took only eight weeks to kill the patient, from rapid metastases in all parts of the body. In this one the secondary growths were mainly cartilage and carcinoma. In another one the secondaries were practically all sarcomata.

These growths therefore appear to arise in a new individual, growing inside the testicle or some other part of the body, and cells of this individual undergo malignant change. The secondary growths reproduce the part of the tumour which is the most malignant. That is the explanation I believe to be true of these various forms of tumours occurring in the testes.

There is another point which I should like to mention. Why should these tumours occur so frequently in the testis and the ovary and so seldom elsewhere? Many explanations have been given. Looked at superficially you would expect that these tumours would occur in the sex-cells, and perhaps they occur in undifferentiated sex-cells which are neither male nor female, and which develop by a sort of parthenogenesis. In the case of the ovum the stimulus to growth is the spermatozoon, and when it receives that stimulus it starts generating. If there is some other stimulus, or if two cells are fertilised and one remains quiescent, one can imagine that ovum starting to develop in an abnormal and irregular way. It starts off forming a condition like these tumours, cartilage, bone, and other structures mixed up together irregularly, and with a tendency, like all degenerating and abnormal tissue, to become malignant. This view explains to a certain extent some curious forms of tumour occurring in the ovary, and perhaps similar conditions occur in the testes; but what I want to impress upon you, from the clinical point of view, is that it makes no difference what pathological view you take with regard to these tumours, because these cases cannot be diagnosed before removal, and before being microscoped, and it makes no difference in your treatment either, because they are all malignant.

October 10th, 1910.

NOTES FROM A FOREIGN SPA.

By JOHN H. BARNARD, M.D.
(Of Contrexéville).

THE return of the "old stager" to his favourite spa year after year is sometimes the result of the force of habit; more frequently it is obligatory. This suggests the query: Are prophylactic measures, especially those of a dietetic order, sufficiently inculcated by the family physician and by those whose lot it is to practise at hydro-mineral resorts such as Contrexéville? Or are deviations from the moderation that should govern the public appetite so ingrained that dietetic rules remain a dead letter? (I assume that we are all agreed that errors of diet are responsible for most of the ailments that bring patients to spas.) Both implied indictments are, I fear, well founded. In the one case spa and other practitioners are inclined to attribute to mineral springs more far-reaching curative virtues than can reasonably be expected of them. The justice of this criticism is, I think, borne out by the small space allotted in their writings to regimen, and more particularly to its importance in the intervals of the "cures." As for the public, the pleasures of the table, and habitual strayings from the paths of physiological righteousness, are, as we know but too well, powerful rivals of such prudential considerations. Modern cooking, with its alluring refinements, tends to make *gourmets*, and, what is infinitely worse, *gourmands* of us all. Colour and fantastic form in dress are well matched by the varied and tempting flavours imparted, nowadays, to cooked food. The mischief is further aggravated by the fact that the hurry-scurry of twentieth century life renders us precipitate and preoccupied feeders.

Let us contemplate the crowd of *buveurs*—study their *physique*, and their habits. Middle-aged men and women compose the bulk of them. This age-factor lends additional force to my contention that the "cure," however efficacious in its immediate effects, is an incomplete therapeutic resource, unless it be supplemented, during the rest of the year, by dietetic restraints.

Viewed in profile, the typical *buveur* is a sad sight, both æsthetically and physiologically. Hyper-alimentation and physical indolence—a want of

balance between income and expenditure—are reflected in the rotundity of his outlines. Enteroptosis and unwieldiness are not infrequently his most striking characteristics. These, with the easily aroused dyspnœa of hypertension, make up a picture of preventable, premature senility that is discouraging to the conscientious practitioner, and which, moreover, does not increase one's respect for human intelligence. See him at table, and the gusto with which he "tucks in" affords an easy explanation of his early physical decadence. I am told by a French brother-practitioner that this picture of the *bon-vivant* practices of too many of the *habitués* of this resort, at any rate, is by no means highly coloured. In fairness to our own compatriots, I should say that the foregoing description applies more particularly to other nationalities. But it cannot be denied that they also are too prone to cling to old-established dietetic errors, to the detriment of their health. I may here aptly recall the fact that the average annual consumption per head of butcher's meat in England is 59 kilog. as against 39 kilog. in France (Professor Armand Gautier). If the Briton be outnumbered at Contrexéville by his foreign fellow-sufferers, it is not because he has less need of the "cure," but because he is not as yet so imbued with the efficacy of the *Pavillon* spring as the Frenchman.

If, then, it be idle to expect our patients, under present conditions, to safeguard themselves, by the maintenance of a due equilibrium between intake and expenditure of energy, against a return of the troubles that bring them periodically to this "renal spa," as Dr. Leonard Williams has happily christened it, does it not behove spa and family practitioners to be more insistent on the observance of prophylactic precautions? Should we not do well to copy more than is our wont the blunt outspokenness of Abernethy?

Do we examine the "mill" as often as is desirable? This season, a gouty patient of mine—a cultured clergyman—arrived here wearing only the upper front part of his plate, the masticatory or molar portion having been left lying cosily at his vicarage in England. It was as if he had brought his surplice to church, but had forgotten his theology. Do we interest ourselves in the care of these indispensable "grinders"? Do we insist on adequate mastication, or have Van Someren and the oft-quoted Gladstone preached in the desert? We are all familiar with the evils induced by "bolting" our food, and we also know that most of us have been "bolters" from our youth up. Imperfectly developed jaws, overcrowded teeth, adenoid growths, deformed nasal passages, dyspepsia, atrophied salivary glands, and, what is more appropriate to the subject of this paper, over-eating—all these consequences are too common to be ignored. Deficient mastication points the way but too surely, later in life, to foreign and native spas. The invasion of French cookery is, *pace* the progressive (?) section of the English community, not an unmixed blessing to our country. Exaggerated sapidity is a mistake, and leads undoubtedly to the ingestion of more food than our tissues require. Only the other day an eminent Paris physician was advocating for the benefit of his fellow-countrymen a no less startling revolution in French family life than the discarding of native in favour of English cookery.

French physicians are expected to supply almost every patient they see with a detailed and written dietary. This practice might, perhaps, be more frequently imitated with advantage in our own country. But even our Gallic brethren do not insist enough on a quantitative restriction of the daily food. This is, I contend, of even greater importance than qualitative limitations. What more common than to see a patient, who, while carefully avoiding certain dishes, compensates himself for these deprivations by consuming more than is good for him of items not included in the "forbidden" list? And what an uncertain quantity is this "forbidden" list! This is what Dr. Harry Campbell, in his admirable work 'On Treatment,' says concerning its variability: "Let a patient consult half a dozen physicians in succession regarding his diet, and the chances are ten to one that no two will be agreed." One is constantly told by a gouty patient that his doctor strictly forbade beef and mutton, while allowing white meats. This is the comment Dr. Marcel Labbé makes on this subject ('Régimes Alimentaires, 1910): "On ne saurait plus dire aujourd'hui que les viandes rouges sont interdites tandis que les viandes blanches sont permises, les unes et les autres donnant des proportions équivalentes de purines et d'acide urique." The same ban is laid on eggs by many consultants. Le Gendre and Martinet in 'Régimes Usuels,' 1910, quoting Fauvel ('Physiologie de l'Acide Urique') say: "Le lait, le fromage . . . les

œufs . . . offrent des ressources variées pour composer un régime sans purines." Walker Hall confirms this. The necessity of a purin-free dietary is generally admitted in uric acid lithiasis, and I always prescribe it to patients suffering from the invasion of the gout toxin. Dr. Campbell (*loc. cit.*) differs on this latter point, but speaks with no uncertain voice on the desirability of curtailing the intake: "With them (the gouty) it is quantity rather than quality that they have to be careful about." Again, "we all know how greatly we can benefit our gouty and plethoric patients by reducing their food." "I am certain that many lives can be saved by a judicious restriction of the diet, even to the point of temporarily withholding it altogether . . . not a few being able to maintain health and strength on so small a quantity as 10 oz. a day. And note . . . that the more thoroughly food is masticated the less the quantity needed. . . . On this well-masticated, reduced diet there is a general levelling up in bodily and mental vigour."

Dietetics is a science that, so far as my knowledge goes, is not taught at any British medical school. It is, consequently, a *terra incognita* to most practitioners. A knowledge of this neglected branch of therapeutics may safely be said to be of greater usefulness than proficiency in prescribing drugs. Dr. Campbell (*loc. cit.*) says: "It is imperative that the physician realise how large a part the establishment of normal digestion plays in treatment. Not until he does so will he be able to secure a full measure of therapeutic success. For this reason, a study of dietetics . . . should occupy a prominent part in the student's curriculum, and throughout the whole of his subsequent career be kept constantly in view." There should be a Chair of Dietetics at each school, and the teaching of this all-important subject should be as practical as possible. The health of the community depends on the well-being of the cells of which the body is composed; this depends on the composition of the tissue-plasma in which these cells dwell, and this, in turn, is largely dependent on the food we eat and how we eat it. "Every disease is mirrored in the plasma" (Dr. Campbell). It is, I think, highly probable that the undoubted benefit derived from a course of treatment in renal and biliary lithiasis, gout, cystitis, and diabetes at such a *station* as Contrexéville is due, not so much to a scavenging of the urinary system, as to a purification—a normalising—of the cell atmosphere the plasma. This enables the hitherto poisoned and ill-nourished cells to resume, under normal conditions, the manufacture of that marvellous therapeutic armamentarium (hormones, enzymes, etc.), the dispensing of which by Dame Nature—ever our best teacher—provides for the harmonious working of the varied machinery found in the human organism.

I have laid stress on the predominant part played by a badly balanced and, especially, an excessive dietary, because I am convinced of the prominence of this ætiological factor in the genesis of the ailments we are called upon to treat at this and similar resorts. Some writer has said that what is wanted in the England of to-day is a wave of puritanism. This is, of course, not to be taken in a religious sense, but as indicating the desirability of a reaction in the direction of primitive simplicity. To no phase of modern life does this apply more justly than to our diet. It is for medical men to effect this reform by enlightening the public on the relative value of different foodstuffs, by urging the necessity of daily exercise, and of keeping the consumption of such a source of proteid as butcher's meat within proper physiological limits, and, above all, by discouraging, amongst the leisured classes, the excessive ingestion of the mixed diet which experience has shown to be the best suited to mankind. In this direction only lies the prospect of immunity from the infirmities that too often embitter the later years of those of our fellow-beings who apply to us in ever-increasing numbers for relief. The task is a tough one, for its successful accomplishment means nothing less than the triumph of common sense over stereotyped habits and the promptings of that ever-present tempter, the palate.

With regard to the Contrexéville "cure," it is a wise precaution in mild cases to make at least two consecutive annual visits. I am persuaded that, even in severe cases, three consecutive "cures" should, as a rule, suffice, provided the principles advocated in this paper be put into practice, and that the drinking of the *Pavillon* water be continued regularly at home.

I am also in the habit of prescribing for two, or, at least, one day in the week, an exclusive V.E.M. (vegetables, eggs, milk) diet, believing it to be a most valuable measure, not only for our Contrexéville patients, but for all middle-aged people.

October 10th, 1910.

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THE LESSONS OF A SESSION.

By H. MACNAUGHTON JONES, M.D.,
President of the Obstetrical and Gynæcological Section of the Royal Society of Medicine.

IT may be well at the close of the year to review the work which has been done during the session, and to ask how we have added to our experience, and what permanent and practical results are likely to follow from the meetings and discussions that have taken place.

Quinquennial Summary of Transactions.

There is to me always a certain atmosphere of sadness surrounding the volumes of transactions of a medical society. Every now and then some investigator interested in unearthing a record, pathological or clinical, which he requires to illustrate his view or enforce an argument, ransacks the library shelf in search of the reference he requires. Sometimes years elapse before this record is dragged to light, and the work of research, which has taken great pains and much time to complete, is again brought to notice. It has remained immured in the catacombs of transactions, awaiting some happy necessity of an explorer, who, in his desire to embellish a theory or amplify some fact, seizes on it and utilises it, giving it the tardy recognition of its originality and worth.

It may be said that this is just the use of transactions—to serve as records for future reference. This is true, yet I cannot help feeling that it would be a great gain to our department if every five years a digest of the 'Transactions' appeared, in which the various subjects dealt with would be carefully and systematically analysed and codified, with a brief epitome of each author's deductions appended. A Section such as this would surely provide an editorial staff willing to carry this out, and I have no doubt a publisher would be found ready to take the risk of publication and issue the

small volume at a moderate cost. In it the original communications and the discussions on these would be summarised, and the specimens classified and the remarks on them epitomised.

Functions and Aims of the Section.

It is my desire to-night to illustrate my meaning in miniature by summarising the work of one session, and classifying it under the three heads of "communications," "clinical cases," and "specimens."

The communication, be it in the form of a pathological specimen, a clinical history, or an original thesis, which leaves no practical lesson in its track, opens up no new field for exploration, or does not assist in solving some unravelled mystery, is not worth the time taken up in listening to it; but it is obvious that there must be degrees of importance in each of these modes of communication. We might take them in this order: (*a*) A new theory (provided it be not a "maggot in the brain" of the advancer) based on original observation, previously unnoticed or rarely observed pathological changes and their initial ætiological relations; (*b*) unrecorded or unusual clinical experiences, bearing on treatment and the action of certain therapeutical remedies; (*c*) confirmatory evidence touching some doubtful or disputed point in operative procedure. In short, some distinct gain in progress, either in the substantiation of previously advanced views requiring confirmation, or the demolition of those which do not stand the test of time, and further investigation. What there is not room for is the study of generally accepted ancient history.

There is, however, another most important aspect of a Society such as this. Representing as it does all the divisions of this great United Kingdom, it ought to be the accepted source of reference and a tribunal of appeal in this country for deciding those critical questions which arise in the everyday practice of obstetrics and gynæcology. Opinions are fairly agreed on the great majority of these. Some are yet *sub judice*. It is in the handling of these latter that so great a responsibility rests. That which may be safe in procedure or practice in the hands of some operators is absolutely unsafe and dangerous in those of others. We can also do much to combat the regrettable tendency to look too lightly on the incipient stages of disease in the female genitalia. Resort to operation has become so usual that it is always present to the mind as a ready and final means of escape from error, doubt, difficulty, or danger.

Therefore, there are two directions in which the Section can influence the general practice of this branch of medicine. One, which demands the utmost caution in its following, is the recommendation for general application of new modes of practice, the carrying out of which by inexperienced hands is fraught with possibly greater immediate danger to life than is the condition for which they are undertaken. The other has rather to do with early and correct diagnosis. It calls for the enforcement, through repeated emphatic utterances and convincing proofs, of the grave responsibility that attaches itself to temporising and masterly inactivity. For these reasons it is well that, in addition to those aims I have referred to, the Section should keep before it the position it occupies as a recognised guide when the practitioner arrives at one of those cross-roads in practice where the turning he takes means life or death to the patient. Communications and discussions are thus occasionally justified that may seem to some unprofitable, yet which have lessons of supreme value to others who have had less opportunity and experience.

COMMUNICATIONS.

Electro-therapy in Gynæcology.

Following the division I have made, and taking the communications in chronological order, the first paper on the list is that of Dr. Curtis Webb, on "*Points for the Use of, and Indications for, Electro-therapy in some Gynæcological Affections.*" Considering the action of the direct current under three heads, chemical polar, ionic, and interpolar, he dealt mainly with the two former. Incidentally he bore testimony to the sedative action of the rapidly interrupted induced current in neuralgic affections of the pelvic organs, as also of static electricity in dysmenorrhœa, and dwelt on the influence upon nutrition of the chemical interchanges that occur throughout the circuit, and the circulatory effects that result from vaso-motor stimulation and the contraction produced in unstriated muscular tissue. Summarising the effects of the faradic, constant and static currents, he

divided the two first of these into: (*a*) sedative, or stimulation contraction; (*b*) (i) cauterising; (ii) congestive or liquefying; (iii) ionisation; and (iv) metabolic. Treatment by the static current, the physiological effects of which were summarised, was not entered into.

Dr. Curtis Webb was rather optimistic in his review of the results of electro-therapeutic treatment by ionisation of inflammatory affections of the vagina, disorders of menstruation, and some diseases of the uterus and adnexa. Certainly he adduced sufficient evidence to prove that in ionisation we have an efficient therapeutic agent in cases specially selected for its employment. His paper, however, and the discussion which followed it, tended to show that such electropathic treatment demands primarily three essential conditions for its sole application: (1) Special training and skill in the application of the kind of electricity used, as regards its nature and quantity; (2) accurate diagnosis and differentiation of the states in which it is contra-indicated; (3) reliable judgment as to its continuance or abandonment, according to the effect it produces.

The associated danger appears to be twofold. First, the temptation to postpone more radical measures in those cases in which time is an all-important element for success; secondly, that it is liable to be used for conditions for which physically and pathologically it is not merely unsuitable, but dangerous.

Leucoplakic Vulvitis.

Dr. Comyns Berkeley and Mr. Victor Bonney made one of the most interesting and original contributions to our year's work on "*Leucoplakic Vulvitis.*" Here the different stages in the pathological processes were beautifully illustrated by micro-photographs, which have been reproduced in the 'Transactions,' and the differences in the microscopical appearances present in kraurosis vulvæ were demonstrated, and the two diseases were thus shown to be pathologically distinct. An important diagnostic difference in their symptomatology was referred to, viz.: the occurrence of pruritus in leucoplakia and its absence in kraurosis, while in the latter affection dyspareunia is more usually present. This does not altogether coincide with my own experience, though I may have in the past confused the two conditions. One most important point in the communication was the inquiry into *the relationship between leucoplakic vulvitis and carcinoma of the vulva.* The authors stated that "in any case of carcinoma of the vulva operated upon by them, leucoplakic vulvitis had always been present," while they knew of "no instance where kraurosis had been associated with malignancy." Here, again, we require a larger experience to decide the accuracy of the conclusion, for many authorities do regard kraurosis (in the sense in which the authors apply this term) as having a correlation to epitheliomatous change. For example, at the same meeting Dr. Arthur Giles narrated the particulars of an advanced case of kraurosis of the vulva, in which there was a malignant growth at the one side of the vulva, and he stated that he had seen malignancy follow in one previous instance. I have been so far fortunate in not having in any case of kraurosis I have treated seen malignancy follow. The malignancy, according to the authors, is most likely to occur in the second stage of the leucoplakia, when there is excess of cellularity and rarefaction of the sub-epithelial tissues. A succinct review of the pathology of carcinoma of the vulva followed, and it would appear that coitus, advanced life, and sterility in younger women have a distinct ætiological relation to the two affections. So in my experience have protracted vaginal purulent discharges, gonorrhœal or other.

In view of the possibility of this malignant change, occurring or of its recurrence, the landmarks for removal should lie completely outside the diseased area. In severe cases, before resort to extirpation where other remedies have failed, recent experience would encourage the use of the Röntgen ray and radium treatment.

Operative Procedures in Contracted Pelvis.

There have been three very important original obstetrical communications made during the session. I propose to group these together. The first was a remarkable paper by Dr. Hastings Tweedy, in which some rather novel propositions were ably advanced in discussing "*Modern Methods of Delivery in Contracted Pelvis.*" The paper practically dealt with the relative indications for Cæsarian section, symphysiotomy, pubiotomy, and hysterotomy. I may group the principal conclusions he offered for discussion: (1) Pelvimetry by Skutsch's pelvimeter gives the most accurate pelvic measurements. The size of the fœtal head may be disregarded; Muller's manœuvre (pressing the head into the pelvis in deep anæsthesia) often fails in its purpose. (2) Induction of premature labour is never required, nor is prophylactic turning justifiable when employed as a measure to aid delivery. These procedures with those of high forceps and perforation are to be looked upon as obsolete, and in a few years will be viewed with abhorrence. (3) Cæsarian section is indicated in a pelvis with a conjugate between three and a quarter and two and three quarters in a healthy patient with

suitable surroundings and early in labour. Also when the child is living in a pelvic conjugate of two and three quarters to two and a quarter. (4) If the membranes have ruptured for some time in a conjugate above two and a quarter, and the child be living, symphysiotomy and pubiotomy are preferred. (5) These operations are also indicated when danger symptoms to mother or child arise during labour in a pelvis not below two and three quarters in a true conjugate. (6) In a true conjugate of less than two and three quarters when labour is advanced with ruptured membranes, and Bandl's ring is well up towards the umbilicus, Cæsarian section is unsuitable, and hysterotomy is the operation of selection. (7) Subcutaneous pubiotomy, the steps of which were fully described (not Döderlein's operation), is so safe in its technique that it may be performed by the general practitioner. (8) Perforation should never be performed in a case of contracted pelvis on a living child. (9) In the lesser degrees of contraction Wolcher's position should be availed of.

The hysterotomy described and recommended by Dr. Tweedy is an extra-peritoneal Cæsarian section, the primary abdominal incisions being transverse, from one anterior superior spine to the other, about one inch above the pubis, neither the peritoneal cavity nor the cellular tissue being exposed. He does not regard the presence of sepsis as a contra-indication.

As might be expected, these views gave rise to an animated discussion, in which several of the obstetric stalwarts in London, including Sir Francis Champneys, and Drs. Hermann, Herbert Spencer, Eden and Griffith took part. Such names show the weight of evidence brought to bear on the contested points. I may summarise the lessons to be drawn from the paper and the discussion:

(1) Greater care generally should be taken in the early months of pregnancy to estimate approximately the pelvic capacity for labour at full term. In lying-in hospitals and maternities Scutsch's pelvimeter may well be employed, but for general purposes in ordinary practice the hand, properly used, is the most ready and reliable guide.

(2) The time has come when the general indications for Cæsarian section, symphysiotomy, pubiotomy, hysterotomy, and the ordinary obstetric methods of dealing with contracted pelvis can be more exactly defined.

(3) There is still a useful place for induction of labour, craniotomy or cephalotripsy, and high forceps application. While ever regardful of the life of the child, that of the mother takes precedence in determining our selection of the manner of delivery.

In connection with this discussion I may refer to the master's report of the Rotunda Hospital, recently issued. Dr. Tweedy certainly shows that the obstetric reputation of the hospital is worthily upheld.

Between the intern and extern maternities there were 4173 deliveries, not including 203 abortions. Of the 1963 intern cases, there were 4 deaths, and of the 2200 extern, 8 deaths. But of the 4 intern cases, one died in the last stage of phthisis pulmonaris, one was admitted with septic peritonitis and rupture of the uterus, with the fœtus halfway through the os. The third was a case of hydramnios, the anencephalous fœtus being dead on admission. The fourth appears to have been a death after spontaneous rupture of the membranes in hydramnios.

Of the deaths in the extern maternity, two had died before the physician arrived; none of the others died in labour. No death has occurred in the last twenty-six cases of true eclampsia. Only two operations for Cæsarian section were performed. There were eight cases of placenta prævia, and no death.

Considerable stress in the report is laid on the importance of the careful differentiation of true eclampsia from other types of convulsion. No case in which there is a history of previous fits unconnected with pregnancy or labour, nor any which is free from albuminuria, are included. If asphyxia threatens, the plan of turning the patient on to her chest, with the head hanging over the side of the bed, the manœuvre being followed by artificial respiration and administration of oxygen, is advocated.

Cæsarian Section in Placenta Prævia.

The next communication is also from the pen of a foremost representative of the Dublin School. Dr. Jellett dealt with the all-important question, "*Will Cæsarian Section Lessen the Mortality of Placenta Prævia for the Mother or the Child?*" Speaking from the statistics of the Rotunda Hospital for twenty years and the confinements of 32,547 women, he showed that out of this number 138 had placenta prævia, the percentage mortality of the latter being 3·6, closely corresponding to the 3·1 mortality of Fry ('Amer. Gyn. Soc.'). I collected statistics from the clinics of Professors Mangiagalli, Pinard, Bumm, and Dmitri de Ott, which, taken together, include a period of thirty-six years of experience in some of the largest obstetric establishments in Europe. Adding the twenty years of the Rotunda Hospital to these we find that in fifty-six years there were 666 cases of placenta prævia. The total mortality of 611 cases of placenta prævia occurring in four of these clinics did not amount to 3 per cent. One Cæsarian section (followed by death) and one Porro's operation were performed in these 611 cases. In the fifth clinic, Professor Bumm's fifteen Cæsarian sections were performed in 179 cases, with a mortality of 6 per cent., the total mortality being 2·5 per cent. I do not here include Professor Krönig's statistics, in which 50 per cent. of the forty cases of placenta prævia had

Cæsarian section performed without any death, as I do not know the total number of deliveries or over what period of time the report spread. In the other twenty of his cases there was one death. Up to the time that Dr. Jellett's paper was read there is a record of one case performed in Scotland (that of Dr. Munro Kerr), and the only cases referred to in the discussion as occurring in England were those of Dr. Griffith and Dr. Gow. In Dr. Munro Kerr's case, which he brought before the Section, the placental presentation was central and was complicated by the presence of fibroids, and in that of Dr. Gow the presentation was also central and the cervix undilatable.

The lessons that must be drawn from the discussion on this communication are that Cæsarian section has a place in the management of placenta prævia when (*a*) this complication occurs under circumstances that would in any case demand the operation; (*b*) when the character of the presentation and form of the attachment, with other conditions, render the classical methods of treatment impossible to carry out, or their practice more dangerous than operation. It would also appear that the risk from placenta prævia to the mother, which must be our first consideration, has been much exaggerated by those who advocate the step.

I have not referred to the relative influence of the operation on the child, as I consider the records bearing on this appear to be very variable and uncertain.

Cæsarian Section in Eclampsia.

The third communication in this group is that of Dr. Frederick McCann, on "*Cæsarian Section in Eclampsia Gravidarum.*" He was instigated to bring the subject forward by the successful issue of a case in which he performed the operation where there was no possibility of emptying the uterus, and when a fatal issue seemed imminent. The statistics quoted of previous operations were certainly not encouraging. Sir Halliday Croom appears to have been the only obstetrician who had performed the operation for eclampsia in the United Kingdom. In his two cases the operation was done under desperate conditions; the mothers died, one three days after parturition. One child was saved.

Dr. McCann suggested that the indications for the operation as a means of rapid delivery included (1) the severity and rapidity of the fits; (2) the time of their occurrence, before labour or after its commencement; (3) if there be considerable disproportion between the head and pelvis; (4) an undilatable cervix; (5) a moribund mother and a living child. Bumm's exceptionally favourable results from Cæsarian section (reduction of mortality from 25 and 30—surely a very high rate—to 2–3 per cent.) were quoted in support of the operation. The discussion virtually turned on these points—(*a*) the advisability of rapid emptying of the uterus; (*b*) Does Cæsarian section, under certain conditions, lower the present mortality? (*c*) The relative advantages of the abdominal and vaginal routes. With regard to the first and second of these questions, opinion was divided, but the leaning was rather to regard the rapid emptying of the uterus, though often indicated, as by no means universally desirable. Still, that in given cases, as that of Dr. McCann's, it was justifiable to avail of Cæsarian section.

An unbiassed verdict would, I think, be that Cæsarian section has a place in the treatment of eclampsia in exceptional cases, and these are all included in the indications laid down by the author of the paper.

Two remarks I might here make: I do not consider that Bossi's dilator, properly applied, is the useless instrument that some who have never used it conceive it to be. Also, I believe that in a special type of case, particularly if a post-partum one, pilocarpine, judiciously administered, is a most useful agent. Certainly I have directly saved life by it on at least two different occasions. Possibly tyramine and infundibulin may now have a place as a means of lowering the blood-pressure.

Dr. Amand Routh, at the recent Congress at St. Petersburg, brought forward the whole subject of Cæsarian section, summarising every obstetrical consideration for this step, and illustrating his paper by the statistics of over 1282 cases performed by living operators in the United Kingdom since 1890, 1632 of these being for contracted pelvis. From the details of 482 of these, he concludes that Cæsarian section should never be done if previous attempts to deliver have been made before the patient comes under notice. If the membranes have ruptured and examinations have been conducted special precautions should be taken before operation.

He does not favour the extra-peritoneal operation. Of 1254 cases operated upon since 1890, the total mortality has been 11·6 per cent., but from the statistics of the last five years he places the present total mortality from Cæsarian section at 8·1 per cent., though in favourable cases for contracted pelvis it is only 2·3 per cent. Improved methods of technique, added to early recognition of indications, have reduced the mortality so as to bring the step much more frequently within the range of justifiable obstetric procedures in parturition. His conclusions with regard to the performance of the operation in placenta prævia are practically those arrived at in the discussion on Dr. Jellett's paper, and virtually, that resort to this step in eclampsia is only justifiable on the grounds advanced by Dr. McCann.

Professor Bumm, at the congress, summarised the relative advantages and disadvantages of the supra-symphysic and classical operation. In the former, the avoidance of the peritoneum and the

easier suture of the cervix; on the other hand, greater difficulty of performance, greater risk to the bladder, the delivery of the fœtus more difficult, and certain disadvantages in the cicatrix leading to displacement of the uterus, and rendering a second operation more difficult. Pestalozza considers the classical Cæsarian section preferable when asepsis is certain, and early in labour. When there are septic complications he advocates embryotomy, and in extreme degrees of contraction, Porro's operation. He adopts Fritch's transverse incision of the fundus in the classical operation, and in the extra-peritoneal, Latzko-Döderlein's technique. He deprecates resort to the operation in eclampsia, and only very rarely considers its performance justifiable in placenta prævia. Selheim prefers the transverse incision of Pfannensteil when haste is not called for, though he recognises its greater difficulty in technique. He is more conservative in his views, believing that with improved management of labour resort to operation will be less frequent, and that if infection is prevented, no operation is warrantable to save the life of the child that is dangerous to the mother.

Mrs. Scharlieb brought before the Section an analysis of 150 cases of ovarian tumour, out of which 25 exhibited various types of malignancy. Altogether, including returns sent to myself by Professors Schauta, Jacobs, Winter, Schotländer, and Mangiagalli, *the proportion of malignant tumours of the ovary* was shown to be 350 out of 3481—that is, 9·9 per cent. Though it is not possible to say exactly, still there can be no doubt that a fairly large proportion of such malignant growths were primary, many, however, being metastatic. The discussion made it clear that much has yet to be done regarding the ætiology and differentiation of malignant tumours of the ovary and their classification.

The Life-history of Uterine Fibro-myomata.

Dr. Arthur Wallace made an exhaustive analysis (*vide* 'Transactions,' 1910, p. 208) dealing with the signs and symptoms, local and constitutional, in ten women in whom disappearances or shrinking of the tumour had occurred to a remarkable extent. These clinical histories only served to convince Dr. Wallace "that palliative treatment was inapplicable when fibroids caused symptoms, and the condition of the general health justified operation." It is notorious that some women enjoy fair health and suffer little from a myomatous tumour; others eke out an existence for years, and live a life of chronic invalidism from various causes. But with the great majority life ends prematurely in pain and misery from some of the many complications that attend on the growth. The myth of a curative menopause has disappeared, and the statistics of the authorities quoted by me in the discussion, including reference to the records of the results of examination of some 8000 myomatous tumours, prove that danger to life is increased directly in proportion to the advance in years after from forty to fifty.

Ruptured Tubal Pregnancy.

Mr. Carwardine's epidiascope demonstration of micro-photographs from a case of ruptured tubal pregnancy beautifully illustrated the cell processes of the syncytium and the diversity in the character of the epithelial covering of the villi. An interesting point brought out was that pathological appearances seen in the nuclei arising from the syncytium had a significant resemblance to the correlated structures seen in chorio-epithelioma.

Complications of the Puerperium.

Sir Japp Sinclair's paper had its use for those in general practice who, either through ignorance or carelessness, are inclined to overlook or treat lightly some of the common post-partum complications of pregnancy or the abnormal uterine and pelvic conditions which are present during the puerperium. He took rather a pessimistic view of the advance of modern obstetrics. If Ould, Chamberlain, or "big paw" Smellie were appointed visitors to the Rotunda, Queen Charlotte's, or the Glasgow Maternity by a celestial medical council, I fancy they would report that the art of the eighteenth century had not stood still, but had been elevated into a science. They might even console the later-day sojourners for the loss of their practice in midwifery by telling them that, in this mournful star, the labours of Braxton Hicks, Barnes, Simpson, Hamilton, Duncan, Denman, Beatty, McClintock, and others were still bearing fruit.

His remarks touching the capacity of obstetric nurses elicited from Sir Francis Champneys a notable defence of the modern registered midwife. He showed statistically that since the Midwives Act had come into operation there had been a great reduction of maternal and infant mortality.

As inspectors I should like to emphasise one point arising out of Sir Japp Sinclair's paper, viz. that we should clearly distinguish between those cases in which ventro-suspension of the uterus is indicated rather than ventro-fixation. The latter method should in my opinion never, save under exceptional circumstances, be adopted during the child-bearing period of life.

CLINICAL CASES.

We turn now to the clinical histories which were brought before the Fellows. Dr. Herbert Paterson's case of "*Rupture of the Uterus, Treated by Suture,*" applied *per vaginam* with drainage, was instructive. It proved the possibility of successful treatment of the utero-vaginal rent as an alternative to hysterectomy, and the value of proctoclysis, which was continued for sixty hours after the operation.

Dr. Philip Turner's "*Hysterectomy Performed Sixty-three Hours after Labour*" was very interesting. The justification of the decision not to interfere, after an exploratory incision had been made to determine the nature of the growth at the end of the second month, was established, and the correctness of the prompt performance of total hysterectomy, when the signs and symptoms of sepsis arose after labour, was clear. Here it is important to note that the infection was due to necrosis of the subperitoneal fibroid.

In conection with this case reference must be made to that of Dr. Russell Andrews, of "*Myomectomy for an Intra-mural Fibroid in the Third Month of Pregnancy.*" Here the enlarged retroverted uterus could not be raised out of the pelvis. Pregnancy went to full term, and a living child was born.

A perfect example of obstetric surgery was afforded in the successful dual issue of Dr. Herbert Spencer's "*Posterior Cæsarian Section, followed by Hysterectomy for Fibroids.*" The operation was performed in the thirty-eighth week, and the 7 lb. living fœtus with the placenta having been extracted, the degenerated and greatly thickened uterus was removed.

In Dr. Griffiths' case the large myomatous tumour which grew from the posterior wall of the uterus, and distended the vagina, was removed with safety to mother and child during labour at full term.

Hæmatocele.

Dr. William Alexander showed how an exceptionally large hæmatocele might complicate the presence of a uterine fibroid without any traumatism having occurred. The extra-peritoneal collection of blood in this case was enormous. It had occurred before operation, and its source was uncertain. Aseptic precautions, with drainage after removal of the uterus, resulted in cure.

A very important communication was that of Dr. Cuthbert Lockyer's on "*Pelvic Hæmatocele.*" Two cases of Mr. Stanley Boyd's and one of Dr. Amand Routh's served to illustrate the occurrence of an ovarian hæmatocele, apart from ectopic gestation. The liability to mistake lutein hæmatoceles for ovarian gestation is obvious, and this error occurred in Dr. Routh's case, and in one recorded by Dr. Giles. It is clinically interesting to note that the symptoms in both of Mr. Stanley Boyd's patients closely simulated those of appendicitis.

Specimens.

In alluding to the various specimens exhibited we may divide them under the heads of *uterine*, *ovarian*, *tubal*, and *vaginal*. We may thus summarise those which more especially had a pathological importance.

In the necrobiotic fibroid incarcerated in the pelvis that complicated pregnancy Dr. Stevens showed that there was extensive thrombosis of the veins of the capsule, hysterectomy having been successfully performed by Dr. Eden.

Dr. Lewer's specimen of "*Cystic Tumour of the Right Broad Ligament*" springing from the uterus, and apparently developed from Gärtner's duct, was believed by Mr. Doran to be homologous with Uterberger's tumour ('Monats. f. Geb.,' May, 1909), which was undoubtedly of Gärtnerian origin.

Complete rupture transversely of the fundus uteri is rare. In the uterus shown by Dr. Lewers there was no degeneration, and nothing to account for the transverse rupture, unless, as was surmised by Dr. Maxwell, it might be explained by a rather unusual arrangement of the muscle-fibres parallel to the place of rupture.

Mr. Targett's specimen of "*Keratoid Degeneration associated with Columnar-celled Carcinoma*" of the uterus was of pathological interest, as was the other specimen he submitted as exhibiting the very rare occurrence of malignancy in a uterine adenomatous fibromatous polypus. Though this was not altogether substantiated by the Pathological Committee it was at least on the borderland of malignancy.

I am indebted to Professor Shottländer for drawings of sections of a cervical polypus in which the malignant change was sarcomatous. The rare occurrence of *carcinoma in complete prolapse* of old standing was illustrated by the discussion on Dr. Russell Andrews' case, in which there was also a contact carcinoma of one labium. The unusual occurrence of a *spheroidal-celled carcinoma of the cervix* in a case of Dr. Griffiths' was demonstrated in the specimen exhibited by Mr. Barris.

Uterine Myomata; Degenerations.

A giant fibroid of the cervix weighing twenty-three and a half pounds was removed successfully by Dr. Giles. My largest tumour, which grew from the cervix and broad ligament, weighed twenty-eight and a half pounds. The patient permanently recovered. The advantage of systematic microscopical examination of growths after removal was evidenced in the specimen of sarcoma shown by Dr. Jellett. The growth was supposed at the time of operation to be a simple myoma. The same lesson was taught by the post-operative examination of Mr. Ernest Shaw's dermoid cyst of the ovary, which proved to be a squamous-celled carcinoma. In the highly suspicious specimen that I exhibited all clinical symptoms and macroscopical appearances pointed to sarcoma. Dr. Lockyer proved its benignity.

The accident of a uterine or ovarian tumour, or even an ectopic gestation, being mistaken for displacement was apparent from the history of the patient who came under Dr. McCann's care. She had worn a ring pessary for thirteen years. The

tumour was a degenerating necrotic myoma of the cervix, which involved the vagina. The rather uncommon complication, *torsion of the uterine tumour*, was attended in Dr. Griffith's patient by morning pains, sweating, and vomiting. There did not here appear to be any definite cause for the torsion.

Dr. Russell Andrews performed hysterectomy on a woman five months after the death of a fœtus at full term. The tumour proved to be the enlarged uterus, and the *pregnancy occurred in one half of a septate body*, with a single cervix, the ovum possibly reaching the cavity, as suggested by Dr. Maxwell, by external migration.

Dr. Lewers' specimen of "*Atresia in One Half of the Double Uterus*" proved how such a condition might remain an undiscovered cause of dysmenorrhœa.

Ovaries.

Some affections of the ovaries of considerable pathological interest were brought before the Fellows. The rare occurrence of a disagreement among pathologists was exemplified in the report on Dr. Giles's specimen of giant adeno-carcinoma, Drs. Giles and Cuthbert Lockyer, in a minority report, adhering to their original opinion, that the characters in the grouping of the cells denoted malignancy, while the other members of the committee decided adversely to this.

Primary carcinoma of the ovary was illustrated by Dr. Garrett Anderson.

Spinal Anæsthesia.

Dr. Tate's *myxo-fibromatous ovary*, removed under *spinal anæsthesia*, demonstrated the advantage of this method in a case of the kind, where serious lung complications prohibitory to general anæsthesia were present. The same remark applies to the case of Dr. Inglis Parsons, where, in consequence of advanced cardiac disease, spinal anæsthesia was resorted to in an extensive colpoperineorrhaphy.

Hastings Tweedy added another instance to those already recorded of *undoubted ovarian pregnancy*. Mr. Ernest Shaw brought an example, rare in occurrence, of *actinomycosis*, secondary to appendical suppuration. Mr. Victor Bonney showed an *intestinal implantation teratoma* secondary to a cystic teratoma of the right ovary, which had been removed, the left ovary being healthy.

Malignant degeneration of dermoid cysts of the ovary is a subject of great interest, clinically and pathologically. Indeed, it may be said that the transitional phases in all forms of ovarian cystomata from the benign type of cell to the malignant are but indifferently understood. The specimen exhibited by Mr. Ernest Shaw, removed by Dr. Blacker, of a squamous-celled carcinoma of a dermoid cyst of the ovary, was an example of such degeneration. This and Mr. Targett's case proved the advisability of careful microscopical examination of such cysts. I have a very large dermoid cyst which I removed, where there was at the time no suspicion of malignancy, though the woman died within a year of malignant metastases to the bowel.

Fallopian Tube.

"*Papillary Columnar-celled Carcinoma of the Fallopian Tube*" conveyed the decision of the Pathological Committee on Mr. Tait's specimen, and the discussion, with instructive observations by Mr. Alban Doran, disclosed the fact that since Orthmann described the first case in 1888, just 100 others have been added.

The extreme importance of early operation in *tubercular salpingitis* was emphasised in the remarks made by Mr. Paterson. Three instances of permanent recovery from the primary affection had been previously brought by me before the Section.

Dr. Hey Groves exhibited a "*Tubal Mole*" with a normal fœtus inside the tubal cavity. Menorrhagia was the only symptom present before rupture in Dr. Aaron's case of tubal pregnancy. There were some interesting particulars disclosed at the operation in Dr. McCann's case of tubo-abdominal pregnancy in the triple attachment of the placenta to the uterus, the floor of the pelvis, and the sigmoid colon, with a special increase in the vascular supply to the placenta through the meso-sigmoid.

Mr. Barris and Dr. Williamson ascribed an abscess in an ovary removed by Dr. Williamson to the *presence of amœba coli*—similar cells with amœboid movements being found in the fæces.

There were a few interesting vaginal specimens.

Dr. Stevens supported his view that *adenomyomata of the vaginal wall* have a Wolffian and not a Mullerian origin by microscopical sections of a tumour of this character.

Dr. Russell Andrews showed a fibroid of the vaginal wall the size of a cricket ball.

From this *resumé* I think you will agree with me that the work accomplished in the last session has been worthy of the Royal Society of Medicine. The credit is entirely due to those Fellows whose name I have mentioned, and to those who have so ably and frequently contributed to the success of the meetings by their presence and the part they have taken in the discussions. I cannot sufficiently express to them my thanks for the support they have given me in the Chair. May I venture to hope that the same enthusiasm and scientific zeal may be seen in the present session, and that the 'Transactions' of the Society will be enriched with original contributions as valuable as those which have already filled their pages.

October 17th, 1910.

TWO CLINICAL LECTURES
ON
CARCINOMA OF THE STOMACH.

Delivered at the London Hospital.

By JAMES SHERREN, F.R.C.S.,
Surgeon to the Hospital.

LECTURE I.

GENTLEMEN,—Carcinoma of the stomach is a disease that has an appalling frequency. On looking up the Registrar-General's reports one finds that in England alone there are over 4000 cases every year diagnosed as carcinoma of the stomach. That probably over-estimates the frequency, because, as we shall see, carcinoma of the stomach is extremely difficult of diagnosis in some cases. In fatal cases of carcinoma it has been estimated—and it agrees with the figures here—that 21 per cent. of the cases of carcinoma causing death originate in the stomach. It may be met with at any age, and is most common between the ages of forty and seventy. In 230 cases of carcinoma of the stomach that have been in this hospital in the ten years from 1899–1908, in which the diagnosis was confirmed at operation or post mortem, the majority were between the ages of forty to seventy, and most of them occurred between fifty and sixty. It has been estimated that 2 per cent. occur in people under the age of twenty. Of the London Hospital cases only one was under the age of twenty. There have been six cases recorded in children under ten, of these two only can be definitely claimed as carcinoma.

With regard to the sex, carcinoma of the stomach resembles chronic ulcer of the stomach and is more common in men than in women.

All surgeons that have worked on the subject agree that a chronic gastric ulcer predisposes to the formation of carcinoma. The evidence has been very gradually accumulating since that possibility was first discussed by Cruvelhier in 1839. Dittrich, in 1848, described six cases of carcinoma developing in the region of a chronic gastric ulcer. The case that I am going to read is illustrative of the sort of evidence that surgeons have been collecting, that chronic gastric ulcer is a predisposing cause of malignant disease.

Last July I had in "Royal" a woman, æt. 32 years, who for thirteen years had had indigestion. This indigestion came on in attacks. She had pain half an hour after food, relieved by vomiting. For the first seven years there was no hæmatemesis at all. The attacks were of a month or six weeks' duration, and then she was absolutely well, at one time, for over six months. The frequency of the attacks increased, and for twelve months before she came into the hospital she had continual pain after food and vomited every day, and she was so ill that she had to stay indoors. The mere jolting from riding in a bus or train would bring on vomiting at once. She was a fairly bright and healthy-looking woman, who had been thoroughly treated medically for twelve months before I saw her. There was no tumour to be felt, but she had a little tenderness in the epigastric region. Her test meal gave free hydrochloric acid, about the percentage we are accustomed to get here. The only other point was that she had well-compensated mitral regurgitation. The probability of malignant disease certainly did not loom very large in my mind. I thought it was a straightforward case of chronic gastric ulcer. There was nothing to make one think that the disease would turn out to be carcinoma.

I opened the abdomen behind the right rectus muscle and came on an ulcer situated on the lesser curvature of the stomach. I thought it was a case of malignant disease, and that the wisest thing to do would be to remove a gland for sections. I accordingly took out a gland and did a gastro-jejunostomy, such as could be used if partial gastrectomy became necessary. As a result of the operation the patient had practically no relief. This confirmed the opinion that we were not dealing with ordinary gastric ulcer. In a case of chronic gastric ulcer if a gastro-jejunostomy is done the patients will get relief as soon as the anæsthetic vomiting is past, and feel they can take more food than you consider it wise to give them. But the treatment in this particular case gave no relief at all. When the sections were examined we found that the case was one of carcinoma. The next week I did a partial gastrectomy, and she left hospital three weeks later and remains perfectly well. That is the sort of case in which one meets with a carcinoma following a chronic ulcer of the stomach.

Physicians generally are rather sceptical as to the possibility of malignant growth originating in chronic ulcer. Osler and Macrae found that in only 4 out of 150 cases under their observation was there a history of gastric ulcer. They say, "We may conclude, so far as the figures show anything, that the victims of chronic dyspepsia and various forms of gastritis are not more prone to malignant disease than other individuals."

That was ten years ago; the development of gastric surgery since that time has led to entirely other views.

Taking one's own cases first, of fifty-four cases that I have operated upon, in seventeen there was a definite history pointing to gastric trouble extending over five years. In the case that I have brought before you there was, as I have said, a history of chronic gastric ulcer for thirteen years, and in another case which I am going to show you there was a similar history extending to fifteen or sixteen years. A great many people have found a higher percentage than myself. Mayo Robson, in the cases he has operated upon, finds a suggestive history of chronic gastric ulcer in 50 per cent. of them, and Moynihan in 60 per cent. Looked at from the other side, Mumford and Stone traced sixty patients treated for chronic indigestion at the Massachusetts General Hospital, who subsequently died, no fewer than thirty of them from what was clinically diagnosed as carcinoma of the stomach. I suppose the most convincing evidence is that recently put forward from the Mayo's clinic by Wilson and MacCarty, who examined all the specimens removed by partial gastrectomy, 153 in number. In 109, or 71 per cent. of those, there was naked-eye or microscopical evidence that carcinoma had originated from chronic gastric ulcer.

Another suggestive point in favour of that view is that chronic gastric ulcer as carcinoma is more common in men than in women, and also has its usual seat in the same part of the stomach. I think there is no doubt that chronic gastric ulcer is a predisposing cause of cancer of the stomach in a large number of cases. Could we prevent an acute gastric ulcer from becoming chronic or deal promptly with a chronic gastric ulcer when it has arisen, we should do a great deal to prevent the formation of a cancer of the stomach. If the cases of suspected carcinoma of the stomach were handed to the surgeon earlier, the results would be very different from what they are at the present time.

Carcinoma may affect the stomach primarily, and that is the variety we have chiefly to do with. A secondary carcinoma of the stomach is of little surgical importance. It is said to occur in 6 to 7 per cent. of the cases. I have been over all the cases that have occurred here in the last ten years, and certainly in our figures the proportion does not come to more than 1 per cent. When carcinoma of the stomach arises secondarily, it does so most often as a result of direct extension from the pancreas, the colon, the gall-bladder, and, though very rarely, from the œsophagus. There is a specimen in the museum of malignant disease of the cardiac end of the stomach having resulted from epithelioma of the œsophagus. Then by direct spread probably there are cases in which carcinoma of the stomach arises secondarily to carcinoma of the breast. Occasionally it originates secondarily to malignant disease of the upper alimentary tract.

Primary carcinoma of the stomach may be composed of spheroidal or cylindrical cells. Either of these may undergo colloid or mucoid degeneration. There is a beautiful specimen here of this form of degeneration in a carcinoma of the lesser curvature of the stomach. Under one of the microscopes there is a specimen in which you can see these changes in the cells. If the fibrous stroma is abundant the carcinoma is called scirrhus; if it is small in amount—medullary. The spheroidal cells resemble those of the gastric tubules. This type of new growth is more than twice as common as columnar, and is the usual type of malignant ulcer of the stomach. That is to say, when you find a malignant ulcer of the stomach you usually find that it is a spheroidal-celled carcinoma. Columnar-celled carcinoma often forms prominent red fungating masses in the stomach. Neither has any special preference to one part of the stomach more than another, but columnar-celled carcinoma is perhaps more common in the pyloric region. In what is called leather-bottle stomach you get the whole of the walls infiltrated with growth. This may occur either in columnar-celled or spheroidal-celled carcinoma, although it is more common in the latter.

If you read the older books you will find it is stated that 60 per cent. of all gastric cancers are

situated at the pylorus. The percentage is by no means as great as that. Since opportunity has arisen of seeing malignant growth of the stomach on the operating table, our views as to the relative situation of the disease have altered. Amongst the fifty-four cases that I have operated upon, the growth was on the lesser curvature in twenty-six, at the pylorus in fifteen only, on the greater curvature in two. The portion of the stomach in which carcinoma usually starts is on the lesser curvature, about an inch away from the pylorus; the common carcinoma of the stomach is pre-pyloric.

Carcinoma at the greater curvature of the stomach is so uncommon that I propose to read you the notes of one case at present under my care, in which carcinoma supervened on chronic gastric ulcer. The specimen removed by partial gastrectomy is before you. The patient, a woman, æt. 41 years, has just left the hospital. Since the age of thirteen she had had indigestion off and on, and had attended various hospitals and various doctors, obtaining a certain amount of relief from the pain for a time, only to recur. The symptoms became severe six years ago, when she had pain in the chest and in the pit of the stomach two hours after food, often relieved by taking more food or by lying down. After being ill off and on for two years she got well, and had what she called then three good years with only occasional pain and a feeling of fulness in the chest. The attack of pain for which she came into the hospital occurred in March, 1910, when the old symptoms came back very much worse than they were before. There is a curious point in her history for which I have no explanation—that when she was pregnant the whole of the symptoms ceased. The last child was born on February 2nd of this year, and when carrying that child she was entirely free of the symptoms. There was nothing very striking in her appearance. She had general visceroptosis with gastroptosis. The whole diagnosis was by no means clear; the one thing certain was that she had gastroptosis. I always explore cases of that kind, because I am never satisfied that gastroptosis explains all the symptoms. If on opening the abdomen you find it is a case of gastroptosis alone, close the belly at once, unless it has produced pyloric obstruction by kinking, when the best thing is to do a Finney's operation.

I explored, and found a large mass on the greater curvature, and hard enlarged glands in the omentum. Even then I was not certain about the nature of the disease. If you look at the specimen, you will see a raised yellowish nodule rather suggestive of gumma, and as gumma is much more common on the greater curvature of the stomach than anywhere else, I thought it was probably a case of that kind. But no matter what it was there was only one thing to be done, and that was partial gastrectomy. I did it in the usual way and removed the stomach that you see there. The sections showed medullary carcinoma.

The other case of carcinoma arising at the greater curvature of the stomach I also treated by partial gastrectomy, a man, who lived about a year afterwards, with comfort for nine months.

The cardiac end of the stomach is said to be affected in somewhere about 9 per cent. of the cases. That, again, is rather higher than the percentage of the cases we have here. The growths in the stomach are occasionally multiple. When they are multiple you generally find them on opposed surfaces of the viscus. The growth starts always in the deeper layers of the mucous membrane or at either edge of the ulcer. If it starts at the edge of the ulcer, it usually starts at the edge of the ulcer next the pylorus. I cannot confirm that from my own experience; I have never been lucky enough to remove a stomach in which one edge of the ulcer was inflammatory and the other showed definite malignant changes.

With regard to the method by which the growth spreads. First of all there is a direct spread in the stomach itself, then a direct spread to the adjoining organs immediately, or through adhesion. There is spread also by the lymphatic system and by the blood-stream. Cancer in the stomach tends to spread along the lesser curvature, and it spreads toward the pylorus, but involves the duodenum in very few cases. Brinton, who was the first systematic writer in English on diseases of the stomach, found that it involved the duodenum in 10 out of 125 cases. That is a much higher percentage than found by later observers. As the result of microscopic examination of sections removed, however, one finds that in about one third of them the growth has spread into the duodenum. That is to say, it would not be safe in doing a partial gastrectomy to remove the stomach

absolutely at the pyloric level, but an inch of the duodenum must be removed as well.

The growth extends widely into the submucosa; the area of induration will mark the limit of the growth in the mucosa, but the cells spread in the submucosa fully an inch beyond the area of induration. The serous and muscular coats are involved to a less extent.

The organs that are affected directly by the spread include the pancreas most often, then the liver and colon, and very rarely the spleen.

Adhesions are present at the time of death in far the greater proportion of cases. In the later stages of the disease that is an important method of spread.

It is impossible to understand the present operation of partial gastrectomy without knowing the lymphatic arrangements of the stomach. The lymphatic vessels first of all start around the glands in the deep layers of the mucous membrane and form plexuses in the submucous and muscular and subserous coats. These are continuous throughout the whole of the stomach and communicate with those of the duodenum and œsophagus. That will make it plain why you get deposits in the mucous membrane of the duodenum—you have the lymphatic plexuses continuous between the stomach and the duodenum and also between the stomach and the œsophagus. From the subserous plexuses the vessels pass out and enter the lymphatic glands, into which they drain. Although as a result of experiments by injection of colouring matter into the lymphatics it was found that there was no barrier between one lymphatic territory and another, yet if an injection was made into the lesser curvature, the vessels in the region of the lesser curvature would all become filled before the injection fluid would flow through to the other territories of the stomach—there is a lymphatic barrier between certain parts of the stomach.

With regard to the groups of lymphatic glands themselves, they are placed along the course of the blood-vessels. The first group are those which accompany the coronary artery and lie along its descending branch, and are divided into two groups. There is the upper group—two or three glands which lie in the fold of mucous membrane in which the coronary artery runs. Then there is the lower group which lies along the lesser curvature of the stomach, and extend to the right as far as mid-point between the pylorus and the cardiac end of the stomach. There are some outlying glands of this group occasionally present, called paracardial, surrounding the cardiac orifice, but these are very seldom affected. If the upper coronary glands are traced backwards they become continuous with the cœliac glands. These glands are divided up into three groups, right, left, middle supra-pancreatic. The left glands lie along the splenic artery, the right glands on the stem of the hepatic artery, and the middle are situated around the cœliac axis itself. The glands which are sometimes present in the gastro-splenic omentum belong to the group of splenic glands.

Another important group are the subpyloric, situated at the angle between the pylorus and the first and second portions of the duodenum. If they are traced to the right they are continuous with the glands which accompany the right gastro-epiploic artery. These seldom extend further to the right than the mid-point of the lesser curvature. The sub-pyloric group of glands receive the lymphatics from the pylorus, from part of the greater curvature, and also the efferent vessels from the gastro-epiploic glands. There is one important thing about these glands: they have a tendency to spread down into the greater omentum, so that when the stomach is being removed it is always necessary to remove as much omentum as possible to get below any outlying glands.

The stomach can, roughly, be divided into three lymphatic territories—the area of the lesser curvature, the area of the greater curvature, and the area of the fundus. The territory of the lesser curvature drains into the upper and lower coronary glands. As a rule the vessels go to the lower group of coronary glands first, and their efferents run to the upper group. The vessels from the fundus pass to the paracardial glands, and to the splenic glands.

In the usual form of carcinoma of the lesser curvature the first glands to be involved are the coronary set; in the greater proportion of cases the sub-pyloric glands are also affected, and in the later stages of the disease the glands at the hilum of the liver, those around the cœliac axis, the superior mesenteric glands, the biliary and the meso-colic and the axillary glands. There are no vessels directly connecting the glands at the hilum of the liver with the stomach, but they receive the lymphatics from the liver and transmit them to

the right supra-pancreatic group so that the glands of the hilum of the liver must become infected either from the liver or through blockage of other lymphatic tracts.

The glands in the left supra-clavicular region are often affected in the later stages. The enlargement of those glands is said to take place in 3 or 4 per cent. of the cases. Their enlargement cannot be relied on to be of much help in diagnosis. Occasionally in cases which are definitely chronic ulcer you get enlargement of those glands, and I have known the enlargement disappear a few weeks after a gastro-jejunostomy has been done.

With regard to vascular infection, this takes place through the veins of the portal system, causing secondary growths in the liver. That infection is most common in carcinoma of the lesser curvature. Carcinoma of the pylorus itself tends to infect the liver by direct spread. The growth becomes adherent to the under surface of the liver and spreads directly into it.

With regard to symptoms, the most important point to bear in mind is that there is no symptom or group of symptoms by which carcinoma of the stomach can be definitely diagnosed when it is really operable. The symptoms vary according as the growth affects the pyloric or the cardiac orifice or the body of the stomach.

In carcinoma of the pylorus pyloric obstruction appears early, and one suspects carcinoma of the stomach when symptoms of pyloric obstruction come on in an adult. Then in growths in the cardiac orifice the symptoms are those of œsophagus obstruction.

Cases of carcinoma of the pre-pyloric portion of the stomach fall into three groups according to their symptoms. First of all there are the cases which follow previous gastric disease, then those which occur in previously healthy persons, and thirdly, those in which the growth is more or less latent.

Those cases in which the symptoms follow previous gastric disease comprise a large percentage of the cases that come to us for operation, and in many instances one has a typical history of chronic gastric ulcer, attacks of pain after food, which may be accompanied with vomiting which relieves the pain. But one has to remember that vomiting is absent in many cases of chronic gastric ulcer, and therefore in the diagnosis very much stress must not be laid on the vomiting. Occasionally the patient notices that in the last attack the pain was not so readily relieved by rest, or if the attacks have been associated with vomiting the pain is not relieved by the vomiting, but becomes more constant and gnawing in character and often wakes him at night. There is often a distaste for food, whereas in ordinary cases of chronic gastric ulcer the patients have no distaste for food even when it gives them pain. In a few cases, as in the first history I gave you, there are no definite changes in the symptoms as compared with what they were five or six years before. Very occasionally one finds that there is a history of some gastric disease many years previously. The patient had something which was supposed to be gastric ulcer, and he was treated medically and apparently became well, and had no further trouble until carcinoma started in later life. That is a very unusual form of the disease, and it would be suggestive of malignant disease supervening on the scar. If these cases were common it would mean that every case of chronic gastric ulcer should be excised, and that we should not be content with doing a gastro-enterostomy.

As to the second group of cases, those in which the disease originates in people who have been previously healthy so far as their stomachs are concerned, an acute hæmatemesis occurring in such a person may be a sign of carcinoma of the stomach. There was a patient in the hospital twelve months ago with acute hæmatemesis. He was thought to be suffering from cirrhosis of the liver. I have just seen him now; for a few months he has had vague pain after food of a gnawing character with occasional vomiting. The pain sometimes wakes him at nights. He is gradually losing weight. Examination shows that he now has carcinoma of the stomach. In other cases the patient puts the symptoms down to some indiscretion of diet taken a short time before. But as a rule the onset is quite insidious. Occasionally the first symptom noticed is a distaste for food. At one time considerable stress was laid upon that symptom, but it is not common as a first symptom. Among the cases I have seen and made inquiry into it was the first symptom in only two of them.

The first and most constant symptom is pain. As a rule the pain is not so sharp or localised as in chronic gastric ulcer, and is not so often relieved

when the stomach is at rest. If the patient vomits the vomiting does not relieve the pain for long, and the pain is very seldom relieved by rest. The vomiting may be absent or infrequent when the growth is pre-pyloric, and it very often occurs independently of food. The vomit is often offensive and contains small quantities of altered blood. In growths causing pyloric constriction vomiting is the first symptom, and if the vomit is examined carefully in every case 90 per cent. of them will show the presence of altered blood. As a rule it is slight and can only be revealed by the various tests for occult hæmorrhage. Bleeding sufficient to cause hæmatemesis does not occur in more than 5 per cent. of the cases. When the growth is diffused and involves the whole of the stomach, as in leather-bottle stomach, the patient finds increasing inability to take more than a certain amount of food. Vomiting occurs if more is taken, and the amount that can be taken gradually diminishes.

A tumour is present in over 70 per cent. of the cases that one sees nowadays. But a tumour is a late sign. It was at one time stated that the presence of a tumour means that the disease is inoperable, but the presence of a tumour really means that the disease is late and that the ultimate outlook is bad. There are many patients who live for years after a partial gastrectomy has been done and a tumour was present before the operation. So that a tumour means that the disease is late, and does not mean that it is inoperable, but that it is too far advanced to cure. The majority of tumours in association with carcinoma of the stomach are found in the epigastric and umbilical regions. But a tumour associated with pyloric carcinoma may be found in any portion of the stomach, and shifts its situation at various times of the day according to the state of the stomach. Growths in the lesser curvature and tumours in cases of leather-bottle stomach are usually found in the epigastrium and the left hypocondrium, and very often can only be discovered by inspection. If the patient is uncovered and the upper part of the belly looked at, the tumour can be seen descending when the patient is taking a deep inspiration.

Occasionally the first thing that will attract the attention of the patient is the presence of an actual growth in his abdominal wall at the umbilicus.

Next time we will consider the diagnosis and surgical treatment of the disease.

October 17th, 1910.

A CLINICAL LECTURE

ON

NON-MALIGNANT STRICTURE OF THE RECTUM.

Delivered at St. Mark's Hospital.

By FRED C. WALLIS, F.R.C.S.,
Surgeon to the Hospital and to Charing Cross Hospital.

GENTLEMEN,—This evening I want to talk about non-malignant stricture of the rectum, and this subject follows somewhat naturally on what I recently told you about sigmoiditis and proctitis; and in dealing with this present subject I expect I shall have to traverse again some of the ground I went over with you then. Non-malignant stricture of the rectum is practically always the ultimate result of some destructive process of ulceration. That you may take as a statement of fact. This ulceration, from whatever cause it arises, gradually destroys the mucous membrane by infiltrating the submucous tissue. After that the formation of fibrous tissue commences, which is the healing process. As this fibrous tissue forms it contracts, and in proportion to the depth of the original ulceration will be the amount of fibrous tissue, and the degree of stenosis will also depend on this. This ulcerative process usually rings round the bowel completely. Of course there may be patches of ulceration here and there which will heal and leave a cicatricial contraction of a part of the bowel. but usually the submucous tissue infiltration involves the circumference of the bowel.

Now as to the forms of ulceration. There is infective ulceration including post-operative ulceration, dysenteric and tuberculous. I have left out syphilitic, which is erroneously said to be the commonest cause of ulceration of the rectum, because, as I have told you before, I have never yet seen a case of syphilitic ulceration of the rectum, and as I have been at this special work fifteen or sixteen years, I think if it had been at all common I should have seen at least one case of it. In that connection I may say I have occasionally seen syphilitic conditions of the skin around the anus—large, pendulous, fibrous masses which are found in a syphilitic subject, which had infiltrated the peri-anal tissue and caused some contraction just round the

anus. But in the mucous membrane of the bowel itself I have never seen any gummatous infiltration. We have spoken of the infective form of ulceration in a previous lecture; it is a process of infective ulceration which starts at the ano-rectal junction as a rule—that is, when it is not a post-operative infection. This form of ulceration is much more common in women than in men, and the symptoms are already known to you. Post-operative infective ulceration is a serious matter, and those who have read the recent correspondence in the 'British Medical Journal' will see that there we have been discussing various operations for hæmorrhoids. One of the main reasons for which I took up the radical cure of hæmorrhoids was to avoid this post-operative sepsis which occurs in connection with the ligature operation for hæmorrhoids. The following case is typical of the dire effects of this form of ulceration.

A butcher, æt. about 28 years, and who looked as healthy a man as one could wish to see, had been operated upon three years previously for hæmorrhoids by means of ligature. He had a very bad infective ulcerative proctitis with commencing stricture, and he had enlarged inguinal glands and a bubo on one side.

It is not the question of one case or two cases of the sort, and those who have seen these cases can hardly think the operation by ligature is so free from dangers and risks as some people would have us believe. The cases I have seen have made me anxious to do away with all sorts of buried or hidden sutures, because of the hidden ulcer which must always arise when a silk ligature ulcerates through the material with which it is tied up. This is an ulcer which it is difficult, almost impossible to get at and keep surgically clean; it is not as if it were outside, as it would be in an ordinary radical excision. If anything goes wrong with it it may go exceedingly wrong before it is discovered. Again, a large post-operative retro-rectal abscess may occur after ligature of hæmorrhoids, and that I have seen recently. Remember, then, that post-operative infective proctitis may follow ligature of hæmorroids, and this is the commencement of stricture.

Dysenteric ulceration is different from the submucous infective infiltrating ulceration which I have written so much about. Dysenteric ulceration is more often in the sigmoid and upper part of the rectum than at the lower part, although eventually it gets low down, also after dysentery ulcers occur in various places in the large bowel, notably in the cæcum. The ultimate result of dysenteric ulceration may be a bad fibrous stricture. With tuberculous disease it is not so common. Tuberculous disease causes much destruction, with no compensating hyperæmia as a rule, and no production of fibrous tissue. It is a destruction without any form of repair in the way of fibrous tissue. Fibrous tissue is a repair, though a very poor one. Uuless you get the tissues healthy, so that they react to any stimulus which you may employ in the way of high frequency, or ionic medication, you will not get contraction with it, but the ulceration will gradually extend. Still, in the case of tuberculous ulceration as in the other forms, the best ultimate result you can have is fibrous stricture.

The *symptoms* of stricture are that the patient has a certain amount of discharge of blood-stained mucus, and if you examine it you will find pus also, and there is a good deal of straining required to get the bowels open, and this difficulty is an increasing one. It is noticed that after an aperient there may be no result whatever, except that the patient feels very uncomfortable, the abdomen becomes distended, and then later fermentive diarrhœa occurs, and the symptoms resemble those which occur in advanced carcinoma of the rectum, where the lumen of the bowel is so narrowed that it is only by the fermentation of the fæcal contents of the intestine that the large bowel can unload itself. Exactly the same happens in these cases of fibrous stricture. The common history is that these people have great trouble in getting the bowels to act, and then if they take medicine they may find the bowels act six to eight times in the morning, and by the time it is two o'clock in the afternoon they are fairly comfortable except that they are somewhat exhausted from the frequent stools. In addition there are general indications of toxic absorption, the skin is sallow and the appetite poor, there is bad sleeping, the tongue is foul, and often these patients have a curious odour. There is generally a temperature, 100°, 100·4°, 101° F., at night.

The usual situation for the fibrous stricture is 2½ to 3 in. up, very rarely lower down, and not often higher up. Last week I had under my care in the Grosvenor Hospital, and operated upon, a woman, æt. 35 years, who had all these symptoms, which had been gradually coming on for years. There was no syphilitic history about her. She had had several healthy children, but apparently one or two bad labours. On examining her one found that she had healthy mucous membrane for

about 1½ in. up, and then obviously it was rather roughened, like a feeling of rough velvet to the finger, and as one got further up it became worse, until finally there was no mucous membrane and only fibrous tissue. This went on for 1½ in., and beyond and above it one found healthy mucous membrane again. All the interval was filled up with submucous infiltration, although the mucous membrane over it and down by the sphincters was quite normal. This is a typical case of fibrous stricture which occurs as a result of infective ulceration, and you may take it as a fact that this is the usual form of ulceration which results in cicatricial contraction. It is a most lamentable state of affairs, and the life of these patients is a misery; there is nothing but misery in it, the whole of their life seems to be centred upon whether they can get their bowels to act, and how exhausted they are when they have finished their evacuations. The unfortunate part is that people do not come for treatment often until the stricture is quite pronounced, and what that means is that when the stricture is quite pronounced the ulceration has probably extended far up—that is to say, not only has the cicatricial contraction gone on which produced the stricture, but beyond that the ulcerative process continues in the submucous area. When examining these people you must be a little careful. When the stricture is a full three inches up, and perhaps extends another one and a half inches beyond that, of course it may invade the peritoneal reflection there, and it often does, and if, in using any undue force to get through the stricture, it is torn, there will be a leakage from the bowel into the general peritoneal cavity, and either general or pelvic peritonitis is the result. In all these cases of examination for stricture you must be careful, even when you feel that the stricture is apparently yielding, and if you are not very gentle you find the finger or bougie, if you are unwise enough to use one, give a sudden jump, like the passing of a catheter into a false passage. You must treat anything of that sort with the greatest suspicion. Personally I am not keen about doing these things unless one has the patient absolutely under an anæsthetic, and watch with a good light, so that one knows exactly where one is and what one is doing.

If these people come quite early, before they have their strictures, a great deal can be done for them by means of ionic medication, as I mentioned at a previous lecture. It is a revelation to see the effects of zinc cataphoresis on these people. Treated by this method cases which have gone on for years will heal up. I have a woman now under my care who has gone through the whole history of this ulceration and she is now perfectly healed from her ulceration by means of cataphoresis.

As to treatment, I am giving you the result of my own experience in the surgery of these parts, and I have studied these ulcerations probably more than most people. My experience is that if the ulcerative process has finished and it is within reasonable reach from the anus, the proper surgery is to excise the whole of the affected tissues, leaving the sphincter, of course, as in excision for hæmorrhoids, and cutting away all fibrous tissue until normal mucous membrane is arrived at. It will very likely be necessary to remove three to four and a half to five inches of bowel, and also even to open the peritoneal cavity. Remember this fibrous tissue is non-elastic, and has numerous blood-vessels, and directly you cut into it the hæmorrhage is considerable. But you must not be alarmed about it, and do not waste time in trying to pick up vessels with forceps. Free one side, and whilst doing so let it bleed for the moment, then plug with cotton-wool saturated with hot hazeline and water, and work away on the other side. Then go back to find the bleeding has stopped, and so work from one side to the other until you get up to normal healthy mucous membrane. Then gradually free this until you can pull it down to the anus, and remove the affected tissue. Put some interrupted silkworm-gut sutures through the muscle and the mucous membrane, giving it a firm hold, and treat it as an ordinary excision for hæmorrhoids. Those cases do remarkably well. As a rule you will find that there is some little separation about the fifth or sixth day at the anal margin. Acute attacks of synovitis are of frequent occurrence in these cases, associated with rigors and high temperature. These attacks are usually mistaken for rheumatic fever. They are *not* that. These swollen joints are due to an infective synovitis. The effusion disappears in three or four days, and leaves no practical ill-effects. In the very bad cases a colotomy is necessary, and there are people whom I have operated upon in this hospital many years ago who were miserable and wretched women and men, and are now perfectly cheerful and go about comfortably with their colotomy wounds. They have great relief from their former condition. There is no absorption from the large bowel, and they can wash through their colotomy wound and keep the parts clean. And with this ionic medication you can cure the ulcerated lower bowel, and even if after it has healed you do not feel justified in excising it—and very often you do not feel justified in doing so—the ulcerative infective process is cured, and these people are content with their colotomy. I do not propose to discuss the operation of colotomy to-day because it is reserved for me later on in another lecture. But I wrote a paper, which I think was published quite lately, on colotomy, and there I described a method of washing these people out by means of enemata, which most of you probably know, but it is extraordinary how few people do know about it. If this is properly done the colotomy is of little trouble.

October 17th, 1910.

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TWO CLINICAL LECTURES ON CARCINOMA OF THE STOMACH.

Delivered at the London Hospital.

By JAMES SHERREN, F.R.C.S.,
Surgeon to the Hospital.

LECTURE II.

GENTLEMEN,—There are two or three late symptoms that I have to say a few words about, namely, anæmia, loss of weight, and change in the gastric secretion. Anæmia is present in all advanced cases. Some cases of carcinoma of the stomach have been treated for a considerable time as pernicious anæmia. If the blood is carefully examined the error will not arise; the blood changes met with in carcinoma of the stomach are those of a secondary anæmia. It has been stated that in carcinoma of the stomach the leucocytosis normally present during digestion is absent. Very little reliance can be placed on this symptom.

With regard to progressive loss of weight, it may very rarely be the first sign of the disease. One patient of mine had been in the habit for years of weighing himself regularly once a month; during the twelve months before he came under observation his weight went down from 16 st. 3 lb. to 12 st. 6 lb. At the end of that time he began to get gastric symptoms, and when I saw him eight weeks later he had carcinoma of the lesser curvature, which on exploration was inoperable. Although he had been losing weight twelve months, he had only had gastric symptoms eight weeks.

Changes in gastric secretion I have put last because they are at present of minor importance. As you know, in certain cases there is an absence of free hydrochloric acid; in others it is present throughout the whole course of the disease. In the first two cases whose notes I read to you last week in which I performed partial gastrectomy for

carcinoma of lesser curvature supervening on chronic ulcer and for carcinoma of greater curvature, free hydrochloric acid was present in the usual percentage we find here after a test-meal. The presence, therefore, of free hydrochloric acid does not signify that the disease is not malignant. It has been stated that free hydrochloric acid is only present in cases where carcinoma has originated in or is secondary to chronic gastric ulcer. Certainly in these two cases I have just mentioned there was a previous history of gastric trouble. There is one other point: it has been stated that free hydrochloric acid may be absent from the gastric secretion when carcinoma is present in organs other than the stomach. That is not usually so. Willcox has found free hydrochloric acid present in patients with carcinoma of the pancreas, of the gall-bladder, and other organs adjacent to the stomach. The absence of free hydrochloric acid in carcinoma of the stomach is due to concomitant chronic gastritis. On examining the mucous membrane removed during operation, it is found that in the cases in which free hydrochloric acid is absent the glandular elements disappear, and their place is taken by cylindrical epithelium.

Much more important is the estimation of the amount of active hydrochloric acid present, that is, the hydrochloric acid which is free and combined with proteid and organic bases. It is probable that in most cases of carcinoma of the stomach the active hydrochloric acid is diminished, although free hydrochloric acid may be present.

Malignant disease of the stomach is sometimes latent, the first symptom observed being loss of weight, ascites, or a tumour.

Taking the cases with ascites first, they fall into two groups—those in which the ascites is the first serious symptom to attract the attention of the patient, and those in which, following on vague abdominal signs, the patient is seized with acute illness and the belly is noticed to be swollen. I have had four cases under my care in which ascites was the principal symptom; in two the onset was gradual, in the other two, sudden. One of the cases in which the onset was gradual was a woman, æt. 31 years, who was in Royal Ward under my care last year, sent to me as tuberculous peritonitis. She had had vague pain in the "stomach" after food for five months, and for two months gradual swelling of the abdomen and loss of weight. Free fluid was present in the abdominal cavity, but there was no tumour; there were a few suspiciously hard glands in the left supra-clavicular region. I thought she had carcinoma of the stomach, because tuberculous peritonitis occurring in a person thirty-one years of age is very rare, and the vague indigestion, the gradual loss of weight, and the presence of the enlarged glands pointed to the diagnosis. On exploration I found a number of tiny nodules looking very like miliary tubercle scattered on the mesentery close to its attachment to the small intestine. The stomach was infiltrated with new growth, most advanced on the lesser curvature. There were a number of enlarged glands in the lesser omentum. I removed one of these and the sections showed spheroidal-celled carcinoma. This is another instance in which the carcinoma must have been in existence for a considerable time although definite symptoms were absent.

Illustrating the rapid development of ascites with acute symptoms the following is a good example: A man, æt. 40 years, was admitted in April, 1905, with a history of vague pain after food for two or three months, without hæmatemesis or vomiting. Two days before admission he was suddenly seized with pain in the upper part of the belly and frequent vomiting. When I saw him he was obviously extremely ill, there was considerable distension of the whole of the abdomen, with general deep tenderness, his pulse was 130, and his tongue dry. He was sent in as perforated duodenal ulcer; with this diagnosis I concurred. On opening the belly a quantity of clear fluid came out, and I noticed on the mesentery close to the gut and on the parietal peritoneum a number of tiny hard nodules, which I thought were probably tuberculous. I did not explore further but closed the belly, considering it a case of tuberculous peritonitis. The patient died two or three days afterwards; at the post-mortem examination we found a huge malignant growth of the stomach situated on the lesser curvature, with generalised malignant peritonitis. There were no signs of perforation.

In a few cases secondary deposits call attention to the disease; these are usually in the liver. I have recently had an interesting case of this nature under my care. A man, æt. 60 years, had had indigestion all his life. About a month before admission, when cutting the grass of a tennis court,

he "strained himself," and since then had had pain in the belly and a "lump." During the whole of this time he had lost desire for food. On admission, I found in the epigastric and hypochondriac region a tumour continuous with the liver. As the diagnosis was by no means clear I explored, and found the tumour to be malignant growth of the liver; the post-mortem examination proved it to be secondary to malignant growth of the stomach. He was well enough to do his work up to the time of the strain, and when he came in the prominent symptom was an enlarged liver.

A malignant ovarian tumour may be the first symptom of serious illness. In one patient under my care this was the condition which brought the patient under observation.

Occasionally cases of carcinoma of the stomach terminate by perforation. Perforation is said to take place in 3 per cent. of the cases. That, probably, is about the percentage. Of the fifty-four cases I have operated on, perforation had taken place in one. A fistula may form externally, or communicating with the colon, or, rarely, with the duodenum.

With regard to diagnosis, a *certain* diagnosis of carcinoma of the stomach is impossible at the most favourable time for radical treatment. All the signs which are positive—changes in the gastric secretion and the presence of tumour—are late signs. When the gastroscope is perfected and its use is as safe as exploratory laparotomy it may be possible to diagnose the disease early, and it is possible that changes in the blood may in future be worked out, but at the present time it is better to explore the abdomen than to wait. All we can say at present with regard to diagnosis is that surgical treatment must be undertaken in all cases in which dyspeptic symptoms in an adult fail to respond to efficient medical treatment. The medical treatment of course must be thorough, and dental sepsis radically dealt with. If after efficient medical treatment the dyspepsia does not disappear, or if it recurs as soon as the patient gets up or resumes work, the sooner the abdomen is opened the better. In all such cases the stomach contents should be examined and the amount of active hydrochloric acid estimated.

So soon as there is suspicion of a malignant growth exploration should be undertaken; you must not wait to make a certain diagnosis. When the abdomen is opened and a tumour is found in the stomach, it may be impossible to say without microscopical examination whether it is malignant or not. In such cases it is helpful to know the percentage of active hydrochloric acid present; if it is raised the "tumour" may be treated as a simple one. Many cases have been reported in which a partial gastrectomy was undertaken for what was thought to be carcinoma, but when microscopically examined turned out to be chronic gastric ulcer. Operations have been done for stricture of the pylorus, believed to be malignant, and the patients have become perfectly well and the tumour has disappeared. I have seen a large stomach "tumour" completely disappear ten days after I had performed gastro-jejunostomy. In diagnosis on the operating table the points to be considered are: the appearance and feel of the tumour, its method of spread, the condition of the lymphatic glands, the presence or not of active hydrochloric acid. In the photographs that were on view here last week you saw the typical appearances of carcinoma of the stomach with lymphatic infection. The shaggy red appearance of the peritoneum that you meet with in chronic gastric ulcer is never present in carcinoma of the stomach. In chronic gastric ulcer you have a depressed area covered with peritoneum, that is, thick, often shaggy, and usually red speckled. On palpating a malignant growth distinct induration may be felt, hard and irregular. If any doubt exists and enlarged glands present, one nearest the suspected growth should be removed and a rapid microscopical section prepared. If there are no enlarged glands present, a portion of the edge of the ulcer should be removed and prepared in a similar way while gastro-jejunostomy is performed, such as can be used if partial gastrectomy becomes necessary. If it is impossible to obtain the section rapidly, the belly should be closed after this and partial gastrectomy done a week later if the section shows malignant disease.

Less than fifty years ago Brinton wrote, "The treatment of cancer of the stomach might be almost left unnoticed." He gave the average duration of life as twelve months after the onset of symptoms. This is in accord with later observations. The outlook now is entirely different and extremely hopeful, the prognosis of carcinoma of the stomach, if treated early, is good, and even if

recurrence takes place life may be prolonged in comfort for a considerable time.

The treatment of suspected carcinoma is surgical. If the question is raised that a growth is present in the stomach no time should be wasted; exploration should be undertaken. If indigestion appears in an adult who has no bad habits and is not overworked, and it does not yield to thorough medical treatment, the abdomen should be explored. If this were carried into effect many cases would be operated upon in a pre-cancerous stage, and the percentage of operable cases would be much higher. The percentage of cases suitable for radical treatment must vary. Of the fifty-four cases that I have operated upon I was able to remove fourteen with a fair hope of success. That is higher than in many other series. Every case of suspected or proved carcinoma of the stomach, unless it is certainly inoperable by reason of secondary growths in the liver, should be explored, and one must be prepared to do a partial or complete gastrectomy. The operation of choice is partial gastrectomy, though in some cases it may be necessary to do the "complete" operation. Gastro-jejunostomy should only be done when there is definite obstruction; in all other cases it fails to give the slightest relief. If cardiac obstruction is present gastrostomy should be done. A growth in this situation has been excised in two cases, but neither of the patients survived longer than fifteen or sixteen days.

The abdomen should be opened in such a way that no hernia results afterwards. An incision is made over the middle of the right rectal muscle above the umbilicus, and its anterior sheath divided; the muscle is pulled outwards and the posterior sheath and peritoneum cut through. The whole stomach and duodenum are then examined, and if a lump is present of uncertain nature its character must be settled on the lines I have just laid down, a gastro-jejunostomy being done meanwhile. If the report of a rapid section shows the presence of malignant disease the stomach should be removed. If a pathologist is not at hand the belly should be closed. If the ulcer happens to be on the lesser curvature you can usually tell its character as the result of the gastro-jejunostomy. If the ulcer be malignant no relief is afforded by the operation; if simple, improvement is extremely rapid. If the case is obviously malignant the first thing is to settle the question of its operability. Your decision will rest on whether the growth is adherent to or infiltrating other organs, such as the liver and pancreas, and on the presence of secondary deposits. If, from the presence of adhesions, it is impossible at first to make out whether the tumour is malignant or not, a gastro-jejunostomy should be done, and the abdomen reopened at the end of a fortnight; if the tumour is inflammatory it will have disappeared, but if it has not disappeared it is sure to be of a malignant character, and a partial gastrectomy should be carried out if possible.

With regard to the death-rate of exploration if nothing further is done, this is quite small. If the operation is done quite early there should be no death-rate; in these cases the only risk is the anæsthetic. But if the operation is carried out in advanced cases in order to see whether any relief can be given by gastro-jejunostomy, there will be a certain amount of risk. The death-rate, taking advanced cases as well, is about 3 per cent.

Occasionally, after an exploratory operation, all the symptoms disappear for a time. I had a striking case of that some time ago. A man, æt. 48 years, was sent to me in October, 1903, as carcinoma of the stomach. He had had symptoms suggestive of malignant growth for eight weeks; they had not yielded to medical treatment. I explored him and found a large malignant growth on the lesser curvature of the stomach. The case was quite inoperable, and I closed the abdomen. The man left the hospital a month later, feeling well. He was a carpenter, and went back to his work, and for fourteen months was perfectly free from symptoms. He was admitted to the hospital again, and died in March, 1905. Post-mortem examination confirmed the diagnosis by finding a huge carcinoma of the stomach. A similar case is recorded by Keen and Stewart, in which symptoms were absent for seventeen months after exploration.

In doing a partial gastrectomy you should remove the growth together with the whole of the lesser curvature, a portion, usually the pyloric half of the greater curvature, the first part of the duodenum, the whole of the lesser omentum, and as much of the greater omentum as you can safely get. It is sometimes necessary to remove a thin slice of the pancreas also. In other cases, when the tumour is at the pylorus and adherent to the liver, a portion of the liver may be removed with the tumour. The operation is started by securing the pyloric and gastro-duodenal arteries and dividing the lesser omentum close to the liver. The hand is then inserted into the lesser sac and behind the stomach. so as to free it from the pancreas and separate the anterior layers of great omentum from transverse meso-colon. The one thing you have to be careful about at this stage is to avoid injury to the middle colic artery. The great omentum is tied off and

as much of it removed as possible. Then the first part of the duodenum is clamped, cut through, and closed. The stomach is next turned over to the left, and the coronary artery divided between two ligatures. When that is done perform a posterior gastro-jejunostomy, remove the stomach, and close the belly.

In complete gastrectomy the first stage of the operation is the same, except that you have to ligature off the whole of the great and the gastro-splenic omentum. The operation is completed by uniting the jejunum to the œsophagus. As you unite the jejunum with the œsophagus you gradually sever the connection between the stomach and the œsophagus.

It is an important thing for the patients to be properly prepared for operation. An endeavour is always made to get the upper alimentary tract as sterile as possible. All septic teeth have to come out before the operation. In the usual chronic case, if treated ideally by the physician before coming to the surgeon, this should have been done. No sane surgeon would ever do an abdominal operation, except as an emergency, on a patient with septic teeth.

For twenty-four hours before operation nothing should be given but liquid diet—milk, Benger's food, and the like. If there is any evidence of gastric dilatation the stomach should be washed out. If there is advanced dilatation the stomach should be washed out daily for several days before the operation. In every gastric operation, if the whole of the stomach and duodenum has been carefully examined, and no evidence of disease is found externally, never waste your time by opening the stomach, but look at other organs, especially the gall-bladder, the kidney, and the appendix. If you find nothing by external examination you will never find anything of importance in surgical treatment by opening the stomach and looking inside.

With reference to the after-treatment of gastric operation cases, as soon as the patient has recovered from the anæsthetic he should be propped up in bed in a sitting position. I never have any abdominal case, unless having a tube for drainage, nursed lying down; they sit right up, and when resting lying down lie on the back or whichever side is most comfortable. It is wise to keep the patient confined to bed for about ten days after operation, chiefly for the sake of the abdominal wound, but I have seen no ill-effects when the patient has got up on the second or third day, except a little redness about the stitches. My only objection to allowing them up early is that the scar may not heal quite so soundly. So soon as the anæsthetic vomiting has ceased you may let them have as much water to drink as they like. Then start them with Benger's or Allenbury's food, which they usually prefer to milk. And the diet is gradually increased until light, solid food is taken about a week after operation.

As to the results of partial gastrectomy, the average immediate mortality at the present time is about 25 per cent. In the hands of those that have done large numbers it is less; thus the Mayos have operated on 100 cases with fourteen deaths. I have performed partial gastrectomy for carcinoma of the stomach on sixteen occasions with three deaths. One died two months after operation. I had excised a portion of her liver with a pyloric growth; she developed a biliary fistula. One died of shock within twenty-four hours of the operation. In the other case the patient failed under the anæsthetic, and the wound had to be stitched up in a hurry with one layer of stitches; on the fourth day the stitches gave. He was sewn up again, but he developed broncho-pneumonia after the second anæsthetic and died. The death-rate for this operation should now not be more than 15 per cent.

In reference to after-results, cases are reported to have lived, some for fourteen years, some for eleven and a half years, some for eight years, and many others for five years after partial gastrectomy for what was certainly carcinoma of the stomach. The average duration of life after operation, when recurrence takes place, is eighteen months. When you consider that those patients in whom recurrence takes place have had symptoms usually for two or three months before the operation, and that from the commencement of the symptoms the average duration of life without operation is about twelve months, even this is a considerable gain, for after being operated upon they can attend to their work for some time, and the quality of their life is good. They have no indigestion and they suffer from no discomfort until recurrence takes place. Recurrence is rare after two years, and if life is prolonged beyond four years there is good prospect of a cure.

Taking all the cases, they would seem to show that the average mortality resulting from the operation has been 25 per cent., but should be no more than 15 per cent. Of the 75 per cent. who recover from the operation, 60 per cent. will probably die of recurrence within three years, and after a period of comfort of certainly two and a half years. The remaining 15 per cent. will survive for five years and possibly be cured. Of my own cases, one lived three years and three months, three years of which were in *perfect* health.

Gastro-jejunostomy should be done to relieve symptoms when pyloric obstruction is present, or the growth produces an hour-glass stomach. The death-rate from gastro-jejunostomy is somewhere about 15 to 20 per cent. This is considerably higher than the death-rate for operations on chronic gastric ulcer. As a matter of fact, the death-rate in this condition is *nil*, except in cases where rare accidents take place.

The average duration of life after gastro-enterostomy can hardly be predicted, but it is probably somewhere about six months. The longest time in any case I have had was two years, and the patient lived in absolute comfort for twenty-two months. This was a case in which a second operation was refused. I did a gastro-enterostomy thinking that I should be allowed to do a partial gastrectomy later. Another patient who refused a second operation lived fifteen months, and in absolute comfort for thirteen. It has been suggested that partial gastrectomy should be done in two stages. If you take my advice, never do it unless you are compelled; nothing is gained in the majority of cases, and when pyloric obstruction is present the patient may refuse further interference.

No loop should ever be left in performing gastro-jejunostomy. The operation of choice is the posterior one. For seven years I have been doing an operation which, I think, I can reasonably claim as my own. It is an anterior gastro-enterostomy without a loop. In one case in which I performed this operation I saw the result some years later. He had chronic gastric ulcer of the lesser curvature with so many adhesions that the posterior operation was impossible. He regained perfect health and lived for twenty-eight months. At the end of this time, when out of work, he cut his throat, and was admitted into the Poplar Hospital. I was informed by the pathologist that a posterior gastro-enterostomy had been done without a loop. At the end of two or three weeks after the operation it is quite impossible, unless you are acquainted with the anterior operation, to distinguish between the two. The anterior no-loop gastro-enterostomy is quite a simple operation, and takes no more time than a posterior gastro-enterostomy. It has only one disadvantage—it is difficult to get such a wide anastomosis as in posterior non-loop gastro-enterostomy—so that I never do an anterior non-loop gastro-enterostomy if a posterior is possible. An opening is made in the transverse meso-colon immediately over the duodeno-jejunal flexure; a loop of jejunum close to the flexure is brought through and united with the anterior surface of the stomach. When the operation is done the opening in the transverse meso-colon is sutured to the stomach around the anastomosis. I have done an anterior non-loop gastro-enterostomy in eighteen cases, and the results given in chronic gastric ulcer are equally as good as those in which a posterior non-loop gastro-enterostomy was performed. The dangers of a loop operation have to do first of all with peptic ulcer of the jejunum; secondly, with various forms of intestinal obstruction; and thirdly, the results as far as chronic gastric ulcer is concerned are not nearly as good as when a non-loop gastro-enterostomy is done.

October 24th, 1910.

A CLINICAL LECTURE

ON

INGUINAL HERNIA IN CHILDREN.

Delivered at the Hospital for Sick Children, Great Ormond Street, London.

By O. L. ADDISON, F.R.C.S.,
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THE consideration of hernia in infancy (that is, under the age of two years) practically limits itself to the question of inguinal hernia, other forms, such as diphragmatic hernia, congenital hernia of the umbilical cord and femoral hernia, being rarities. The small umbilical hernia, which is comparatively common, causes no particular symptoms, and requires as a rule no treatment other than restraint by pad and bandage.

Inguinal hernia in infancy is about ten times as common in boys as in girls ('G. O. S. Reports,' 1899–1909). It is more common on the right side but it is not infrequently bilateral, and is of two main types, the so-called "congenital" and the "infantile," both forms being truly congenital in origin.

In the "congenital" form the processus vaginalis remains patent, and into it the hernia descends, while in the "infantile," in addition to the processus vaginalis, a second peritoneal diverticulum is present, forming the sac of the hernia, and it is therefore obvious that in the infantile form various conditions of the processus vaginalis may co-exist with the hernia. Thus the processus may be sealed off above, and distended with fluid (infantile hydrocele), sealed off above, and below from the tunica vaginalis (encysted hydrocele of the cord), or may be patent throughout its length, in which case there are two potential hernial sacs present.

You will remember the formation of the processus vaginalis. Its descent is supposed to be due to the action of the gubernaculum, which consists of some strands of involuntary muscular fibres attached below to the pubes, perinæum and scrotum. Above, the gubernaculum is closely applied to the processus vaginalis, and above the testicle the fibres extend to a small fold in the posterior abdominal wall of the fœtus, the plica vascularis, from which the vessels of the testicle are later formed; above this again the gubernacular fibres extend to the

mesentery of the cæcum or iliac colon and disappear in the peritoneum lining the iliac fossæ.

The formation of an "infantile" hernial sac is supposed to be due to over-action of this gubernaculum, which pulls down a second sac of peritoneum from the floor of the iliac fossa. It has been found that traction on the gubernaculum in the fœtus with undescended testes causes not only the processus vaginalis and testes to move downwards, but also the peritoneum lining the posterior abdominal wall.

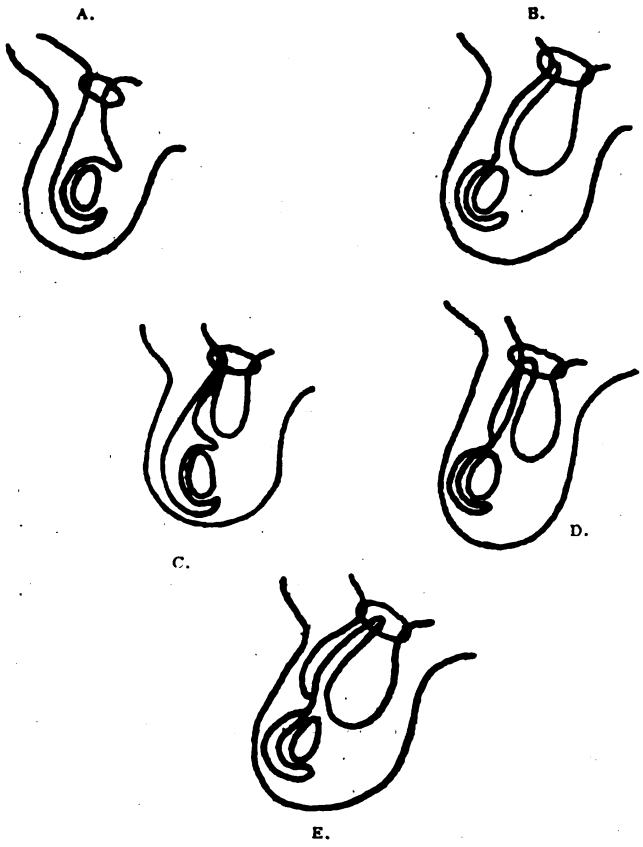
The "infantile" may be differentiated clinically from the "congenital" type of hernia by the fact that, while in the "infantile" the testicle can be distinctly felt below the hernia, in the "congenital" the hernia surrounds and masks it.

The contents are usually small intestine and omentum, but not infrequently the cæcum and appendix or pelvic colon are found. The bladder is not found within the sac of a hernia in an infant. I will mention later, when treating of complications, how the bladder does come into relation with a hernial sac in an infant, but an actual peritoneal covered process of bladder is not found, as is occasionally the case in an adult. The cæcum and appendix, however, are comparatively common contents, the appendix being recognisable by its shape. There is one other very common content of a hernia, but naturally in girls only, that is, the ovary.

The symptoms in a healthy child are usually slight, but if there is constipation or other intestinal disturbance the hernia is forced down, causing great pain and consequent screaming with further straining. The child is usually brought up for advice because there is a swelling in the groin, and the diagnosis as a rule is obvious. The only kind of case which causes difficulty is that of a small omental hernia, which may be difficult or impossible to distinguish from a hydrocele of the cord. Generally the shape and feel of the swelling and the presence or no of translucency if it is of any size will allow a definite diagnosis to be made. In the case of a large hydrocele extending into the inguinal canal the diagnosis may be difficult, and here especially translucency may be of great assistance, though in a child a hernia when distended with gas may be freely translucent; but in this case it will also be resonant and readily reducible.

The ovary in a hernial sac is said to be difficult to distinguish from a gland, but, if it be remembered that there is no gland lying over the external ring, and also that the ovary is more movable and more irregular in shape than a gland, this difficulty will not arise.

Treatment.—It is frequently said that these hernias require no treatment as they will "cure



A. Congenital hernia. B. Infantile hernia. C. Infantile hernia with infantile hydrocele. D. Infantile hernia with encysted hydrocele of cord. E. Congenital and infantile herniæ.

themselves," but this statement is absolutely incorrect.

Treatment is by two methods, truss and operation. Trusses are used for two purposes, either for the purpose of effecting a cure, or to restrain the hernia pending operation; if used for the former reason, treatment must be commenced before the age of three months.

Two kinds of truss are in use, the woollen and various forms of rubber truss. The woollen truss, though despised by many, is of considerable value, but must be carefully applied and is not suitable to all cases. It has the merit of cheapness, and also

does not chafe the skin—a point of no small importance. The truss consists simply of a skein of thick wool which is tied round at intervals of two or three inches with a single strand; a loop is thus left at either end. To apply as a truss pass the skein behind the child's back, bringing both ends to the front, below the iliac crests. If the hernia is on the right side, pass the left end through the loop on the child's right, and pull the truss tight, so that the point at which the loop now lies is over the right external ring. The free end is now passed beneath the perinæum, brought up over the buttock on the right side and tied to the band across the back. For the left side the procedure is reversed. It is important to remember that the loop should lie along the groin of the affected side and not at right angles to it, as in this way it is less liable to slip out of position. A woollen truss can also be made for a double hernia, but is not of much value.

Rubber trusses are of two main types, the horse-shoe pattern and the ordinary spring truss. They are more efficient than the woollen truss, and should be used if a preliminary trial of the simpler form is not successful.

The spring truss has two disadvantages, first, that it must be constantly renewed as the child grows, and secondly that it is difficult to regulate the amount of pressure.

Whatever form is used, cleanliness is absolutely essential. The child must not be allowed to get sodden with urine, or the skin will quickly become excoriated. A simple dusting powder, preferably containing a small proportion of tannic acid, is of value.

Two trusses should always be in use and one should always be worn, even when the child is being bathed. This should remain in place until the child has been dried, when it should be removed, the mother retaining the hernia with her finger until the dry truss has been applied. The most important point in truss treatment is the care of the general health of the child, neglect of this the commonest cause of failure. If the child is healthy and therefore contented, practically any truss will retain the majority of these hernias, but if the child is improperly, or irregularly fed, with the inevitable result that it suffers from gastritis, colic, wind, constipation or diarrhœa, with constant attacks of screaming, no truss which has yet been invented will prevent the hernia from coming down, so that the first and most essential fact to be impressed upon the mother is the necessity for suitable, and above all, regular feeding.

Circumcision is commonly considered necessary as a part of the treatment of hernia, indeed it is not infrequently the only treatment carried out. In reality, only a very small proportion of children have so tight a phimosis as to cause straining, and the operation is seldom necessary for this reason.

It must be remembered that, should the hernia be allowed to come down even once, it will prevent a cure as surely as if it were down constantly. The treatment outlined above must be strictly carried out for at least a year, and should a cure not have resulted in that time, it will not be obtained by this method, and operation should be resorted to, as further treatment by a spring truss causes weakness of the muscles, thus lessening the chances of a successful operation.

Operation is preferably postponed until the child is weaned, though should the hernia be uncontrollable it may be operated upon at any age.

The operation, never an easy one, is often extremely difficult, owing to the smallness of the parts, and delicacy of the tissues, especially the sac, which tears with the very greatest readiness, and naturally, the smaller the child the more difficult the operation.

Before operating it is most important to see that the child's general health is good. The child should be kept in bed for twenty-four hours or more, lying flat on its back, restrained by shoulder and ankle straps. Preliminary purging is unnecessary. The skin should be prepared in the usual way some eight or ten hours before the operation by a thorough washing with hot water and ether soap, all traces of soap, etc., being removed by ether, and the area covered over with a piece of sterilised lint, or a very weak (1 in 4 to 6000) compress of perchloride of mercury. A carbolic compress should on no account be used, as it is absorbed very readily indeed through the unbroken skin of an infant, and gives rise to carbolic poisoning. The child is again washed when on the table, and the operation area swabbed over with methylated spirit. The previous washing is an additional safeguard, but may be omitted. As an alternative method of preparation the skin may be painted with a 2 per cent solution of iodine in

rectified spirit an hour or two before the operation, and again on the table. If the iodine method of preparation is used, there must be no immediate preliminary washing of the skin, as this renders the epithelium sodden, and the iodine therefore is not absorbed.

The incision, two and a half to three inches in length, is made parallel to Poupart's ligament, the lower end of it being within half an inch of the root of the penis. An incision carried as low as this is necessary to give a thorough exposure of the external ring in a fat infant. Cut through the skin and superficial fat, which varies greatly in depth. One or two vessels require the application of forceps. Cut straight down on to the structures leaving the external ring, separate carefully and thoroughly the deep fascia from them and from the external oblique round the external ring. The deep fascia is often very dense, especially round the ring, and the fibres of it are easily mistaken for the tendon of the external oblique. If any difficulty is experienced, begin the dissection nearer the outer end of the incision where the glistening white fibres of the external oblique are easily recognised. Having freed the cord and its coverings carefully on either side, separate from the bone beneath. If the cord be now put on the stretch it will be seen that, although recognisable, the external ring is not sharply defined, the intercolumnar fibres being continued from it on to the cord. With the cord still on the stretch divide these fibres all round the external ring. Having done this, the cord can be readily freed as high as the internal ring, by passing a director or closed forceps into the canal. A longitudinal incision is then made through the coverings of the cord, or these may be torn through with forceps, until the sac, which is easily recognised, is reached. The sac is picked up, and its coverings peeled off; special care being taken in separating the vessels and vas, which are extremely closely applied to the sac, and are very easily damaged. This separation is the most difficult part of the operation owing to the thinness of the sac and the readiness with which it tears. Should this occur, insert a finger into the sac, thus defining it more clearly, and proceed carefully with the separation. When this is completed free the lower end if the sac is an "infantile" one, or, if "congenital," divide it between two pairs of artery forceps. Pull on the proximal end of the sac, and separate carefully as high as possible. At the inner side of the neck a process of subperitoneal fat is often found, and if of some size should be stripped off; the bladder also may be seen occasionally to the inner side of the neck appearing as a whitish thickened part of the parietal peritoneum. Before tying off the sac its interior must be examined to see that there is no adherent intestine or omentum. Then, still kept tightly on the stretch, the neck is transfixed as high up as possible with a needle and silk or catgut and securely tied; the sac is now cut off not too close to the ligature and the stump immediately retracts out of sight. It is always worth while to transfix the sac, as this obviates the possibility of the ligature slipping. One or two stitches are then put into the external ring, leaving an opening large enough to admit the tip of the little finger. No vessels require tying as a rule, and the wound is sewn up, preferably by a subcuticular suture, which, although taking rather longer to apply than the ordinary suture, is much easier to remove—a point of some importance. The essential feature of this operation is the tying off of the sac as close to the internal ring as possible, and an operation of this type is curative, anything more elaborate being unnecessary. Should, however, the external oblique be very much stretched, as is not infrequently the case, it may be divided, and subsequently overlapped. The wound is covered by a piece of gauze and Whitehead's varnish, which irritates the skin less than the collodion. A small pad and firm bandage may be used for twenty-four hours to prevent the possible formation of a hæmatoma, and the whole covered with a piece of mackintosh.

After the operation the child is kept flat on its back, and if restless is given potassium bromide or tincture of opium. The penis is fixed by a piece of strapping in a test-tube, the end of which has been broken off, the test-tube being led into a receptacle. By this means the dressings are kept dry. Feeding with milk and barley-water may be commenced in a few hours. The stitch is removed at the end of a week and the child allowed up on the tenth day.

The complications of hernia are few, and as a rule not of serious importance.

(1) *Hydrocele*, either of the vaginal type, or, more rarely, encysted hydrocele of the cord. These are as a rule easily diagnosed, and if necessary they may be tapped, or if a radical cure of hernia is to

be undertaken, may be dissected out at the same time.

(2) *Incomplete descent of the testicle* is not infrequent, the testicle being either in the canal or lying over the external ring. In these cases operation should be delayed, in the hope that the hernia may assist the descent of the testicle. Should the hernia be causing much pain, thus necessitating an operation, the testicle may be brought down at the same time.

(3) *Interstitial hernia* is an uncommon complication, usually associated with imperfect descent of the testicle, and the scrotal hernia is generally a small one. The varieties of interstitial hernia are three: pro-peritoneal, interstitial, and subcutaneous. The first and third are sufficiently explained by their names; the interstitial proper lies between the internal and external oblique muscles. The pro-peritoneal is not recognisable except at operation; the other two cause swellings above Poupart's ligament, and may be distinguished by pushing the finger up from the scrotum into the external ring; when in the subcutaneous form, the contents of the hernia may be felt as they leave the external ring. The treatment is operative.

(4) *Irreducible hernia* is rare, and almost always consists of omentum. In girls the ovary is not uncommonly irreducible.

(5) A very rare complication is appendicitis occurring in the hernial sac.

Considering the large number of hernias in infants containing the cæcum and appendix, or both, the complication of an acute attack of inflammation is less frequent than one would expect, for it would be reasonable to think that the appendix in such a situation, owing to the probable stagnation of its contents and its greater liability to injury, would be particularly prone to inflammation.

Should an abscess develop, it should be opened and drained.

(6) *Strangulation.*—A great many cases usually described as strangulated are not cases of true strangulation at all, but of what, for want of a better name, may be called pseudo-strangulation. These cases are very common. The child, during some act of straining, forces an extra piece of gut into the sac, with the result that the hernia becomes nipped at the external ring, and is made temporarily irreducible. This occurrence is associated with pain, vomiting, and often a certain amount of shock. By far the larger proportion of cases of "strangulation" really belong to this class. They differ from cases of true strangulation in that the shock and collapse are much less severe, the vomiting not so constant, and they may all be reduced without operation. The treatment consists in putting the child to bed on its back with the buttocks well raised. Potassium bromide is given in doses sufficient to keep the child quiet, and an ice bag is suspended so that it just rests upon the scrotum. In the course of a few hours the hernia will be found to be reduced; if not, chloroform should be given, and taxis applied. If this fail, it is clear that a true strangulation is present, and operation should be immediately performed. These cases when untreated as a rule pass on to true strangulation, though spontaneous reduction may occur.

True strangulation either follows on the preceding condition or may be present from the first. Strangulated bowel, though quite common in boys, is very rare in girls, but a strangulated ovary due to torsion of the pedicle is by no means infrequent. The signs and symptoms of strangulation are acute pain, shock and continuous vomiting, at first gastric, finally becoming stercoraceous. Sometimes a single action of the bowels occurs. The hernia is tense and tender, irreducible, and has no true expansile impulse, though a transmitted one from the abdominal wall is present. After about twenty-four hours the abdomen becomes distended and the scrotum red and œdematous.

Commonly the diagnosis is easy and obvious, though if œdema and redness are present the diagnosis from the infantile type of acute hydrocele, torsion or acute inflammation in the cord, or in a girl from an acute adenitis, may not be evident at first sight.

(*a*) From acute hydrocele the diagnosis cannot be made on the translucency of the swelling, for one of the first changes that takes place in strangulated hernia is that a considerable amount of fluid is poured out into the sac, making it freely translucent. The general symptoms are, however, much more severe in a case of a strangulated hernia.

(*b*) In acute inflammatory conditions of the testicle and cord the temperature is high and the child's condition not so grave.

(*c*) From acute adenitis.

It is important to remember that there is no gland lying over the external ring, also that an acutely inflamed gland is more fixed and is most unlikely to be solitary.

There is one method of examination by which a definite diagnosis can be made in every case, and that is a rectal examination, and it should never be omitted in any case of doubt. A forefinger thoroughly well coated with vaseline and very slowly inserted is very well tolerated by an infant. With one finger in the rectum, and the other hand on the anterior abdominal wall, it is perfectly easy to feel the internal ring on either side in any child under the age of two years, and in this way the two pieces of gut may be felt fixed in the ring, or in the case of a strangulated ovary, the uterus may be felt pulled up tight against it.

There is only one method of treatment—operation. Spinal anæsthesia should always be used: 3 per cent. stovaine, with 6 per cent. dextrin, and

·005 per cent. adrenalin in normal saline solution, as advocated by Mr. Tyrrell Gray, the late medical superintendent of this hospital, is the best solution, and as much as 3 cgrm. may be given to a child of two years. Mr. Gray has worked the whole matter out very thoroughly, and it is entirely due to him that the results obtained in these and other cases of acute surgical, especially abdominal emergencies have shown so great an improvement within the last two or three years at this hospital. Not only is shock prevented, but the child is free from the after-effects of a general anæsthetic which so largely contribute to a fatal result in these cases. Failing spinal anæsthesia, ether should be used in preference to chloroform.

An incision is made over the external ring extending down the scrotum, and a careful dissection made until the sac is reached. The sac is difficult to recognise, but an escape of fluid at once shows when it has been opened. Strangulation in every case occurs at the external ring, which is deeply buried beneath the swollen mass, and it is most easily exposed by following down the fibres of the external oblique. The sac being opened, a director is carefully passed beneath the ring, and the latter divided upon it. The bowel is then lifted out of the sac, pulled down and examined. The next step depends upon the condition of the gut, which should be replaced in the abdomen if not hopelessly damaged. Often it is advisable to wrap the intestine in gauze soaked in hot saline solution, and leave for ten minutes or longer to see whether recovery is likely to take place. If the bowel seems viable with the exception of one or two small areas, usually at the site of constriction, they may be invaginated with a row of Lembert sutures and the gut replaced in the abdomen; should, however, the lumen of the gut be too small to allow of any invagination, the damaged portion should be left against the internal ring, and a gauze drain led down to it with the hope that adhesions may form and seal off the inevitable fæcal fistula from the rest of the abdominal cavity. If there are more than small areas of gangrene present, there is little chance of recovery whatever form of treatment be adopted, as the condition of the child is probably desperate, though with spinal anæsthesia there is a chance of an enterectomy being successful. The operation is completed by ligature and removal of the sac, the closing of the cut in the external oblique, and suture of the wound. Every means must be taken to combat shock in these cases, and for this purpose saline solution, given subcutaneously or intra-venously, is of the greatest value. After the operation, rectal saline should be given continuously, and bromide or morphia may be used if necessary. Feeding with small quantities of albumen water or milk and barley-water should be begun within a few hours if there is no vomiting.

October 24th, 1910.

A DEMONSTRATION* OF PATHOLOGICAL SPECIMENS.

By ERNEST H. SHAW, M.R.C.S., L.R.C.P.,
Pathologist to the Great Northern Central Hospital.

GENTLEMEN,—I propose to show you a few specimens which I have put together at short notice and point out the lessons which they teach us. I show you first of all the lower end of a femur in which there is a blood-stained growth filling up the whole of the interior of the bone. The growth was soft when fresh, but not dilating the shaft to any extent, it is a typical endosteal growth of the lower end of the femur.

The usual type of growth in the ends of long bones is one in which there are many giant-cells; it is usually called myeloid sarcoma. It was thought at first that this growth was a myeloid sarcoma—a much less malignant type of growth than the round-celled or spindle-celled form. The first point which the surgeon had to consider, however, was, Was there an actual new growth in the bone, or was it a case of inflammatory thickening? I have the skiagram to show you. I cannot show you a skiagram which was taken before the limb was amputated. As I have said, it was thought that the growth was probably a myeloid sarcoma, and that therefore amputation through the middle of the thigh would be enough. If it proved to be spindle-celled, or round-celled, or mixed-celled sarcoma, it was considered to be much safer to take away the limb though the hip-joint. I went down to Croydon and received from the surgeon who exposed the femur and made a hole in its side a small portion of the growth which he removed; it showed under the microscope—for I made a section on the spot—a mixed-celled sarcoma. There was no doubt about its nature, it was not a myeloid sarcoma. It was a very malignant type of growth, and accordingly the whole of the limb was amputated at the hip-joint. This case is interesting from the clinical point of view with regard to the planning of the operation which was necessary, and also interesting from the pathological point of view in that it is an endosteal growth in the end of a long bone which is of mixed-celled variety and

* At the Polyclinic.

not a myeloid. It is often stated that all endosteal growths at the ends of long bones are myeloid sarcomata, but this is a very good exception to that rule.

Here is a specimen which shows very well a malignant stricture of the œsophagus affecting principally the upper end of the tube, that is to say the commencement. You see that the lumen is very much narrowed and the wall is thick, and there is an ulcerated growth. The growth extends a good distance downwards. This is one of the usual situations in which malignant disease of the œsophagus occurs. Malignant disease of the œsophagus is almost always of a carcinomatous type, it is usually the squamous-celled variety of carcinoma. The lining of the œsophagus consists of squamous epithelium, therefore you may conclude that a carcinoma arising in that situation is nearly always of the squamous-celled type. Now and then one meets with a carcinoma which is of the spheroidal-celled type. It is not known exactly why that is, but it is known that there are mucous glands sparsely scattered throughout the length of the œsophagus, and I think that these spheroidal-celled growths arise in those glands. At any rate this specimen shows very well the type of squamous-celled carcinoma at the upper end of the œsophagus causing a good deal of constriction of the lumen. The other situations in the œsophagus where malignant growths occur are opposite the level of the bifurcation of the trachea, and at the lower end. The most common situations are at the upper end and in the middle; according to some it is more common in the middle than it is at the top. It is certainly uncommon at the lower end. An interesting point about the growths at the lower end is their structure; sometimes they are found to be columnar-celled carcinomata. You have to remember that the growth is starting at the junction of a tube which is lined by squamous epithelium, and the stomach which is lined by columnar epithelium, and you can easily understand that the growth might start from the end of the œsophagus or the beginning of the stomach. That is a point to remember. This is an unusually extensive growth: most of the carcinomatous growths of the œsophagus are more or less sharply marked off, but this one has a very indefinite beginning and ending.

I have here two specimens which you see resemble one another slightly and yet in nature they are quite different. The first is a cæcum, which is occupied by a large growth. The growth is fungating into the lumen of the bowel and it had caused obstruction. It involves the region of the ileo-cæcal valve and spreads upwards to the colon. The growth is ulcerated in the centre and the edges are raised and nodular. These growths, when they are fresh, have a good deal of mucus on them. You must remember that all this mass is made up of columnar-celled glands, which are well formed and secrete a good deal of mucus, therefore there is much mucoid discharge from the surface. Another point to remember is that ulceration is going on at the same time, and therefore there are attacks of bleeding, but not much, just enough oozing to stain the contents of the bowel. Here is the second specimen, also a cæcum, from a case which I saw recently. The vermiform appendix is at the bottom of the specimen, and you can easily recognise it. The patient was operated upon a few weeks ago, when the surgeon found that there was a good deal of thickening of the appendix at its base and a hard mass inside the cæcum. The condition present was very suggestive of new growth, and he thought it would be wiser to remove the whole mass, which he did. On looking at the specimen after removal I found the cæcum was greatly inflamed, thickened, and œdematous, and on looking inside found a growth surrounding the orifice of the appendix; it formed a ring of raised tissue. But it was soft, and one could not tell by naked-eye inspection whether it was malignant growth or not. I had a section cut, including the raised piece here, and the base of the appendix. The condition proved to be inflammatory only. So you see it is a very good example showing the impossibility of making out from naked-eye appearances only whether a mass of tissue is a mass of new growth or whether it is inflammatory. You might, as surgeons, think it rather an extensive operation to remove the whole appendix and cæcum and portion of ileum for an inflammatory condition of the appendix. So it is, but appearances were all very much in favour of growth. The only way to have settled the diagnosis would have been to cut a microscopic section at the time, but unfortunately that was not possible in this case. At any rate the patient is all right now, and I do not think there will be any further trouble.

I have here a very good specimen of epithelioma —that is to say, squamous-celled carcinoma—of the tongue. You see the right half of the tongue and a mass sprouting from its surface; on looking at it closely the growth is seen to extend a good deal towards the lateral aspect of the tongue; it is somewhat raised and presents a definite edge. A piece of this raised portion was cut off and microscopically proved to be typical squamous-celled carcinoma. The specimen shows very well the white, infiltrating growth in the darker coloured muscle.

Here is another intestinal condition. It is an ordinary columnar-celled carcinoma of the rectum, and it is of exactly the same character as the growth which you saw in the cæcum—a fungating nodular mass of growth. It shows very well the manner in which the growth extends round the lumen of the bowel. Before the bowel was cut open there was a definite stricture of the rectum.

I have here specimens showing typical uterine fibroids. In the first you can see a large uterus on the outside, with one of the Fallopian tubes and an ovary attached to it. You see the fibroids in the wall of the uterus; they are composed of fibrous tissue and unstriped muscle, *i. e.* fibro-myomata—an example of innocent tumour. Here is another specimen, which is not so extensively involved, showing the cavity of the uterus and a fibroid projecting into it. It shows very well the way in which some fibroids are pushed towards the cavity of the uterus. Over the fibroid you can see the dark, congested mucous membrane. It also shows you how hæmorrhage is brought about in these cases. First there is an inflammatory thickening of the mucous membrane over the tumour, followed later by thinning and ulceration. Later the fibroid is completely exposed. While this change is going on hæmorrhage occurs from the congested mucous membrane—a well-marked feature in the signs of this disease. It would be very easy to imagine, carrying the process still further, that after the fibroid has ulcerated the mucous membrane over it, it becomes pushed further and further into the cavity of the uterus, and in time forms one of the pedunculated fibroids, hanging by a definite stalk into the cavity of the uterus, cervix, or vagina. Later on the stalk may give way and the fibroid be discharged—a very rare occurrence. You know that one of the types of polypi of the uterus is formed by one of these fibroids hanging down into its cavity. The earlier sign is hæmorrhage, and later on, if the polypus gets further down, there is a chance of septic contamination, followed by purulent discharge from the vagina. In addition to that change occurring in fibroids we have sometimes a change which goes by the name of necrobiosis. Here is a specimen illustrating that condition. You do not often see these specimens. In such a case the blood-supply becomes cut off from the tumour, which gradually dies, but there is no infection by micro-organisms, therefore you do not find a sloughing gangrenous mass; a lot of blood remains in the tumour, which appears as a dark red, firm mass. This used to be considered an example of pure myoma of the uterus. All fibroids of the uterus contain a good deal of fibrous tissue in addition to unstriped muscle, some more some less. This tumour caused a good deal of pain, and the patient's temperature was raised. The patient was seen by Mr. Mower White at the Great Northern Hospital and operated upon by him. It was not known exactly what was wrong before the operation. He found this big tumour in the wall of the uterus, and was able to cut through the superficial layers of the uterine wall and completely enucleate the mass. It is a very good example illustrating the operation of myomectomy.

I have brought a selection, not complete by any means, of some of the various kinds of tumour of the breast. Several of them are rather interesting as being outside the ordinary run of tumour. First I will show you an ordinary type. Here is a portion of a breast showing a dark mass of growth in its substance, continuous by a narrow strip of growth with the nipple. It is a carcinoma of the substance of the breast, and the growth is also involving the region of the ducts; it passes up towards the nipple, and is at the same time causing definite retraction of that structure. You also see the pectoral muscle and a large amount of fat. There is very little real breast-tissue left; this is a common occurrence in the large breasts of elderly women—even in those breasts which are healthy you may cut a section like this and find only little areas of real breast tissue dotted about in the fat. A portion of breast tissue has in this case become malignant, and increased in amount to form a mass involving the ducts and nipple. I think there is also some growth in the skin.

Here is an example of hard or "scirrhous" carcinoma; the cells of this type of growth may be round, columnar, or mixed round and columnar. Usually most of the cells are round, or, as many people describe them, spheroidal; but if you cut sections large enough, or from various parts of the same tumour, you will find in the majority of them places where the cells are short columnar in shape.

The next specimen shows the same sort of thing, a mass of growth and fat, and I think it also shows to a slight degree that feature which you see in these tumours when they are fresh. When a section is made through the growth it is seen that the tumour shrinks and the fat around it bulges, and so you get that concave appearance which is so typical of hard carcinoma of the breast. The other specimen was cut through after it was hardened in formalin, and does not show this concave appearance. On cutting through these tumours they give that peculiar firm impression which is obtained on cutting through an unripe pear or a raw potato. The hardness of these growths is due to the presence of a large amount of fibrous tissue.

Here is another specimen in which a carcinoma is involving the mamma in the same way, and with it a growth involving the lymphatic glands of the axilla.

In the next specimen the nipple is flat; we will not say it is retracted. Below it is a mass of growth which has involved the nipple and also ulcerated through the skin. Clinically one felt a hard mass of growth in the tissues beneath the skin. On making a section after removal it was found that practically the whole of the breast was involved by a hard mass of carcinoma. This is the most common type of growth seen in the breast, and therefore I think one cannot see too many examples of it. I want to tell you about the case from which this specimen was obtained. The breast was removed from a young woman by Mr. Mower White, whom I have to thank for many of these specimens. The patient was only æt. 32 years. She had first noticed a lump in the breast four months previous to the operation. When she came to see Mr. White he found this undoubted growth in the breast. At the operation he could not get it all away, and so had to remove as much of the affected tissues as he could. The growth was adherent to the deeper structures. The most sad part about the case is that the patient has come back within two months of the operation with the skin of the chest-wall and scar affected with many small nodules of carcinoma. So it is presumably a hopeless case as regards ultimate cure. But she is being treated by Dr. Knox by means of X rays in the hope that the growth may be arrested. The case, therefore, illustrates the extreme malignancy of carcinoma when it occurs in a young person. This patient is young as such cases go. You may see patients with this condition below thirty years of age. I think the youngest I have seen was æt. 25 years. At any rate I think cases under thirty-six or thirty-seven years are not common. More often these women are over forty years. I think you will agree, from the history which I have given you, that this case is an extremely malignant one, and I want to point out to you the malignancy of carcinoma when it occurs in young people. I wish I had here to show you the tongue from another case which we had at the Great Northern Hospital last year, a tongue with a large mass of growth which involved also the floor of the mouth, the jaw and the glands in both sides of the neck in a man man æt. 27 years. It was a hopeless case from the start. Nothing could be done for it, and when the man died we found secondary growths in various organs of his body, including those of the chest and abdomen, and also a secondary mass in the upper end of the femur. Every now and then one sees these cases of carcinoma in young people, and when you do see them you should give a bad prognosis.

There is a variation in the type of growth occurring in the breast—colloid carcinoma, of which I have a section here. A mass of growth lies immediately beneath the skin, and is adherent to it. All this mass to one side of the breast represents axillary fat and involved glands. There is another mass of firmer growth in the substance of the breast. The specimen was obtained from a woman, æt. 56 years, who had a soft growth over the nipple, which felt like a cyst; several of us who saw the case diagnosed a cyst. That, of course, is a common mistake. Further over we could feel a hard mass. We thought the condition present was a cyst, such as occurs in chronic mastitis, together with a malignant growth involving the breast tissue. As a matter of fact both masses proved to be malignant. The growth below was

very hard, and of the "scirrhous" type; the growth above was soft and gelatinous, and hæmorrhage had occurred into it. Just a word about these colloid growths. It is sometimes stated that colloid carcinoma is not so malignant as the other forms. From what I have seen, however, I should say it is quite as malignant, judging from the number of secondary growths and the ultimate result to the patient. The only difference I can make out is that the course is longer; that is to say, where an ordinary carcinoma would kill in eighteen months, these cases go on for five or six years. But I have found that patients with this kind of growth die ultimately of the disease, with recurrences and secondary growths in other parts.

Next, I show you a cyst of the breast, with a fairly thick fibrous wall, which is smooth on its inner surface. It is surrounded by hard breast tissue, and is a very good example of the form of retention cyst which occurs in connection with chronic mastitis. These cysts have sometimes been called serous cysts. It has been the practice of some to divide these cysts of the breast into serous cysts and retention cysts. The term "serous cyst" was applied on the supposition that they were cysts formed by dilatation of the lymphatic spaces of the breast. As a matter of fact, one knows from a study of microscopic sections of the cyst-wall that they are always due to retention of fluid in dilated ducts or acini. First, chronic inflammation of the breast occurs, then the duct becomes involved and its lumen occluded, secretion goes on in the acini, the fluid cannot escape from the duct, and a cyst is formed. Sometimes they are single, more often they are multiple. They are always surrounded by inflammatory breast-tissue. Sometimes the breast is completely riddled with cysts. If that is the case, or the cyst is somewhere near the surface, it is not so difficult clinically to make a correct diagnosis of cyst. But very often the cyst occurs deep down in a large hard breast and forms a mass which feels almost like a stone in hardness. In such a case you may delude yourself by saying you can feel fluctuation. You can obtain a sense of fluctuation in many tumours in the breast, and afterwards, when the mass is exposed or removed, you will find that it is a soft fibro-adenoma, or perhaps carcinoma. It is all a matter of guesswork, I mean in regard to tumours which are situated deeply in the breast. I see all the breast cases I can, and endeavour to make a correct diagnosis by means of physical signs, and time after time, like other people, I am caught. You may get all the physical signs which accompany a cyst, *i. e.* a rounded mass, which is tense, indentable, and fluctuating. You may turn out to be right in your diagnosis, or, on the other hand, you may turn out to be wrong. I am laying stress on this because it is important, and is a condition in which I am very interested. I see a good many cases in which the diagnosis is confirmed by operation, and also many in which the opposite is true.

Here I show you a larger cyst, with a smooth wall, somewhat crinkled. It is a thick, fibrous wall, and it is surrounded by a thin layer of breast-tissue. At the bottom of the cyst you can see a dark reddish mass. It is a very good specimen, and illustrates an important point in connection with these cysts. It is a retention cyst which contained dark-coloured fluid under high pressure, causing it to feel almost solid. The surgeon who removed the breast could not tell beforehand whether the tumour was solid or whether it was cystic. It was cut into and the dark-coloured fluid escaped. "Sometimes the fluid is thin, sometimes it is thick." When the fluid came out we were very much relieved, for it showed the tumour to be a cyst, and presumably innocent in nature. But after getting all the fluid out and looking at the cyst more carefully we found there was a mass of growth at the bottom of the cavity. A piece of this growth was taken out and a microscopic section prepared at once. It was found to be carcinoma. The sequence of events in this case was probably as follows: First, chronic mastitis, followed by the formation of the cyst; then in the chronically inflamed breast-tissue, just outside the wall of the cyst, malignant disease arose. On this finding the breast and glands were removed together with the pectoral muscles and fascia. If one had been satisfied with doing what has been recommended by some, namely, sticking a trocar into the cyst, and on finding fluid concluded that it was simply a cyst and nothing else, that solid carcinomatous mass at the bottom would not have been found. I would strongly recommend that arrangements be made at all operations on tumours of the breast for a microscopic section to be prepared during the operation, while the patient is

anæsthetised, so that the surgeon may proceed to clear out the axillary glands, or not, with the one anæsthetic. I have seen breasts which looked like, and cut through like, chronically inflamed breasts, and yet a microscopic section showed that they were carcinomatous. The reverse has also often happened. More often than not the cases which clinically appear to be chronically inflamed breasts turn out to be carcinoma. Every now and then, unless the precaution is taken to have a microscopic section made, you will remove a breast which does not prove to he carcinoma, and then you will feel very annoyed for having done such a big operation for a comparatively simple condition.

Here is another piece of breast, in which it was very difficult to make out, on cutting through it—difficult to make out with the naked eye I mean—whether it was a mass of chronically inflamed breast or whether it was commencing carcinoma. It turned out to be one of those cases which are described as duct carcinomata. There is a fallacy held by many about the nature of these growths, namely, that they are not so malignant as the other type. I do not think myself that there is any difference; they are quite as malignant as the other forms of carcinoma. They are usually described as columnar-celled carcinomata growing in the ducts. But they are not always columnar-celled. Many of them are of this type, but there are others which contain large spaces filled with round cells, and in other parts of the same section definite tubes lined by columnar epithelium. I should say that such a breast ought to be completely removed, just as in an ordinary case of carcinoma, because it is carcinoma.

Both the breasts I show you next were removed from one case at the same time. It was found that the hard masses found clinically—there were several of them—showed early carcinoma, and therefore a complete operation was done. In the other breast were found some indurated nodules, but no definite carcinoma, but there were some of those little papillary growths which one finds now and then in small, almost microscopic, cysts and attached to the inner surface of the wall by a pedicle. That is a fairly common condition to find in connection with chronic mastitis. You find inflamed acini and ducts, with an overgrowth of epithelium; this may arise from the wall all round or from one side only, together with a stalk of connective tissue, thus forming a pedunculated tumour. These tumours are most commonly seen just by the nipple, where the main ducts are found. They sometimes form fair-sized cysts, and in most cases there is bleeding into the cyst, followed by a blood-stained discharge from the nipple. If you find in any part of the breast a hard lump containing small papillary growths the chances are that there are others in some other part of the same breast. I have found that out by cutting into many of these breasts. A part of the breast becomes hard as a result of chronic mastitis, and in this there are cysts containing pedunculated growths. This part has been exposed and cut out and microscopic section made; on these revealing small papillomatous growths the entire breast has been removed. On cutting up the breast I have found these pedunculated growths in other parts. I have seen them quite an inch long growing along a duct and filling its lumen; my opinion is that it is not necessary to do more than amputate the whole breast in such cases. It is not enough, however, to merely remove the quadrant containing the affected mass. It may be sufficient, but the chances are that there are other tiny growths present, and that these will continue to grow; in that event you will have to do another operation. There is a still more serious possibility, namely, the formation of definite carcinoma.

The last specimen I have here is half a tumour of the breast; it imitates very well the shape of this organ. The patient from whom the specimen was obtained was a young girl, æt. 16 years, who had had a large breast nearly all her life which latterly had increased in size. It was thought better to remove it. You see it is not a simply hypertrophied breast; it is a fibro-adenoma which during its growth has kept the shape of the breast. The original breast-tissue has not developed; the remains of it are seen as a thin band on the outer side. I have seen this condition four or five times. It is interesting clinically also, because you may see a patient with a very big breast; perhaps both breasts may be affected in the same way. The breast has been enlarged perhaps from infancy, and you may think it is an example of pure hypertrophy of the breast. That condition, of course, does occur, for one has seen examples of pure hypertrophy of the breast, but most of the cases which clinically were apparently of that nature have turned out to be, not hypertrophied breasts, but fibro-adenomata. They form definite encapsulated tumours and are innocent in nature. This tumour was removed by Mr. Mower White last year. He also removed another one a few weeks ago, which, however, I could not find in time for to-day's demonstration. The patient was a woman, æt. 26 years, who was rather tired of carrying such a big breast about, and it was getting larger. When Mr. White exposed the tumour he found the breast-tissue spread out over it; the tumour was easily enucleated and removed. The patient had had children and suckled them from this side, so that the breast-tissue which was present was probably of very efficient quality.

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* *Specially reported for the Clinical Journal. Revised by the Author.*

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A LECTURE

ON

THE COMPLICATIONS OF GALL-STONES.

Delivered at Guy's Hospital.

By C. H. FAGGE, F.R.C.S.,
Surgeon to the Hospital.

GENTLEMEN,—The subject of the complications of gall-stones is so large that I propose to deal only with some of those conditions of which examples have come under my notice during the past five years, and in reading you accounts of typical cases I shall discuss their treatment. These concretions formed usually in the gall-bladder for some unknown reason, often reach a large size without giving rise to any symptoms, and therefore are often discovered at an operation when they are quite unexpected. There is at present in Esther Ward, under the care of Dr. Pitt, a middle-aged woman, who was operated upon a week to-day for early peritonitis, which was thought to be of appendicular origin. The condition of the appendix seemed one of such early inflammation that it might not account for the local peritonitis, so the abdominal cavity was rapidly explored and the only other abnormality discovered was a gall-bladder full of stones. On closely questioning the patient afterwards there was nothing in the history which would suggest that these four or five large stones completely filling her gall-bladder had ever given any symptoms of their presence.

Commonly the only symptom of gall-stones in the gall-bladder is irregular intermittent pain, which is most often seen with the large stones which lie freely in the gall-bladder, of which the following case is a good example. She was a lady, æt. 60 years, under the care of Dr. Curtis, of Beckenham, on whom I operated last September.

She complained of occasional intermittent pain in the right hypochondrium for some years, but for the last three months it had been constant: at times the pain was most acute, shooting up into the right shoulder, into the right ribs and round to the back on the right side: it had no relation to food, though she suffered from loss of appetite and had lost much weight: this loss of weight in a woman æt. 60 years naturally made us suspicious of malignant disease. There was extreme tenderness in the right hypochondrium, and her liver projected three inches below the costal margin, but at the operation this proved to be a ptosis rather than a true enlargement of the liver, and other organs, especially the right kidney, were also unduly movable. She had a well-marked hydrops of the gall-bladder, though it could not be felt through the abdominal wall: the gall-bladder was six inches long, and contained eight fair-sized stones, and the usual clear viscid fluid which is met with in cases of distended gall-bladder in which the cystic duct is obstructed. The gall-bladder was drained by inserting a large rubber tube after its wall had been sutured to the parietal peritoneum. Since then she has been quite free from pain.

Smaller stones work their way down the cystic duct and give rise to the typical colic which is evidence of muscular spasm from blockage of the duct and retained secretion (Kehr): this colicky pain, with irregular attacks of dyspepsia, are the chief symptoms, but transient bouts of slight jaundice are frequently met with, even when the stones only occupy the cystic duct and when no stone can be found at an operation in the common bile-duct. I think this is important and not generally recognised. Jaundice in these cases is no doubt due to one stone becoming impacted in the cystic duct and setting up catarrhal inflammation of the duct, which spreads along the mucous membrane into the common bile-duct as a secondary cholangitis, and does not necessarily indicate that the common duct is mechanically obstructed by a stone.

That this was the correct explanation of transient jaundice in the next case I shall relate to you was clearly proved at the subsequent operation; though he had been jaundiced for three weeks there was no stone in the common bile-duct, but in another case, taking these symptoms as probably due simply to catarrhal cholangitis, I overlooked a movable stone in the common bile-duct: I will give you the details of this second case later. The first case was that of a doctor, whom I saw in 1908 with Dr. Lauriston Shaw. He gave a history of dyspepsia and slight attacks of abdominal pain for several years. In 1907 he was jaundiced for a week: on November 3rd, 1908, he had acute pain in the right hypochondrium, which continued until the 15th, and during this time he again became jaundiced; at times the pain was severe enough to demand morphia. When I saw him on November 30th he had no pain, but was obviously jaundiced: the gall-bladder and liver were not palpable. Dr. Shaw independently made the diagnosis of a gall-stone blocking the cystic duct, and advised operation. Four days later I exposed a small, thick gall-bladder, adherent to the omentum and duodenum. I carried out the same operation as in the first case—cholecystotomy—and evacuated a dozen stones in an ounce of bile.

In contrast to the first case, the presence of bile in the gall-bladder here showed that at the time of operation—as one would expect by the cessation of the pain and by slight diminution of the jaundice—the blockage of the cystic duct had subsided, for had the cystic duct been blocked there would have been clear mucus in the gall-bladder, whereas it contained a normal amount of bile-stained fluid. Three of these stones were located quite far down in the cystic duct, and were milked upwards into the gall-bladder and extracted; these were, I think, the cause of the spreading cholangitis which gave rise to the jaundice. Even after the operation we had some qualms as to whether there still might be a stone in his common duct, for the jaundice continued for nearly a fortnight, but then gradually cleared up, and the fistula produced by the drainage-tube through the parietes was closed in four weeks. The early closure of the cholecystotomy wound was a very strong argument against the presence of a stone in the common duct, for it is a common clinical observation capable of obvious mechanical explanation that external fistulæ of mucous tracts do not close when stenosis persists further down. From November, 1908, to the present time I have constantly seen the patient, and he has been in perfect health.

The smaller stones may become impacted in the cystic duct, and then the colic is very marked

indeed and cholecystitis results. This acute inflammation may go on so far as to give rise to phlegmonous cystitis, ending in gangrene of the gall-bladder. Sometimes in patients who complain of no acute symptoms the condition of the gall-bladder is acute, and the gall-bladder contains a large amount of pus. In September, 1908, a man, æt. 68 years, was admitted into Bright with an attack of gall-stones. He had had an attack of colic with jaundice in Australia in 1907, and while on the way home he had a second attack, in which gall-stones were diagnosed. When I saw him he was tender in the right hypochondrium, but he did not seem to have any acute lesion. His temperature and pulse were normal, and he had not the aspect of one suffering from an acute suppurative focus. The operation was rendered somewhat difficult owing to widespreading peri-cholecystitis, and a trocar introduced into it evacuated an ounce of pus, followed by clear mucus, which is so characteristic of cystic duct obstruction. Four stones were removed, one of which was firmly impacted in the cystic duct. In about eight weeks the fistula was entirely closed and the wound healed.

If impaction is firm and unrelieved, inflammation spreads to the peritoneum and sets up local acute peritonitis. It is often difficult to correctly infer that gall-stones are the cause of such a condition and to exclude the other causes of peritonitis in the right side of the abdomen.

Thus, in April, 1907, Dr. Hale White asked me to see a patient of Dr. Guy Wood, a lady, æt. 65 years, who had been ill for two weeks with right-sided abdominal pain. This was associated with extreme tenderness, making a thorough examination and an accurate diagnosis impossible: her temperature for three days had been about 103° F., and her pulse was 100. Having arrived at the provisional diagnosis of local peritonitis probably due to appendicitis, she was given an anæsthetic, and only then was it easy to make out a rounded lump below the liver under the right costal margin, whereas the region of the appendix was soft and supple, without the usual obvious signs of inflammation in its neighbourhood. This led to the incision being made in the right position, exposing the gall-bladder, which was adherent to the abdominal wall, liver, and omentum; there was some free fluid in the peritoneal cavity and lymph on the omentum, showing that the local reaction was indeed acute. The gall-bladder was inflamed and very tense, and after packing it off it was incised. Clear fluid and then pus, not bile-stained, was evacuated, and a single date-shaped stone was found blocking the cystic duct. The gall-bladder was drained, and the patient recovered from this acute cholecystitis but died a month later, and three days after a second operation for an acute infection of a long-standing left pyonephrosis.

Another acute case, which again was interesting on account of the difficulty in accurate diagnosis, was a man, æt. 38 years, who was admitted into Stephen Ward for abdominal pain and vomiting in July, 1905. His temperature was 98·4° F. and the pulse 106. He was very tender in the right hypochondrium and epigastrium over two small areas, which were quite definite. Two days after admission he was seen by one of the assistant physicians and his corresponding assistant surgeon, who, excluding a suppurating gall-bladder because of the absence of previous symptoms, diagnosed a perforation of a duodenal ulcer, with localised peritonitis. Late the same evening I opened his gall-bladder and evacuated sixty-one gall-stones and several ounces of pus from a large, distended, and adherent gall-bladder. I must relate to you the further history of the case because it must be almost unique, and raises the question of the association between gall-stones and malignant disease, on which I shall touch later. The fistulous opening connecting his gall-bladder to the skin having healed, he attended my Out-patients regularly for some months. In December, 1906—eighteen months after the cholecystotomy—he came to see me with a curious depressed hardness in the centre of the old scar. He was advised, on the diagnosis that this was epitheliomatous, to come into the hospital, but he failed to put in an appearance for some six weeks. At the end of January, 1907, I operated on this evidently malignant growth in the abdominal wall, and included in a large ellipse of the abdominal wall the lower margin of the right costal cartilages, a slice of the liver, and a portion of the anterior surface of the stomach, which were adherent to the growth. On the posterior aspect of the parts removed was the whole of the gall-bladder. On examining this specimen it was clear that the growth had started in the fundus of the gall-bladder, and the Registrar reported that microscopical sections showed it to be a columnar-celled

carcinoma. Two or three weeks later, when he was under an anæsthetic for the purpose of suturing the large defect left by this first operation, an enlarged axillary gland was removed, and this showed a similar deposit of columnar carcinoma. The problem for us is: Was it originally a case of malignant growth in the gall-bladder which suppurated, or was the incidence of malignancy in the fundus of the gall-bladder in any way determined by the previous operation and the suturing of the fundus of the gall-bladder to the parietes? Against the former hypothesis was the long period of good health after the first operation, and the fact that the gall-bladder was thinned and showed no evidence of growth; again, it is unusual to find malignant growths at the fundus of the gall-bladder. My view is that there was no growth at the time of the first operation, and that it was in some way determined by the suturing of the gall-bladder to the parietes.

I have quoted five cases in which cholecystotomy has been performed for gall-stones; and in six other similar cases I have performed the same operation: I have also done it several times for the temporary relief of malignant disease obstructing the common bile-duct. The only non-malignant case in which I have performed the alternative operation of cholecystectomy—removal of the gall-bladder, as opposed to its temporary drainage—was for a thickened and contracted gall-bladder. The diagnosis of this case was so interesting, and the report is so full of sound clinical reasoning that I will quote it rather fully to you: Caroline H—æt. 45 years, was admitted under Dr. Hale White on September 14th, 1908, for pain under the right costal margin. She had a very severe attack of pain in the same region in 1904, which was accompanied by vomiting for twenty-four hours. After this she was said to have passed a gall-stone. Until Christmas, 1907, she remained well; but for the nine months preceding admission she had paroxysmal pain in the right hypochondrium, which she definitely localised to one spot over the gall-bladder. It sometimes radiated to the right shoulder, and over the area on the abdomen, where she said the pain occurred, there was distinct tenderness on deep palpation. She occasionally vomited when the pain was acute, but she had never been jaundiced: she thought she had lost a little flesh. The ward clerk's report goes on: "Dr. Hale White discussed gall-stones, duodenal ulcer, and movable kidney, and decided on the diagnosis of the first. He advised operation because, firstly, in her present condition the patient cannot work, and she is dependent on her own exertions for her living. Secondly, 20 per cent. of gall-stones cases were followed by malignant disease of the gall-bladder, and 4 per cent. by cholangitis, cholecystitis, or perforation." Would not the reports be more interesting if the opinions expressed at the bedside were more fully recorded, even allowing for the natural inaccuracies of their reporting? On September 21st, 1908, I operated, and found a distended and thickened gall-bladder, with the cystic duct dilated above and narrowly stenosed at its lower end: the gall-bladder and dilated duct were excised, and the right kidney-pouch was drained. She subsequently did well and was relieved of her pain. The ward-clerk goes on to say: "Dr. Hale White explained the condition found at the operation by suggesting that a gall-stone passing out of the gall-bladder caused ulceration of the cystic duct, which ultimately led to stenosis, and that this might all be accounted for by the gall-stone which she passed in her initial attack four years previously."

At the present time cholecystotomy—temporary drainage of the gall-bladder for gall-stones in the cystic duct or gall-bladder—has largely regained the position it formerly held, from which it was for a time ousted in favour of cholecystectomy. Some of the earliest gall-bladder operations I remember as a student, when cholecystotomy was the routine operation, were followed with undue frequency by the persistence of mucous or biliary fistulæ, probably owing to the fact that stones were left behind in the cystic or common bile-duct, and the natural passages not being patent, the gall-bladder fistula did not close. Again, some of these patients required a second operation for removal of a painful, thickened, adherent gall-bladder; so it came about that for a time primary cholecystectomy held the field. All those with large experience of cholecystotomy agree that recurrence of gall-stones after drainage is so rare as to be negligible and with regard to its risks, Mayo Robson* states that from 3000 cholecystotomies performed by Mayo, Kehr, and himself the mortality has

* 'Burghard's System of Operative Surgery,' vol. iii, 1909, p. 123.

been 1·8 per cent. in non-malignant cases, as compared with about 5 per cent. after cholecystectomy. Mayo Robson gives as the commoner indications for cholecystectomy—gangrenous and phlegmonous cholecystitis, a dilated gall-bladder, the presence of a mucous fistula opening on to the surface, from stricture of the cystic duct, or lastly a localised malignant growth of the gall-bladder. But for an ordinary case, associated with no marked changes in the gall-bladder, cholecystotomy, though it necessitates the persistence of a biliary fistula for a few weeks, is followed by no evil results, and, as stated above, has a very low mortality. Mayo Robson, from a large personal experience, and reviewing the work of others, points out that recurrence of gall-stones after cholecystotomy is practically unknown. And Moynihan* says it is very much open to question whether so-called recurrences are not cases of persistence, where gall-stones have been overlooked and left behind at the first operation. I shall quote you a case in which a gall-stone was overlooked by me, and this case might have been regarded as one in which recurrence took place. I have several times drawn your attention to the fact that the biliary fistula was closed in so many weeks, because dressers ask, How long will bile continue to escape? As a rule after cholecystotomy bile drains away for from ten to twenty-one days and the fistula takes six weeks to heal. In one case I quoted the fistula was closed in four weeks, and the longest time in those I have quoted was eight weeks.

Stones in the Common Bile-duct.

It is generally agreed that a stone in the common duct has been formed in the gall-bladder, and has passed down the cystic duct into the common bile-duct, where, as it is forced on towards the duodenum, it gives rise to a continuance of colic and sets up typical cholangitis. The severity of the constitutional symptoms will depend on the size of the stone and the nature and virulence of the subsequent infection. The strongest indication for drainage in the treatment of all cases of obstructive jaundice is that, while normal bile in normal ducts is sterile, in cases of obstruction of the ducts the bile is found to contain some virulent organism—it may be the *Bacillus coli*, or any other organism which at the time inhabits the duodenum or blood-stream—and this infection calls for temporary drainage, so as to obviate the risk of leakage of septic bile into the peritoneum and the setting up of a virulent peritonitis.

My first case of stone in the common bile-duct was of a very mild type: Annie B—, æt. 60 years, was admitted under Mr. Lane in June, 1909, on account of recurrent attacks of pain across the upper part of the abdomen, which was most severe in the right hypochondrium. She has had similar attacks for eight years, which are at times accompanied by jaundice and last three or four days, and on passing off leave a spot of tenderness in the right hypochondrium. The attacks are slowly increasing in severity. The last attack occurred six weeks ago, and she now has no pain or tenderness. She was operated upon by me during Mr. Lane's absence and a single facetted stone was found in the common bile-duct, just above the first part of the duodenum. It was freely movable, so that bile probably got past it easily, and consequently there was no persistent jaundice. The fact that the stone was facetted suggests that other stones had been present, but none could be detected on probing the ducts both upwards and downwards. A small incision into the common bile-duct, where it lies in front of the foramen of Winslow, let the stone out, and a long drainage-tube was passed upwards into the common bile-duct and held in position by two sutures, the incision in the duct being otherwise closed by a continuous catgut suture; at the same time the gall-bladder was drained. Forty days later she was discharged, the wound being healed. Recently Dr. Joyce, of Cranbrook, wrote to say she is quite well, and that the operation may be regarded as perfectly successful.

A similar case was that of Emma R—, æt. 66 years, admitted in February of this year into Esther for colic and jaundice. Her illness began with a bad attack of biliousness a year ago, which lasted two or three days, and has recurred at intervals of four or five weeks, associated with profuse vomiting and shivering fits; this was clearly a more virulent infection than in the last case. She has been twice jaundiced, and was admitted into Esther under Dr. Pitt in 1909, and refused operation, though she had been told she had a gall-stone in her common bile-duct, and that probably operation alone would relieve her. On admission she was slightly jaun-

* 'Abdominal Operations,' 1906.

diced, there was no pain, and nothing abnormal could be felt in the abdomen. On February 28th of this year her abdomen was opened, and a stone was at once felt in the common bile-duct, just above the first part of the duodenum. The peritoneal cavity was packed off, the common bile-duct incised, and a single stone removed, which the ward-clerk in his report says was the size of the last segment of a man's thumb: the common bile-duct was drained, but in this case no drainage-tube was inserted into the gall-bladder. Twenty-three days later the wound was healed, and she was discharged a month after the operation.

You may have noticed that the type of patient who suffers from gall-stones is often a woman who is "fat and fifty," and this is fortunate for the operator, for there is no doubt that in men the subphrenic region is more difficult to deal with than in women, owing to the anatomical differences of the lower costal arch in the two sexes. By placing a kidney pillow under the back in the lower dorsal region the bile-ducts are more easily exposed, as the lower ribs are thrown forwards and the lower costal arch is widened out: the position so obtained is sometimes known as the reversed Trendelenberg position; but for thoroughly exposing the gall-bladder, the cystic duct, the common bile and hepatic ducts, rotation upwards of the liver, which was first suggested by Moynihan, is of the greatest value, for by this means the whole biliary tract is straightened out, and what at first appears a most difficult operation often becomes a comparatively easy matter.

In regard to the mortality of these different operations, I have already mentioned that cholecystotomy in non-malignant cases had a mortality of about 1·8 per cent., incision of the common bile-duct for a stone blocking it, whether in the supra-duodenal or the retro-duodenal part, varies from 1·9 to 5 per cent., and removal of the gall-bladder from 3 to 5 per cent.

Other complications arise from gall-stones outside the bile-ducts, but I have not time to draw your attention to my cases of chronic pancreatitis, or to the many different fistulous connections which the gall-bladder or the bile-ducts may make with the duodenum and other abdominal viscera. But as it illustrates the formation of one variety of fistula, I will quote to you the case in which I missed a gall-stone, for at the second operation a cholecysto-duodenal fistula was found. Agnes K—, æt. 58 years, was admitted into Martha Ward last November for pain in the abdomen, which began in November, 1908, and continued until March, 1909, and then subsided for six months, only to begin again in October, 1909: she described it as localised to the right hypochondrium. Three weeks before admission she became jaundiced, and was much troubled with constipation: she was operated upon, and her gall-bladder was found hard and contracted: there were many adhesions, indicating former peri-cholecystitis, and four stones were removed from her gall-bladder, the largest of these being impacted in the cystic duct. I could not feel any stones, either below in the common duct, or above in the hepatic duct, or in the region of the lower surface of the liver, so I completed a cholecystotomy, after suturing the fundus of the gall-bladder to the parietal peritoneum. The day before her discharge on December 18th she passed bile in her urine, and on reaching home was jaundiced for two or three days, but this disappeared, and during January she came to my Out-patients several times, apparently quite well. Towards the end of January, 1910, the pain recurred, and she became again jaundiced. On February 4th she was re-admitted and her abdomen was opened. The gall-bladder was found adherent to the duodenum and there was a fistula connecting these viscera. I removed a stone two inches long and one inch broad from the supra-duodenal portion of the common bile-duct and drained the gall-bladder and common duct. This relieved her pain and jaundice, but she is still attending Out-patients with a mucous fistula leading apparently into her gall-bladder. I suppose cholecystectomy will be necessary to cure this.

This case serves to show the formation, with common bile-duct obstruction, of fistulous openings between one part of the bile-passages and the intestine; it also serves to show that gall-stones may be missed, and as it is inconceivable that one could have missed a stone of such a size at the first operation, that they may rapidly increase in size.

Intestinal Obstruction.

In Barnard's lectures on intestinal obstruction,* he regards gall-stones as a rare cause of intestinal obstruction, for in 669 cases of intestinal ob-

* 'Contributions to Abdominal Surgery,' p. 95 *et seq.*

struction which were dealt with at the London Hospital in thirteen years, there were only fifteen cases due to gall-stones: Fitz analysed 295 cases, of which twenty-three were due to gall-stones. Of Barnard's fifteen cases, thirteen were operated upon and eight died, giving a mortality of over 61 per cent. This is higher than the average mortality of acute intestinal obstruction from other causes, so that you will be well advised when making a diagnosis of intestinal obstruction from gall-stones to give a serious prognosis. The symptoms are those common to most types of acute obstruction, abdominal pain, constipation, and vomiting, but the latter is peculiar in that on about the third day it may cease, and the general condition of the patient may improve. This Barnard ingeniously accounts for by showing that when the gall-stone at first lies in the duodenum it sets up acute abdominal pain and bilious vomiting, but having passed down into the jejunum on about the second or third day the symptoms remit and the vomiting subsides. On the fourth day the patient usually becomes collapsed, and vomiting recurs as the stone becomes impacted in the narrower ileum and it is usually only at this time that the acuteness of the symptoms draws attention to the patient's urgent condition. Here is a typical picture of a person who had intestinal obstruction from gall-stones—a fat woman, æt. 60 years, suffering from acute obstruction of the small intestine type, without distention. The intermittent colicky character of the pain during the early days of the attack leads us to infer that strangulation of the bowel is absent, and that the obstruction is partial only. Vomiting is the great feature of the attack: It is severe, continuous, and profuse, and is early stained brown by bile or blood, so as to closely simulate fæcal vomiting. The following case is in many respects typical. Florence S—, æt. 51 years, was admitted under Dr. Hertz last August for vomiting and constipation. In 1908 she had pain in the abdomen for six days, but she was never jaundiced. On July 26th, 1909, she had an acute attack of pain all over her abdomen, but the pain was most acute under the right costal cartilages; she vomited, and her bowels acted frequently. Since July 29th her bowels have not acted, and she has passed flatus only by the mouth. On admission the pulse was 128, and only just perceptible at the wrist, temperature 96° F.; she was fat, collapsed, cold, and clammy. The abdomen generally was tender and slightly distended, and there was no rigidity. The rectum was full of hard fæces. The report states that Dr. Hertz suggested as the cause of the obstruction a growth, a band, or a gall-stone, and it also says that I remarked that she was the kind of person to have obstruction from a gall-stone. She was a fat, flabby woman of over fifty, in whom there was nothing to suggest she had a band from tuberculous peritonitis, and there was no previous history of chronic obstruction becoming acute, suggesting she had a growth, though in many cases such a previous history is withheld. Her abdomen was opened, two hours after admission, during which time she was continuously infused, and the small intestine was found to be distended, while the large intestine was collapsed. Two stones were found immediately by passing the hand downwards into the pelvis. They were in the ileum, quite loose, and the small intestine beyond them was distended, so the bowel below was passed through the hands and a third stone was found lying firmly impacted two inches above the ileo-cæcal valve. The bowel above it was distended, thickened, and enlarged, whereas the small intestine beyond it was shrunken, collapsed, and small. The stone was removed by enterotomy and the incision sutured. In spite of frequent stimulation the patient died four hours later.

I like a paragraph in Barnard's book so much that I must read it to you: "This form of obstruction is almost the only one in which a hand swept at a venture round the abdomen and pelvic bowel is almost certain to detect the cause." For although I have only recently read this graphic description, it well expresses exactly what I did, for passing my hand down into the pelvis at a venture, I found the gall-stone and immediately brought it into view.

The obstruction is in the ileum in 65 per cent. of the recorded cases, and my case died, as apparently all the cases of which I have seen the records have died if operated upon after the fourth or fifth day of acute obstruction, from continued stercoræmia and shock.

October 31st, 1910.

THE LATE RESULTS OF SOME OPERATIONS ON THE STOMACH.*

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MR. PRESIDENT AND GENTLEMEN,—I thought it would be a relief from the tedium of watching operations if I were to let you see some of the patients upon whom I have performed operations similar to those which you have witnessed this afternoon, so that you can judge for yourselves how far some of the statements I have made to you are justified by the results obtained.

I am disposed to think that in the past we have paid such attention to the immediate mortality of these operations that we have concerned ourselves too little with the remote results. Fortunately, at the present time the mortality rate of gastric operations in such skilful hands as those of the members of your distinguished Society is so small that it is scarcely a matter for further discussion. What now is necessary is accurate information as to the remote results, which are of no less importance than the immediate.

Accordingly, I have asked all of those patients, whose operations date back more than four or five years, and who reside within easy reach, to come up this afternoon to be examined and cross-questioned by you, and some thirteen have replied to my invitation.

The first group of patients whom I have to show you illustrates the treatment by gastro-jejunostomy of ulcers at the middle, or at the cardiac end of the stomach. As I said in the operating theatre, I strongly disagree with the dictum that gastro-jejunostomy is indicated in the case only of those ulcers which are situated near the pylorus. I think it was Cobden who said that if one wishes to get any truth into the head of the British nation, one must go on repeating the same thing in slightly different language. For years I have been advocating the view that gastro-jejunostomy is not a drainage operation. The success which follows this operation in cases of gastric ulcer is due, not to drainage, but to the physiological effects of the operation in altering the gastric secretions, and this alteration follows gastro-jejunostomy, irrespective of the situation of the ulcer. In my experience, the results of gastro-jejunostomy in cases of ulcers at the cardiac end of the stomach have been equally, if not more, satisfactory than the results of operation for ulcers near the pylorus. I have here five patients, illustrating the treatment of cardiac ulcers by gastro-jejunostomy.

The first is a woman, æt. 46 years, upon whom I performed anterior gastro-jejunostomy nearly seven years ago for a gastric ulcer towards the cardiac end of the stomach. This case illustrates another point in the management of gastric cases, namely the treatment of hæmatemesis. For six years before her admission this patient had been, more or less, a complete invalid. She had had several attacks of hæmatemesis, and had been living almost entirely upon milk. Shortly after admission she had a very profuse hæmatemesis, which nearly cost her her life. Now, personally, I am strongly of the opinion that it is rarely, if ever, expedient to operate during the progress of gastric hæmorrhage. The number of cases in which hæmorrhage is immediately fatal is very small, and the percentage mortality from operation *during* hæmorrhage is exceedingly high.

Neither do I approve of opening the stomach and searching for the bleeding point. We used to be taught to cut down upon and tie a bleeding vessel, and this rule was impressed upon us with such high-sounding phrases as that we should look our enemy in the face. Personally I prefer not to see him if I can deal with him in another way, and I believe that in those cases in which surgical treatment is necessary, gastro-jejunostomy is the best and safest way of treating gastric hæmorrhage.

Therefore I believe the safer plan is to rely upon medical means to arrest the hæmorrhage, and then to operate later when the patient has recovered from the anæmia consequent on the hæmatemesis. This patient was kept in bed for nearly three months after her severe hæmorrhage before gastro-jejunostomy was performed. She now tells us that she has remained perfectly well ever since.

The next patient is a woman, æt. 36 years, upon whom four years ago I performed posterior gastro-jejunostomy for an ulcer near the cardiac end of the stomach. She likewise has not had a day's illness or discomfort since the operation.

* A demonstration given before the American Society of Clinical Surgery, at the London Temperance Hospital.

Here is a man, æt. 44 years, who had a large ulcer on the posterior wall of the cardiac end of the stomach, which was treated by posterior gastro-jejunostomy four years ago. He also has remained ever since in perfect health.

The next patient whom I have to show you illustrates another point on which I wish to lay stress. She is a woman, æt. 55 years, on whom posterior gastro-jejunostomy was performed by another surgeon more than seven years ago for an ulcer about the middle of the stomach. She had suffered from much pain and vomiting for five years. Latterly she had on occasions vomited as much as two or three pints at a time. Within two months of her operation her symptoms returned, the vomiting being more persistent than before. Two months later I re-opened the abdomen and found the anastomotic opening completely closed. I performed an anterior gastro-jejunostomy and the patient has had no further trouble.

The closure of the opening in this case is of interest. Recently we have heard a good deal about the closure of gastro-jejunostomy openings in cases of patent pylorus. It seems inevitable that some misconception or other should be prevalent with regard to the operation of gastro-jejunostomy. A few years back we were confronted with the bogey that gastro-jejunostomy interferes with metabolism. This bogey is now dead, and it will be within your recollection, Mr. President, that in 1907 I visited the United States in order to take part at its obsequies. Now another bogey is with us, namely, that in cases of patent pylorus the gastro-jejunostomy opening gradually becomes obliterated. Acting on this belief, some surgeons advocate that when gastro-jejunostomy is performed in cases of patent pylorus, the pylorus should be deliberately excluded by suturing. The train of reasoning appears to be that, when the pylorus is patent, the food passes out through the pylorus in preference to the gastro-jejunostomy opening, and so the artificial opening, as it is not used, gradually contracts and becomes completely closed. Personally I do not believe that a properly performed gastro-jejunostomy opening will close up because the pylorus is patent. In the case I have just related the first gastro-jejunostomy was effected by means of a single layer of serous sutures without suture of the mucous membrane. This is an antiquated and unsatisfactory method, and, as I have shown elsewhere, is a method apt to result in considerable contraction or even closure of the opening. If the opening is made of large size, if the mucosa of the jejunum is accurately stitched to the mucosa of the stomach, and if the patient is properly treated after the operation, we need not anticipate any serious contraction of the opening. Physiologists tell us that if dogs on which gastro-jejunostomy has been performed are fed on bags of shot, the shot passes out, not through the gastro-jejunostomy opening, but through the patent pylorus. These experiments are of interest scientifically, but do not concern the practical surgeon. We are not accustomed to feed our patients on bags of shot, and I do not think it is of the slightest importance whether the food passes out by the pyloric or by the anastomotic opening. Why we should close the pylorus to compel the food to travel by a route it does not like passes my comprehension. At the risk of being wearisome, I repeat my opinion that gastro-jejunostomy is a physiological operation, which acts by altering the secretion of the stomach, irrespective of whether the food passes out of the stomach by the pylorus or by the gastro-jejunostomy opening. Exclusion of the pylorus, at least so it appears to me, is an unnecessary complication of gastro-jejunostomy based upon erroneous pathology, and therefore should be considered meddlesome surgery.

I have been able to follow the after-history of every patient on whom I have performed gastro-jejunostomy for a cardiac ulcer, and not one of them has had any recurrence of symptoms. I feel, therefore, justified in my belief that, when medical means have failed, gastro-jejunostomy for ulcers in the body of the stomach is a satisfactory and successful procedure.

The next patient is a man, æt. 32 years, who had a perforated gastric ulcer at the cardiac end of the stomach, which was treated by anterior gastro-jejunostomy. He also illustrates the beneficial effects of gastro-jejunostomy on cardiac ulcers, but in connection with his case I wish to say a few words on the routine performance of gastro-jejunostomy in cases of perforated gastric or duodenal ulcer. His case is of such interest that I will first of all, with your permission, give a short account of his illness.

He was admitted to the hospital seven and a

half years ago. The first point of interest is that his ulcer perforated while he was asleep in bed five days after admission, during which period he was fed entirely by the rectum. On opening the abdomen I found a perforated ulcer at the extreme cardiac end of the stomach. Surrounding the ulcer was a mass of friable indurated tissue, rendering a satisfactory closure of the gap in the stomach-wall impracticable. I sewed up the perforation as best I could and performed anterior gastro-jejunostomy. None of the sutures of the abdominal wall held, and the edges of the wound sloughed, exposing coils of intestine in the whole length of the wound. Two weeks after the operation the patient's temperature gradually rose, and for some weeks varied between 98·8° and 103·8° F. No cause for the rise of temperature could be discovered. The sub-diaphragmatic area was explored on several occasions, but no signs of pus were discovered. Two months after the operation he had a rigor, and on the following day a sudden attack of severe dyspnœa. Examination then showed that the whole of the right chest was dull to percussion. The right chest was aspirated, and sixty-three ounces of pus were drawn off. He was extremely collapsed, vomited several times, and his pulse-rate was 140, very feeble and irregular. He rallied, however, and one week later his right chest was aspirated again and forty ounces of pus were drawn off, and a week later a portion of rib was resected. Thereafter he made a slow but steady recovery, and left the hospital six months after his operation with the sinus in the right side of his chest unhealed.

The sinus persisted for seven months, and as it did not seem likely to heal I advised him to come in and have another rib resected. The mere suggestion of another operation was sufficient; within six weeks' time the sinus had completely healed up.

Notwithstanding all he went through, he is to-day in perfect health, and his abdominal scar is surprisingly strong considering the large area of intestine exposed. His condition after his first operation rendered out of the question any attempt at secondary suture of the abdominal wall.

Here is another man, æt. 42 years, on whom I sutured a perforated duodenal ulcer and performed posterior gastro-jejunostomy at the same time. He has had no recurrence of dyspeptic symptoms.

Since the beginning of 1904 I have adopted the plan of performing gastro-jejunostomy in addition to suture of the perforation as the routine treatment of perforated ulcers, gastric or duodenal. My reasons for advocating this step are as follows:

(1) It improves the after-condition of those that recover, but, of greater importance—

(2) It lessens the risk in cases where a second perforation is undiscovered.

(3) It diminishes the risk of perforation of a second ulcer.

(4) It diminishes the risk of hæmatemesis after operation.

(5) It allows of more efficient infolding of the ulcer in cases in which the perforation is in the pyloric third of the stomach.

(6) It promotes more rapid and certain healing of the line of suture, and so lessens the risk of further leakage.

(7) It permits earlier feeding, earlier administration and more rapid action of purgatives, and so secures the more efficient draining of the peritoneal cavity.

The first patient I treated in this way died, but since then (1904) I have not lost a single case of perforated gastric or duodenal ulcer in which I have carried out this procedure, and I have done so in all cases but one. The one exception was the case of a man with a perforated duodenal, as well as a perforated jejunal, ulcer, who was too ill to admit of a general anæsthetic being administered, so that I had to be content with inserting a tube into the perforation and packing it round with gauze. He rallied for a time, but died from perforation of a second jejunal ulcer.

Some years ago I had a case of perforated duodenal ulcer in which it was absolutely impossible to suture the perforation at all, but the patient made a complete recovery after the performance of gastro-jejunostomy.

Two objections have been raised to this procedure: first, that the tissues are not in a condition for a plastic operation, and secondly, that it unduly prolongs the operation.

In my opinion neither objection is valid. My experience is that, even in cases of septic peritonitis, peritoneal surfaces unite perfectly well if kept in apposition by the firm pressure of a continuous suture.

To the objection that the performance of gastro-

jejunostomy unduly prolongs the operation, my answer is, that it ought not to prolong the operation more than five, or at the outside seven, minutes. I have several times sutured a perforated ulcer and performed gastro-jejunostomy in less than twenty-five minutes.*

A well-known writer, commenting on my advocacy of a primary gastro-jejunostomy, states that in some very *early* cases a gastro-jejunostomy may be performed primarily, but that, in the majority of cases, it is safer to perform it at a second operation. He has missed the point of my argument. My chief reason for advocating a primary gastro-jejunostomy is, not to guard against future recurrence, but to improve the patient's chance of recovery. It is the *later*, and not the *earlier*, cases in which gastro-jejunostomy is especially indicated. I hope that those of you who have not tried this plan will do so, as it is my firm conviction—and I feel very strongly indeed on this point—that in all cases the performance of gastro-jejunostomy greatly improves the patient's immediate chance of recovery, as well as insures a more complete and permanent cure.

The next patient who is coming in is a man, æt. 58 years, who was admitted to the hospital in October, 1902, with a severe attack of gastric tetany. For six weeks before admission he had suffered from attacks of vomiting every two or three days, accompanied by severe pain. The tetanic attack lasted for six hours after admission, every muscle in his neck, trunk and limbs being in a state of rigid contraction; his temperature rose to 102·8° F., and he was unconscious.

A few days later he had another, but slighter, attack. His stomach was greatly dilated, and the vomit contained an excess of volatile acids. Dr. Fenwick diagnosed simple pyloric stenosis, and asked me to see him with a view to performing gastro-jejunostomy. On opening the abdomen I found a very tight pyloric stricture, and I performed an anterior gastro-jejunostomy. He now tells us that he is in perfect health, has never had a day's illness since the operation, eight and a half years ago, and can eat and digest anything. When he left the hospital he weighed 5 st. 3 lb.; he now weighs just 9 st., which is about his normal weight.

This case is of interest because it is, I believe, the first case in England in which gastro-jejunostomy was suggested, and performed, for gastric tetany. In Scotland, a few months earlier, Mr. Caird, of Edinburgh, had performed gastro-jejunostomy in a similar case, but it was not reported until a later date, so that when Dr. Fenwick suggested gastro-jejunostomy in this case he was unaware that this operation had previously been practised for gastric tetany. At an earlier date some cases had been treated surgically, but in all of them pyloroplasty had been performed. I feel sure that you will agree with me that pyloroplasty is an unsatisfactory operation. In my experience relapse occurs in at least twenty-seven per cent. of the patients submitted to this operation, and probably the proportion of relapses is even higher.

The next two patients whom you will see are examples of the surgical condition known as "hour-glass stomach."

The first patient is a woman, æt. 47 years, upon whom I operated just over five years ago. The patient stated that she had always been subject to indigestion. She vomited frequently, usually soon after taking food. She had lost all appetite, and had been living entirely upon slops, and had wasted considerably. On admission she was very thin, sallow, and anæmic, and weighed 6 st. 5 lb. There was a prominent swelling in the left hypochondrium, over which a well-marked stomach-splash could be detected. In the middle line, close to the umbilicus, there was an ill-defined swelling. On washing out the stomach there was constant loss of a portion of the fluid introduced.

On distending the stomach with CO_2, there was at first a marked swelling in the left hypochondrium, which gradually disappeared, and was followed by a fulness in the epigastrium. A diagnosis of hour-glass stomach was made, and operation was advised. The patient declined surgical treatment, and left the hospital a month after admission, having gained five pounds in weight and feeling very much better. Two weeks later she was re-admitted, the pain and vomiting having recurred shortly after her discharge. She had lost three pounds in weight. Shortly after her re-admission I opened the abdomen and found

* Recently, in a desperate case, I sutured a perforated gastric ulcer, performed anterior gastro-jejunostomy and drained the pelvis in twenty-two minutes from the time of first incision to the final closure of the wound. The patient stopped breathing while on the table and again while on the way to the ward, but eventually made an excellent recovery.

that the stomach was divided into three pouches by two transverse constrictions—the so-called "trifid stomach." The pyloric pouch was the largest of the three, and the constriction at this end of the stomach formed a dense mass, chiefly on the posterior gastric wall. Internally, this stricture just admitted the tip of the little finger, while that towards the cardiac end admitted two fingers easily. Gastroplasty was performed and the jejunum united to the anterior wall of the pyloric pouch (gastroplasty and gastro-jejunostomy). The patient made an uneventful recovery, and has remained in good health since the operation, can take ordinary food, and has regained her normal weight.

My present opinion is that gastroplasty is an unnecessary addition to gastro-jejunostomy. In treating such cases now I simply perform gastro-jejunostomy on the proximal side of the constriction, and leave the constriction untouched.

This other patient was treated in this way. She is a woman, æt. 44 years, who gave a three years' history of pain and vomiting, and had had repeated attacks of hæmatemesis. She had a well marked hour-glass constriction round an indurated ulcer at the junction of the pyloric with the middle third of the stomach. Four years ago I performed a posterior gastro-jejunostomy on the proximal side of the constriction, and the patient has remained quite well ever since.

The next case I have to show you—that of a man æt. 36 years, with visceral syphilis suggesting malignant disease of the stomach—is one of some interest. His illness began about a year before his admission into the hospital, with pain and a feeling of fulness coming on about an hour after food. He improved for a time, but later suffered from a good deal of vomiting. On admission his stomach was markedly dilated, and to the right of the umbilicus there was a freely movable, insensitive lump about the size of a walnut. The lump moved with respiration. The motility of the stomach was greatly impaired. The total acidity of the gastric contents was 44, and there was excess of volatile acids, but no free hydrochloric acid. The hard lump at first suggested malignant disease, but consideration of the clinical history and the chemical findings led to a diagnosis of inflammatory stenosis of the pylorus. On opening the abdomen I found a gumma of the liver, which was adherent to, and had caused obstruction of, the duodenum. The gumma was drained, and posterior gastro-jejunostomy was performed. He tells me that until he received my letter asking him to come up this afternoon he had forgotten all about the operation, and that, although not careful with his diet, he has never had a day's trouble since he was in the hospital, and can eat and digest anything.

The next two patients illustrate the treatment of duodenal ulcer. The first is a man, æt. 32 years, upon whom I operated for duodenal ulcer when Dr. William J. Mayo visited this hospital some years back. He gave a history of dyspeptic trouble for nine years, and has remained quite well ever since his operation.

Up to the time of my visit to the United States in 1907 most of my gastro-jejunostomies were done for gastric ulcers, whereas I now find that for one gastric ulcer I operate upon four duodenal ulcers. I think the reason for this difference is that I did not accurately distinguish between ulcers on the distal side and ulcers on the proximal side of the pylorus. In my notes they are grouped together as pyloric ulcers. I have no doubt that many of them were really duodenal ulcers.

The next patient is a woman, æt. 44 years, who was admitted for a duodenal ulcer associated with profuse hæmatemesis and melæna. She also illustrates what I have said regarding the treatment of gastric hæmorrhage. Even after admission the hæmorrhage persisted for some weeks, and it was over three months before she was in a fit state for operation.

At the operation I found a duodenal ulcer, and also gall-stones. I performed posterior gastro-jejunostomy and cholecystostomy, but I did not infold the ulcer nor exclude the pylorus. The patient has had no further trouble since the operation.

As far as I could tell the ulcer was healed when I operated, but there was a lot of cicatricial contraction round it. I have no doubt that duodenal ulcers do heal, but my experience is that in healing they usually produce a degree of stenosis, which eventually requires operation.

The last patient I have to show you is an example of those surprises with which we meet from time to time in surgical practice.

The patient is a woman, æt. 28 years, who was admitted for pain and severe vomiting. Just below and to the right of the umbilicus was a hard, round

mass. The motility of the stomach was greatly impaired, but the result of gastric analysis was against malignant disease. On opening the abdomen I found an extensive lump involving the pylorus extending along the lesser curvature, and many enlarged glands. I had little doubt that the swelling was a malignant growth. It would have been just possible to remove it, but it would have been a very extensive operation. I decided therefore to trust to the evidence of the gastric analysis, and act on the assumption that the disease might prove innocent. I confess that I had not much hope that this confidence would be justified. However, I performed an anterior gastro-jejunostomy, and, as you see, the patient, who was at the time of the operation greatly emaciated, is now fat, robust, and well, having put on three stone in weight since the operation. It is of course possible that the disease is, after all, malignant, but in view of the very rapid and marked improvement in her condition, I do not think that this is probable.

I had hoped to show you a patient upon whom I operated for a jejunal ulcer following gastro-jejunostomy, but she has not arrived. I can, however, show you the specimen and give you a brief account of the case.

She is a woman, æt. 55 years upon whom I performed anterior gastro-jejunostomy for pyloric stenosis in August, 1902. She remained quite well for three years, and then began to have some pain after food. She was re-admitted in September, 1907, with a perforated jejunal ulcer, for which I resected six inches of jejunum, and anastomosed the distal end of the jejunum to the stomach, and the proximal end of the jejunum to the side of the jejunum four inches below the point at which it had been anastomosed to the stomach (Roux's operation.)

Since then she has put on weight and remained in good health, except for occasional pain after food, due, I think, to the circumstance that she is very reckless as to her diet. At any rate the pain is at once relieved by alkalies. Here is the specimen, showing an ulcer in the jejunum with hard induration round it. The ulcer had become adherent to the abdominal wall, and finally perforated, resulting in a jejunal fistula. I have published elsewhere* a full account of the case, together with an account of the gastric analyses made before and after operation.

You will have observed, perhaps with some surprise, that the anterior operation was performed in the case of six of the thirteen patients whom you have seen. The reason is that most of the patients who are here to-day date back to a period when I performed the anterior operation exclusively. Since visiting your country I have usually performed the posterior no-loop operation, anastomosing the jejunum to the stomach from right to left, as suggested by Dr. W. J. Mayo. My experience is, however, that either operation is equally satisfactory and that both give excellent results. I have had only two cases of vomiting after the operation, one being one of my early anterior gastro-jejunostomies, and the other one of my first posterior gastro-jejunostomies. In this latter case I believe that I twisted the jejunum a little in its longitudinal axis during my suturing. After two or three months the vomiting gradually diminished and ceased, and the patient is now quite well. My only reason for preferring the posterior operation is that possibly —although this is not certain—it is less apt to be followed by jejunal ulcer

October 31st, 1910.

* 'Jejunal and Gastro-jejunal Ulcer,' 1910, Bale, Sons and Danielsson.

Corrigendum.

Dr. H. Macnaughton Jones writes to say that the passage in his paper on "The Lessons of a Session," printed on page 21 of The Clinical Journal, October 19th, which runs thus: "Also, I believe that in a special type of case, particularly if a post-partum one, pilocarpine, judiciously administered, is a most useful agent. Certainly I have directly saved life by it on at least two different occasions. Possibly tyramine and infundibulin may now have a place as a means of lowering the blood-pressure"—should have appeared as follows: "Also, I believe that in a special type of case, particularly if a post-partum one, pilocarpine, judiciously administered, is a most useful agent. Certainly I have directly saved life by it on at least two different occasions. *Possibly in certain cases tyramine and infundibulin may now have a place as means of raising the blood-pressure.*"

THE APPENDIX IN MIDDLE AND LATER LIFE.

By EDRED M. CORNER, M.C.Cantab., F.R.C.S.,
Surgeon and Lecturer on Practical Surgery to St. Thomas's Hospital; Surgeon to the Children's Hospital, Great Ormond Street, etc.

THE operation of appendicectomy is nowadays a frequent one. In the quiet interval it is very easy and as safe as anything in surgery. Modern surgery has proved this, and also that a post-operative hernia is only likely to occur when there is deep and extensive suppuration. Hence there is every reason to remove the operation of appendicectomy from the list of those which require specialism for their performance and to place it in the list of those for frequent and "daily" use. Is there, then, any benefit to be derived from its frequent use? Undoubtedly there is; and that particular use is bound up with a greater knowledge of the causation of appendicitis. The causation of appendicitis is undoubtedly to be found in the action of the micro-organisms in the ileo-cæcal region. The reasons why the ileo-cæcal region is picked out and why the appendix has evolved in that region have been considered at length in the Erasmus Wilson Lectures delivered at the Royal College of Surgeons in 1904 and in 'Surgery of the Diseases of the Vermiform Appendix' (second edition).* The conditions may be restated briefly: in the ileo-cæcal region there is a pause in the passage of the products of digestion along the alimentary canal to enable the liquids in the small intestine to be converted into a more solid condition for their passage along the large intestine; this "pause" in an alkaline medium allows the growth of micro-organisms, which are a thousand-fold more frequent in the ileo-cæcal region than elsewhere in the alimentary tract; this concourse of organisms demands the presence of a quantity of lymphoid tissue for the production of leucocytes and the protection of the body; this lymphoid tissue is the characteristic tissue of the appendix, and of animals with no appendix, of the apex of the cæcum. Hence the appendix represents the specialised terminal part of our ancestral cæcum and is not functionless.

From this brief *resumé* it will be seen that this civil war in the ileo-cæcal region will be taking place in the ileo-cæcal regions of everyone; and if the struggle becomes unduly fierce its results become recognisable clinically, and are called appendicitis. Therefore it may be said that everyone has appendicitis in degree! A truly alarming statement. But it must be remembered that it is only called appendicitis when the condition becomes sufficiently bad to cause signs and symptoms enabling it to be recognised clinically. When the ileo-cæcal mischief does not cause obvious symptoms it is not called appendicitis, but ill-health, and is treated by tonics and purgatives. Hence it is true that we only recognise the severer forms of appendix mischief, and we are quite unable to recognise less severe conditions, so much so that it has become obvious to me when attempts have been made to reconcile the clinical history of the patient with the conditions found at operation. In the second edition of the 'Surgery of the Diseases of the Appendix Vermiformis'* an account has been given of the appendix as it was found in two cases which never had a recognisable attack of appendicitis. In both the appendix was badly diseased. The latest similar example which I have met may be briefly quoted.

A boy, æt. 4½ years, the son of sensible parents and a patient of Dr. Hill, of Wimbledon, had an attack of acute appendicitis from which he died on the sixth day after the operation. His parents stated that this attack was his first attack. Yet the appendix was bent in shape of the mediæval bishop's crook, it was very adherent, was much thickened, and perforated, and contained no fewer than four concretions. The boy must have had inflammation about his appendix for a long while—unrecognised and unrecognisable until it produced a fatal attack!

Hence it is true that *we can only recognise the more severe forms of ileo-cæcal strife as appendicitis; and at present we are quite incapable of recognising clinically the less severe.*

Returning to the proposition that every living being has strife proceeding in his ileo-cæcal region between the micro-organisms and the lymphoid tissue, it may be asked, What are the results of such physiological stress? There are many results, but a few may be enumerated: fulminating appendicitis, suppurative appendicitis, and acute appendicitis in a few cases; chronic appendicitis in a large number; and physiological incompetence of the appendix in a still larger number. If a fire

* Both published by Constable & Co.

* Published by Constable & Co., pp. 105–8.

is burning continually it may burn up all available and inflammable material, or as the material is burnt it may be replaced by new material. A similar relation to the fire and the inflammable material exists between the micro-organisms of our ileo-cæcal region and the lymphoid tissue of our appendix. This was first recognised "as in a glass darkly" by those who stated that as years advanced the lymphoid tissue disappeared. This statement is not universally true; some septuagenarians' appendices have plenty of lymphoid tissue. In the former case the ileo-cæcal strife has "worn away" the lymphoid tissue; in the latter case it has been replaced by fresh lymphoid tissue. Yet this strife may cause no direct sign or symptom by which it may be recognised. Indirectly it can, I think, often be recognised—headache, furred tongue, loss of appetite, constipation or irregularity of the bowels, plus a general feeling of being "below par." Such symptoms in a person over thirty-five or forty years of age should suggest unrecognised appendicitis (appendicular incompetence). A safe and simple operation is all that is needed and should be advised. It should not be a rare and financially ruinous operation, but a common one. It demands about three weeks' convalescence. And it must have struck all inquiring surgeons how often, after removing a diseased appendix from a subject over forty, in six months' time that subject has said that he "never felt better in his life." Why is it that that feeling of *bien être* after appendicectomy is not found more frequently in younger subjects? It must be connected with the impaired physiological value or the loss of the lymphoid tissue in the appendix region. Hence it may be said that if an appendicectomy was performed in all people over forty years of age who were in ill-health, attributable to no other ascertainable cause, the great majority would be greatly improved.

To sum up, strife between the micro-organisms and lymphoid in the ileo-cæcal region is physiological to all; that strife is already recognised in its severer forms as appendicitis; that strife in its less severe forms, such as that which takes place with the passage of years at the present time is only recognisable by general symptoms and ill-health; that general ill-health, due to no other ascertainable cause, will be improved or cured by the simple and safe operation of appendicectomy.

October 31st, 1910.

MEDICAL EDUCATION.

THE difficult subject of medical education in this country has received masterly treatment by Dr. S. Squire Sprigge, the well-known editor of the 'Lancet.' This gifted writer has so clearly and so succinctly described the details of the amazing tangle of conflicting interests concerned in medical education that at last the question is plainly set before us, and it will not be Dr. Sprigge's fault if the matter now fails to receive adequate attention. The book in which Dr. Sprigge has crystallised his well-balanced opinions and thoughtful judgments is entitled 'Some Considerations on Medical Education,' and is published by Baillière, Tindall & Cox. The author's preface is pregnant with suggestive ideas and illuminated with sound common-sense. His book explains the position of the General Council of Medical Education and Registration, and insists upon the valuable nature of the work done by this body. He takes the opportunity of reminding those who ignore the fact that the Council, which works for the public and exists to see that the public is served by a properly enrolled and adequately instructed medical profession, is supported pecuniarily by the medical profession. There are weak places in our system of medical education, and for their treatment many remedies have been suggested; and as the progress of science increases these weak places, so the proposals of remedy increase, until those who conduct the system find many complications awaiting them. The various difficulties which have arisen from a plethora of degree- and diploma-giving bodies, from an over-crowded curriculum, from an excessive elaboration of purely scientific instruction, and from other well-known reasons, are duly set forth and their solutions are suggested. The seventh chapter ends in an abrupt manner, but there is just now a pause in the proceedings described in the section which is concerned with the medical education of the London student. That the University of London on the one hand, and the Royal College of Physicians of London and the Royal College of Surgeons of England on the other, may come to some mutual arrangement which will further the cause of medical education as a whole, and will remove some obvious hardships for the London medical student, is sincerely to be hoped.

CONGENITAL DISLOCATION OF THE HIP.

THE dangers incident to the manipulative operation of Lorenz for congenital dislocation of the hip are due either to too much being attempted, or to inexperience on the part of the operator. If a patient approaches the age limit—about eight years for double dislocation or about ten years in single dislocations—the danger of too extensive laceration of muscles or injury to bones can be entirely removed by a course of preliminary treatment; *e. g.* the adductor longus and hamstring tendons and the tensor fasciæ femoris may be divided, a weight-extension applied, the legs being fully abducted, and, after some days, the manipulation for stretching the muscles practised daily for a week or two before the operation, which will then be found to be an easy matter in practised hands—and it is only by practice that the safe degree of force to use in the operation can be learned. It may be asked why this preparatory treatment should not be adopted in every case, in order to diminish the gravity of the actual operation. To this it may be answered that a certain amount of reaction that is set up by stretching resistant muscles and ligaments, and by tension between the replaced head of the femur and the previously empty acetabulum, is required for the making of a firm new joint.

The above is from an excellent book by Mr. J. Jackson Clarke on congenital dislocation of the hip, published by Baillière, Tindall & Cox. In this volume Mr. Clarke gives an interesting exposition of his views on this condition and the examples he quotes are as convincing as his arguments.

HERNIA.

MR. R. W. MURRAY, in a clearly printed and well-illustrated book called 'Hernia, its Cause and Treatment,' issued by Messrs. J. & A. Churchill, describes his successful method of operating for the cure of hernia and the subsequent treatment of the patient, based upon the view that the presence of the sac is the essential cause of the hernia. The procedure he adopts is much the same as that practised many years ago by the late Sir William Banks. Banks was the first surgeon in England who said that the most important part of the operation was the complete removal of the sac. Writing in 1884 he said, "The great object of the whole proceeding is to restore a uniform surface to the peritoneal wall, and hence the higher up the sac is tied the better the chance of this being permanent." However, Banks made the skin incision so low down as to encroach upon the scrotum, thus rendering the application of the ligature to the neck of the sac at the highest possible level a matter of considerable difficulty.

To the Editor of THE CLINICAL JOURNAL.

DEAR SIR,—I shall feel much obliged if you can find room for a few lines from my pen on the treatment of consumption by tuberculin as practised by Dr. Camac Wilkinson at the dispensary,* 263, Kennington Road, S.E., where I have been a visitor off and on for the past two months.

I have been struck by the marked improvement in the patients as regards their own sense of well-being and the increase in weight.

The method seems to be eminently suitable for the treatment of large numbers of sufferers at a cost much below that entailed by sanatoria, and, it would appear to me, with far better results.

The secret of success lies in a proper dosage (and the dose that ought to be given is not to be learnt in a day) and correct diagnosis. The method of testing a case before commencing treatment is not one that appears to be generally employed, but without this preliminary grievous mistakes are apt to occur, cases that are tuberculous being often missed entirely and those that are not tuberculous being sometimes diagnosed as such.

Let me emphasise the point that the method is not dangerous *when rightly employed.* This must be seen at the dispensary to be properly appreciated. There is much else that one might write about, but those who are interested should see for themselves.

Yours faithfully,

L. D. PARSONS,
Port Surgeon.

Gibraltar.

* The hours of attendance are from 9 to 11.30 in the morning on Tuesdays and Fridays. The Dispensary is open to the medical profession.—EDITOR.

THE CLINICAL JOURNAL,

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ARTHRITIS: ITS VARIETIES AND TREATMENT.

By WILLIAM MURRELL, M.D., F.R.C.P.,

Physician to the Westminster Hospital, and Lecturer on Medicine and Clinical Medicine.

RECENT advances in bacteriology and the careful study and accumulation of clinical cases have thrown much light on the differential diagnoses of the various forms of arthritis and have added somewhat to our knowledge of their treatment. Some forms of arthritis, such as the rheumatic, the rheumatoid, and the gonococcic, are familiar to everyone, whilst others, for example, the influenzal, the pneumococcic, the enteric, and the scarlatinal are less generally recognised. It is proposed to describe these various forms of arthritis, setting forth their salient features, beginning with the common forms and passing on to those less known.

(1) *Rheumatic arthritis*, also known as rheumatic fever or acute rheumatism, is due probably to the *Micrococcus rheumaticus*, although this organism is rather of academic interest than of practical value. In a doubtful case of enteric fever we ask for a Widal, but in an obscure case of rheumatism we do not call for a Paine and Poynton. It is a seasonal disease in the sense that in England the majority of cases occur in the autumn and winter. It is met with chiefly in young men and young women who may have had one or more previous similar attacks. In children arthritis is not a prominent feature, the brunt of the disease falling on the heart. In adults there is usually a history of exposure to cold and wet, and although the onset is sudden there is no rigor. Tonsillitis is an early symptom marking the site of the introduction of some organism. There are the usual symptoms of fever, a temperature of from 101° to 103° F., with rapid pulse, loss of appetite, headache, furred tongue and constiption. The skin is not dry as in most fevers, but is covered with sweat. The

joints are early involved, the middle-sized being chiefly implicated, and they are red, hot, swollen and tender. There is effusion, but it is serous and does not lead to the formation of pus. Suppuration is uncommon, and when it occurs is due to a mixed infection or to osteo-myelitis involving the joint. Metastasis is common, the inflammation subsiding in one joint and appearing in the corresponding joint on the opposite side. There is no actual transference of inflammation, and metastasis simply means that corresponding joints are affected in succession. The sterno-clavicular articulation is not involved and the smaller joints of the hands and feet escape. The inflammation subsides completely and does not give rise to permanent disablement. The serious complications are endocarditis and pericarditis. The rashes are miliaria and sudamina in connection with the sweating, erythema nodosum and peliosis rheumatica. These cases are efficiently treated with the salicylates, the temperature falling in three or four days either by crisis or lysis. The disease may be subacute from the onset, and chronic cases are those in which the salicylates have from motives of economy been withheld or have been administered in inadequate doses.

Rheumatic arthritis may be mistaken for the acute stage of rheumatoid arthritis. The involvement of the larger joints, the occurrence of cardiac complication and the termination by salicylates are characteristic of the former disease.

(2) *Acute rheumatoid arthritis* is bacterial in origin, and occurs chiefly in young and middle-aged women. The acute attacks occur with fairly regular periodicity in the course of the chronic disease, coming on at intervals of a few weeks. There is elevation of temperature ranging from 102° to 103° F., accompanied by the ordinary symptoms of a febrile attack. The smaller joints of the hands are first attacked and other joints are gradually involved. There is but little effusion and it is never purulent. Each successive invasion is followed by greater deformity of the joints, the heads of the bones are enlarged, and there is considerable wasting of the interphalangeal muscles. The disease itself is essentially chronic and results in a general impairment in health, with, in the hands, imbrication and ulnar deviation. The temporo-maxillary joint may be involved. An initial tonsillitis is not common, and there is neither sweating nor cardiac complication. Rashes of the purpural type are not seen. Salicylates are useless, but benefit is obtained from serum treatment. The acute attacks are commonly overlooked or confounded with rheumatic arthritis. The diagnosis presents no difficulty, for the characteristic symptoms of the latter disease are absent.

(3) *Gouty arthritis or acute gout* usually occurs in middle-aged men who habitually indulge freely in alcohol, but cases are met with in women and children. It is one of the few hereditary diseases. It occurs as chiragra or podagra, the former in out-patients and the latter in the wards. The classical description is no longer applicable to hospital patients, although typical cases are sometimes seen in private practice. The acute attack comes on suddenly, but exceptionally there may be premonitory symptoms such as irritability of temper and gastric disturbance. There are the ordinary signs of fever, the temperature rising to 102° or 103° F. The joints affected are the metatarso-phalangeal joint of the great toe, the knees, ankles and wrists. Localisation may be determined by a previous injury to a joint or by some unusual strain on it. Inflammation subsides in a few days and does not lead to suppuration. After repeated attacks tophi are seen on the joints of the hand and the helix of the ear. As the acute symptoms subside there may be an attack of neuritis. Colchicum and colchicine are the remedies employed and give prompt relief.

The diagnosis is not difficult and the gouty arthritis cannot well be confounded with rheumatic arthritis. In chronic cases with much deformity gouty and rheumatoid arthritis may co-exist.

(4) *Gonococcic arthritis*, commonly known as gonorrhœal rheumatism, is in no sense a rheumatism, and strictly speaking it is not an arthritis, but a synovitis with effusion into the sheaths of the tendons. The first attack is usually preceded by a gonorrhœal discharge, but subsequent attacks may follow a urethritis in which gonococci cannot be detected, and still later there may be attacks without a urethral discharge. It is a prevalent opinion that gonococcic arthritis is essentially a man's disease but it is quite as common in women, although in them the origin is often unsuspected. The point of infection in women is the vagina or os uteri and in children the vulva. In many cases both in

children and adults the conjunctiva is the seat of inoculation. A knee or a wrist in a young married woman is nearly always gonococcic in origin. The specific organism is the *Micrococcus gonorrhœa*, first described by Neisser and subsequently elaborated by Foulerton. As the infection occurs on what is practically an exposed surface the specific organism is apt to be contaminated, especially by the *Streptococcus pyogenes aureus* and the *Diplococcus urethræ communis*. Unless elaborate precautions are taken in collecting specimens from the urethra or vagina this contamination is certain to occur. In a well-known book on clinical bacteriology it is stated that "nothing is more certain than the fact that gonorrhœa can only be diagnosed by bacteriological methods." This is indeed a startling assertion, and the author apparently hardly realises that the microscope is only one of a number of modes of examination. As a matter of fact, in a considerable number of cases of gonococcic infection with arthritis the reports obtained from bacteriologists of repute are unsatisfactory in the sense that they are inconclusive.

The interval which elapses between the infection and the arthritic manifestations varies from a few days to a week or more. The onset is not ushered in by a rigor; there is little elevation of temperature, and when it is hectic it is an indication of a mixed infection. There are the ordinary indications of constitutional disturbance, such as headache, furred tongue, loss of appetite and pains in the limbs. It is said that gonococcic arthritis is a monarticular affection and that the favourite site is the knee. These statements are not correct, for in the early stage many joints are involved. The knee suffers, but it, from its exposed situation, is liable to arthrites of all kinds, and the proclivity is not characteristic of gonococcic infection, the hip, wrist, elbow, shoulder and hand being equally commonly involved. The sterno-clavicular articulation is often inflamed, and when this occurs it should excite suspicion. The joints are red, swollen, and painful, and there is effusion of serum not only into the joints but into the adjacent tendon-sheaths. The imflammation soon becomes localised in the vicinity of one joint, the most common form being a dome-shaped swelling on the dorsum of the hand. Serum obtained from the affected part is usually sterile, and vaginal swabs as a rule show nothing characteristic. Affections of the fibrous tissue are common, involvement of the plantar fasciæ giving rise to difficulty in walking and of the lumbar fasciæ to a condition often mistaken for lumbago. The inflammation in the joint speedily results in ankylosis. Passive movements might prevent it, but the pain renders this difficult. The arthritis in spite of treatment proves obstinate, and other joints may be attacked in turn. In a recorded case the arthritis produced ankylosis of every joint in succession, and in five years the whole skeleton was implicated: the atlas was ankylosed with the axis so that the head could not be moved; all the vertebræ were ankylosed together and the hips, knees, ankles, shoulders, elbows, wrists and jaws were so firmly fixed that no movement in them could be obtained.

The disease is often confounded with rheumatic arthritis, but the ultimate localisation in one joint with involvement of the serous sheaths and the implication of the plantar and lumbar fasciæ should clear up the difficulty. The typical case is that of a man who takes a girl, æt. 16 years, for a drive in his motor car, seduces her and gives her gonorrhœa followed by gonococcic arthritis of the wrist and elbow. Incidentally it may be mentioned that carnal knowledge (C.K.) of a girl over sixteen is not a criminal offence provided that she is a consenting party, and it is not expected that she will give that consent in writing. A civil action by the girl's father for seduction and the loss of her services could probably be maintained.

A serum may be used and should be given in a series of three 10 c.c. injections in a little over twenty-four hours. An auto-vaccine is better, but commonly there is no material from which it can be prepared. A stock vaccine may be tried and is sometimes successful. It should be given in gradually increasing doses of from 5 millions to 120 millions and 200 millions at intervals of about five days. Should these methods fail there is no alternative but to fall back on drugs and local applications. Salicylate of sodium is useless, but the iodides in large doses may check the progress of the disease.

(5) *Syphilitic arthritis* occurs both in the secondary and in the tertiary stages but it is not common. Gonorrhœa attacks the joints and syphilis the bones.

Secondary syphilitic arthritis comes on suddenly with elevation of temperature, the points most frequently involved being the wrist, ankle, elbow,

and shoulders. There is considerable effusion, but it does not give rise to suppuration. It is often mistaken for rheumatic arthritis, although it is not followed by endocarditis.

In tertiary syphilitic arthritis the knee is most commonly affected and the synovitis causes little pain or constitutional disturbance. Gummatous deposits may involve one or more joints extensively, and the condition may be confounded with osteo-arthritis or with tuberculous diseases. Syphilitic joints often ultimately become tuberculous. Ankylosis may ensue if the joint is kept too long on a splint. In the secondary form inunction of calomel ointment usually checks the progress of the disease, whilst in the tertiary stage the iodides are indicated.

(6) *Tuberculous arthritis.*—Tuberculous joints are a common result of tuberculous infection, especially in children. The tubercle bacilli find their way into the joints from the enlarged lymphatic glands, cervical, brachial, or mesenteric as the case may be. These glands break down and become caseous, the tuberculous growth perforating a small vessel and pouring the organisms into the blood-current. Even when a blood-vessel is not perforated the virus is taken up by the lymph circulation, ultimately finding a resting-place in the joints. The determination of the joint affected may depend on a local injury, possibly a preceding attack of rheumatic arthritis. The joints most frequently involved are the hip, knee, ankle, elbow, wrist and shoulder. The tuberculous deposit takes place beneath the articular cartilages at the line of the epiphysis or in the synovial membrane. Caseation and suppuration follow, and a perforating abscess with a fistulous opening leading down to the joint results. This fistula affords a ready portal of entrance for other organisms, giving rise to a mixed infection in which the *Staphylococcus pyogenes* plays an important part. The effusion into the joint may be serous but is commonly purulent. The pain is often referred, not to the seat of the lesion, but to the termination of the nerves involved, and there is wasting of the adjacent muscles. The temperature remains persistently high for months at a time with frequent pulse, loss of appetite, furred tongue, progressive weakness and night sweating. Ultimately there are tuberculous deposits in other parts of the body, in the lungs and pleura, in the peritoneum and intestines and in the meninges.

The foregoing varieties of arthritis are familiar to everyone, and may be used as a standard of comparison for other varieties which are less commonly recognised.

(7) *Infective arthritis* includes those forms which follow in the wake of the acute specific diseases and may be subdivided as follows:

(i) *Pneumococcic arthritis.*—That an arthritis sometimes follows pneumonia was known long before the discovery of the pneumococcus. It may be primary or secondary, the latter being by far the most common. When primary it arises from a direct blood infection. When secondary it may follow soon after the crisis or it may be delayed for some time, even a week or ten days. It is not every case of arthritis following pneumonia which is of pneumococcic origin. The organism usually found is Fränkels pneumococcus, the *Diplococcus pneumoniæ.* The symptoms are not dissimilar to other forms of arthritis, but there is a marked tendency to suppuration. It occurs in children quite as frequently as in adults. The temperature in some cases is but little elevated, but usually ranges from 102° to 104° F. The larger joints are most commonly involved, notably the knees, hips and shoulders. The smaller joints do not always escape, and the sterno-clavicular articulation may be involved. The symptoms vary from slight pain and swelling of the joints to inflammatory œdema of the adjacent parts, accompanied by much heat and redness. The fluid obtained from the joint may be thin, turbid, or sero-purulent, or it may consist of pure pus of a thick creamy consistence. The specific organism may be detected in this fluid and often a pure culture of pneumococci is obtained. If the joint is not opened the pus may travel for a considerable distance along the tendon sheaths. In one case it penetrated from the knee for six inches upwards along the femoral artery and downwards between the muscles of the calf. In another case the knee "burst" and there was a dislocation. A selective affinity for one particular joint is sometimes shown, but it will usually be found that that joint has been subject to some previous injury. A preceding attack of rheumatism or gout may be the localising cause. Pneumococcic arthritis has to be distinguished from the gonococcic variety and also from septic conditions of the joint. The history and an examination of the pus will solve any difficulty in diagnosis. Early incision and

washing out of the joint with an antiseptic such as peroxide of hydrogen is usually followed by a good recovery. An auto-vaccine is useful, but as it takes forty-eight hours to prepare, a stock pneumococcic vaccine is employed for the first few days.

(ii) *Influenzal arthritis* was long overlooked, but is now generally recognised. Only a small proportion of cases of influenza are followed by arthritis. The hip is the joint most frequently affected. It begins as a synovitis with pain and swelling, followed by wasting of the muscles. The temperature rises to 102° F., and there may be night sweating and emaciation, but there is rarely suppuration. It runs a somewhat prolonged course and may be mistaken for a tuberculous affection. The micro-organism of influenza is rarely found in the fluid from the joints. When influenza is followed by pneumonia pneumococci may be detected, and these cases are followed by suppuration. Mixed cases of influenzal and gonococcic arthritis are recorded. Rest in bed may effect a cure, but it is safer to aspirate or to open the joint.

(iii) *Scarlatinal arthritis.*—There are two forms of arthritis included in this term:

(*a*) The joint affection following in the wake of scarlet fever and due to the specific organism of that disease. It appears from the fifth to the seventh day of the illness, when the rash is fading and the temperature is falling to normal. It affects the smaller rather than the middle-sized joints. It may go on to suppuration, giving rise to the condition known as scarlatinal pyæmia. The point of entrance is the pharynx or tonsils. The organism is usually *Streptococcus pyogenes*, but there may be a mixed infection.

(*b*) This is often known as scarlatinal rheumatism, and is a rheumatic arthritis following in the wake of scarlet fever. It appears about the third week, attacks the larger joints only, and does not go on to suppuration. The diagnosis is not difficult, always supposing that the initial disease is diagnosed. A scarlatinal rash may be due to a streptococcus, and is then usually labelled scarlet fever, but it may be toxic in origin or it may be a serum rash or an enema rash. Desquamation does not help us much, for all cases of erythema desquamate more or less, and some cases of scarlet fever do not desquamate at all. The salicylates are useful in the rheumatic form, but are of no avail in the first-mentioned variety.

(iv) *Enteric arthritis* is a rare complication, being met with in only a small proportion of cases of enteric fever. It begins as an osteitis with extension to the neighbouring joints. The hip is frequently involved, commencing with synovitis and rapid effusion into the capsule. Suppuration is rare, but there is always a danger of dislocation. Aspiration is usually necessary, and injection of the joint with iodoform emulsion, made by shaking up precipitated iodoform with 1 in 20 carbolic acid or 10 per cent. formic aldehyde solution, is advisable.

(v) *Dysenteric arthritis.* — This form is not common, but it sometimes occurs during the stage of convalescence, the knee, ankle, and sterno-clavicular articulations being attacked. Progress towards recovery is slow, and tenderness of the soles of the feet may persist for many months.

(vi) *Arthritis melitensis* follows in the wake of Mediterranean, or, as it is commonly called, Malta fever, and is due to the *Micrococcus melitensis*. The inflammation is acute and there is much effusion into the joints, although it rarely runs on to suppuration. The knee and ankle are commonly affected, but the small joints of the hands and feet may be the first involved. When the sacro-iliac articulation is implicated the pain may be very great, and as the patient is unable to turn in bed there is danger of the formation of bed-sores. Nodular swellings form on the ribs and are seen in other parts of the body. The attacks are often spread over a long period, and are not really amenable to treatment. Exceptionally effusion occurs in the tendon sheaths, the extensor tendons of the wrist and the tendon Achilles being the first to suffer.

(8) *Septic arthritis* may occur either in an acute or in a chronic form:

(i) *Acute septic arthritis* or acute purulent inflammation of the joints may be due to *Staphylococcus pyogenes* or to other organisms. The point of entrance may be a wound, or it may follow acute tonsillitis or other septic conditions of the mouth or of the ear. The worst cases are the puerperal infections, which occur as cellulitis, peritonitis, or phlegmasia dolens, and exhibit the symptoms of septicæmia or pyæmia. Many organisms are found associated with these conditions, and not uncommonly there is a mixed infection. Some of these are of a most virulent description, and infect the hands,

the clothing, and the room the patient occupies. The onset is abrupt: there is an initial rigor followed by pyrexia with malaise, restlessness, headache, vomiting, and diarrhœa. Many joints may be attacked almost simultaneously, and present all the signs of purulent effusion. Rashes of various kinds may make their appearance. The only safe course is to lay open the affected joints and wash them out freely. A serum, or still better an auto-vaccine, should be used with as little delay as possible.

Some forms of acute septic arthritis are caused by osteo-myelitis in the neighbourhood of the joint. The majority of these cases occur in children or in young adults in whom the bones have not attained complete development. The organism is usually the *Staphylococcus pyogenes aureus*, but it is not a specific infection, and may be caused by a number of different organisms acting either independently or in collaboration. The onset is sudden, there is a rigor, the temperature rises to 105° F., and before long there is an œdematous swelling of the soft parts over the bone and around the joint. A blood examination shows a large increase in leucocytes, which may number from 20,000 to 30,000 per c.mm. In its early stages this condition is often mistaken for rheumatic arthritis, and when the symptoms are more pronounced the diagnosis may be given of circumscribed abscess of the bone. In one of my cases, in a syphilitic subject, a suggestion had been made that the condition was due to acute periostitis. A hectic temperature with a rigor, acute tenderness at the end of one of the long bones and in the neighbourhood of the joint, especially if there is endocarditis as a complication, should clear up all doubt. Prompt surgical treatment is necessary and the diseased bone should be gouged out. An autogenous vaccine may be employed, but in acute cases only as an accessory measure. The usual dose is from 50 to 100 millions.

(ii) *Chronic septic arthritis.*—In this category should be included the cases of arthritis arising from chronic septic conditions of the mouth and ears. Chronic joint affections have often their origin in septic uterine discharges, even when not due to gonococci, or if gonococcic when the gonococcus has long been replaced by streptococci and other organisms. Arthritis may even follow a crop of boils or septic forms of eczema. Vaccines are of more value than in the acute form.

(9) *Toxic arthritis.*—This is admittedly not a very satisfactory grouping, and is adopted only as a matter of convenience. It is obvious that many forms of arthritis already discussed are toxic in origin. For example, rheumatoid arthritis is often due to the production of toxins in the intestines or elsewhere.

In this category should be included the condition known as pyorrhœa alveolaris or Rigg's disease, and the cases of hypertrophic pulmonary osteo-arthropathy following chronic bronchiectasis, chronic phthisis, pleurisy with effusion, empyema and malignant disease of the pleura. It is a question, too, if under this heading might not be included some of the trophoneuroses or neuropathic arthrites, such as the Charcot joints of locomotor ataxy and the gliomatous joints of syringomyelia. The method of dealing with these cases is best illustrated by the treatment now commonly adopted for pyorrhœa alveolaris. Decayed stumps having been extracted, and the dentist having accomplished his mission, a specimen is taken from the infective gums and a vaccine is prepared. This is injected, not into the gums, but subcutaneously; it is futile giving it by the mouth or rectum. It is best to begin with 20 millions and gradually work up to 400 millions. There is, as a rule, no marked reaction, and a considerable number of injections may have to be given before a cure is effected. In the majority of cases the results are extremely satisfactory, but failures are sometimes met with, probably from the wrong organism being accidentally collected. The organism commonly met with is the *Streptococcus brevis*, but there is often a mixed infection.

(10) *Serum arthritis: malade de sérum.*—This is the joint condition which sometimes follows the injection and absorption of serum. It is but one of a group of symptoms, of which the other component parts are pyrexia, loss of appetite, nausea, vomiting, and marked depression. They constitute what is called the "reaction," and may last three or four days. The rash, which may be a simple erythema or an urticaria, may spread fairly uniformly all over the body, but is most marked on the chest and abdomen, and is attended with much itching and discomfort. The joints are often distinctly inflamed and there may be some effusion of serum, but it soon subsides. There is no affinity for any special articulation, and the joints which suffer most are those most frequently used. This combination of symptoms is often met with in cases of rheumatoid arthritis in which serum is given, and may follow an injection of 30 c.c. distributed over twenty-four hours. It is common after the employment of anti-diphtheritic serum. The injection of Jersen's anti-plague serum as a protective measure may induce persistent arthritis. In serum arthritis the salicylates are useless.

It is easy to describe typical cases and to lay down rules for diagnosis, but in actual practice difficulties are met with which are not easy to solve. The following is a case in point:

A girl, æt. 21 years, was admitted on May 4th, 1910, with a history of sore throat of a fortnight's duration, followed by pain and swelling in the wrists and ankles. Examination showed inflammation of both wrists with a dome-shaped swelling

of the left and effusion into the tendon sheaths. There was well-marked tenderness in the lumbar and plantar fasciæ. No murmur, no sweating, no rash, and no vaginal discharge. A gonorrhœal origin was suspected but a vaginal swab showed no gonococci, and in fluid drawn from the left wrist there were no organisms in the films and culture was sterile. The patient was put on salicylates, the temperature fell to normal, and the joint condition subsided. On the 9th the salicylates were discontinued and the temperature rose to 103·4° F., at which it remained for twenty-four hours. The salicylates were resumed, and on the 12th the temperature reverted to the normal. The patient made a rapid convalesence and it was arranged that she should go home on the 21st. On the morning of that day the temperature suddenly rose to 104° F., there was double tonsillitis with a scarlatinal rash on the chest and abdomen. A throat swab showed cocci only, streptococci and staphylococci. The urine contained much free blood, and on examination showed hyaline and blood-casts in abundance. There was no return of the arthritis. The temperature kept up in spite of the administration of aspirin, a terminal pneumonia with right pleuritic effusion developed, and the patient died on the morning of the 25th.

The autopsy confirmed the correctness of the physical signs observed. The kidneys showed typical acute nephritis. There was no cardiac mischief and the hymen was intact. Streptococci were found in the lungs and pleuritic effusion.

The diagnosis, in spite of the care with which the case was observed, remains in doubt. It may originally have been either rheumatic or gonococcic, and presumably the patient subsequently contracted scarlet fever, but of this there is no absolute proof. There was no other case of scarlet fever on the female side, but there was a similar case in a man, and the notes were taken by the same clinical clerk, who, however, was not infected.

Although in some forms of arthritis a predilection is shown for certain joints too much importance must not be attached to this, for a previous attack of rheumatism or an injury to the part may act as a determining cause. This is exemplified by a very simple experiment. If in an animal one particular joint is for some days subjected to massage and a culture of pneumococci is then injected into the popliteal space or into the abdominal cavity the arthritis will appear first in the manipulated joint. This is also shown by clinical observation. A woman had an extensive nævus involving the left elbow. When nearly forty years of age and after her seventh confinement she had an attack of puerperal fever with which she was laid up for many weeks. It resulted in arthritis and subsequent ankylosis of the joint marked by the nævus.

November 7th, 1910.

ENLARGED TONSILS AND "ADENOIDS."

A lecture delivered at the Hospital for Sick Children, Great Ormond Street.

By GEORGE F. STILL, M.A., M.D., F.R.C.P.,
Professor of Diseases of Children, King's College, London.

GENTLEMEN,—If I have undertaken a subject which might seem to appertain rather to the surgeon or the throat specialist than to the physician, I pray you deal gently with my temerity and let my excuse be that the physician is very frequently consulted with regard to the naso-pharyngeal troubles of childhood, even though the treatment of them may pass into surgical hands.

Troubles in this region have so often a close relation to disturbances of general health and to symptoms remote from the nose and throat, that it is very necessary for every medical man not only to realise that there is such a relation, but to define it as clearly as possible in his own mind.

In the limits of a single lecture it is impossible to deal with all the morbid conditions of the naso-pharynx which are met with in childhood, so I shall restrict my remarks to two of the commonest viz. enlargement of the tonsils and adenoid overgrowth, and I have chosen these for consideration because there are in connection with them many points of practical and every-day importance.

In one sense it is a mistake to speak of "adenoids" and "enlarged tonsils" as if they were two different affections; they are really both manifestations of one process, an overgrowth of normal lymphoid tissue, and are therefore likely to be associated together. And here let me remind you that lymphatic gland tissue or adenoid tissue has a very wide distribution in the body. In the naso-pharynx there are not only the tonsils but patches of exactly similar tissue on the wall of the pharynx, aggregated in the middle line to form the so-called third tonsil or Luschka's tonsil, with outlying small patches on the lateral parts of the pharyngeal wall near the orifices of the Eustachian tubes. At the base of the tongue there are some small patches and throughout the intestine in the form of solitary follicles and Peyer's patches, whilst in the appendix there is much lymphoid tissue, and in early life the thymus in its cortical part consists

of similar tissue. The spleen also is largely a lymphoid structure; and lastly there are the lymphatic glands proper all over the body, the number and wide distribution of which is hardly realised until some such disease as lymphadenoma brings into prominence many which in health are not appreciable by ordinary examination.

But it is not only the wide prevalence of lymphatic or adenoid tissue in the body which I wish to emphasise here, but also the curious variability of its development in different individuals. Anyone who is familiar with the post-mortem room appearances of childhood will know how greatly the prominence of the solitary follicles and Peyer's patches varies in different cases without any apparent reason. Again, anyone who has tried to determine the average size and weight of the thymus gland in children of a given age will know that the variations are so great that hardly two individuals can be found to agree as to what constitutes a normal thymus in these respects. Again, examine clinically a score of children in succession, children of about the same age and apparently free from any local source of irritation which could affect the glands, and you will find that the palpability of the lymphatic glands varies in so striking a degree that it is often very difficult to say what size of glands, in the groin or in the neck, or axilla, is to be considered normal. So it is with the adenoid tissue of the naso-pharynx and tonsils; in different children it varies largely in development quite apart from any variation of health.

I have drawn your attention to these points with a practical purpose: in the first place, I wish to insist that the so-called "adenoids" are not new growths; they are simply a more or less excessive development of normal structures, which are normally present in this situation in every child. Perhaps I ought to apologise for mentioning such an elementary fact to medical men; but I do so because I have sometimes thought that medical men might do more to prevent the mischievous conception in the popular mind of adenoids as new growths, which, being of the nature of "tumours," must inevitably require operation. Excess of adenoid tissue in the naso-pharynx may be apparent within so few days after birth that it seems probable that in some cases at least it is congenital, whilst in others the differences of amount are due to variation in the development of these tissues after birth. Childhood is the age at which the lymphoid tissue is specially abundant, and it must not be forgotten that this tissue undergoes an involution in adolescence, a fact of no small importance in connection with treatment.

Lastly, I will mention here a point to which I shall refer again in connection with treatment, that the adenoid tissues of the naso-pharynx, and the tonsils, being part of the general scheme of adenoid tissue in the body, may share in such a general overgrowth of this tissue as is supposed to occur in the "status lymphaticus."

Symptoms.

Turning now to the results of adenoid hypertrophy and enlarged tonsils, I suppose that the majority of them are directly or indirectly due to simple mechanical obstruction to the ingress of air; that some are due to the catarrhal state which seems to be favoured so greatly by the presence of adenoids and enlarged tonsils; and that a further count against them is the possibility of their serving as a portal of infection.

It is hardly necessary to catalogue here the results of mechanical obstruction which may be due to adenoids; the facies, with its open mouth and its "pinched-in" nose, the heavy or snoring breathing, the stooping shoulders with the head poked forward, the contracted or perhaps pigeon-shaped chest, the nasal voice, are only too familiar. Hardly less characteristic are the catarrhal symptoms—frequent colds, deafness, at first occurring when the child catches a fresh "cold," but soon becoming persistent; otitis media, hoarseness indicating a spread of the catarrhal condition to the larynx, cough, especially at night soon after going to bed or in the early morning. Such are the more or less direct results, mechanical and catarrhal, of adenoids, and no one, I suppose, doubts their relation to the adenoids; but when we come to the supposed indirect results, there is much more room for scepticism.

It is natural enough that the child whose air entry is diminished and whose sleep is so disturbed by the difficulty of breathing that he never has a properly refreshing sleep should be pale and lacking in vivacity, may, indeed, seem stupid, and suffer with headache, and it is even possible that the nervous system under such conditions may become unduly irritable, so that the child is pre-

disposed to such affections as habit-spasm or stuttering, or to a combination of nervous with catarrhal conditions, as in spasmodic croup, laryngitis stridulosa, or asthma; possibly even the tendency to so remote an affection as enuresis may be increased by the deficient aëration and consequent irritability of the nervous system.

But to suppose, as some have done, that adenoids can produce nervous or any other affections by some mysterious reflex effect apart from any mechanical obstruction or catarrh is an assumption which seems to me wholly unwarranted.

In addition to the mechanical and catarrhal results of adenoids, there is another group of consequences, and by no means the least important, namely those connected with bacterial infection.

Look at the irregularities, the pits and furrows on the surface of enlarged tonsils: see the cheesy secretion protruding from the open crypts; or again, look at a recently removed pharyngeal tonsil, the so-called "adenoid" mass, with its folded irregular surface; it is hardly surprising that in the nooks and crannies of such a structure bacteria should find a nidus. Local infection must surely be favoured by this condition, and when such an infection as diphtheria has once taken hold everything is against the rapid disappearance of the bacillus from such a throat; it is more than probable also that under such conditions any infective disease attacking the fauces or naso-pharynx is likely to run a severe course.

Research has actually shown the tubercle bacillus both in the tonsils and in "adenoids," and Drs. Poynton and Paine have obtained the coccus which they have found in rheumatic joints and rheumatic heart lesions from the tonsils.

So far I have referred chiefly to the results of adenoid hypertrophy; now let me say a few words about the results of enlarged tonsils. These similarly range themselves under the three headings —mechanical, catarrhal, and infective. In the very large majority of cases enlargement of the faucial tonsils is associated, as one might expect from the similarity in the character of the structures, with enlargement of the pharyngeal tonsil that is "adenoids"; and the resultant evils are for the most part only an aggravation of those I have already mentioned. But enlargement of the faucial tonsils does introduce certain modifications into the picture: the voice is different, the catarrhal affection takes the form of a recurring tonsillitis, a cold always "flies to the child's throat." Enlargement of the faucial tonsils adds also to the infective risks. As already mentioned the micrococcus of rheumatism has been found in the tonsils, not, so far as I am aware, in adenoids; the tubercle bacillus has been found in both, but Dr. Hugh Walsham, in his work on the channels of tuberculous infection, says that he could not find it after many investigations on adenoids, whereas it was found repeatedly in enlarged tonsils.

Treatment.

The first principle that I wish to lay down very emphatically is this, the mere fact that a child has adenoids or enlarged tonsils is not necessarily an indication for treatment of any kind, much less for surgical interference; the one and only one criterion of the necessity of treatment is the presence of harmful results from them. I see children in whom someone with an excess of zeal for examination, as it seems to me, has passed his finger up into the child's naso-pharynx, and finding, as he thought, that the pharyngeal tonsil is more easily felt than normal, has announced that the child has "adenoids" and requires an operation, although perhaps admittedly they are producing only the slightest symptoms, and these possibly of doubtful relation to the adenoids, and sometimes, indeed, they are producing no symptoms at all.

I think that such advice is wholly bad: but before stating my reasons for this view, let me *en passant* say a word on the digital examination for adenoids. Is it necessary to poke one's finger into the naso-pharynx to see whether there are adenoids? It is, to put it mildly, an extremely uncomfortable procedure. To a child it is more than uncomfortable, it is terrifying; and I know only too well from experience of children who have been examined by a throat specialist before they are brought to me, that henceforth the very sight of a doctor makes them scream with fear.

Now I very much doubt whether adenoids which so far fail to declare themselves by symptoms as to require a digital examination to determine whether there is adenoid hypertrophy or not, require any treatment whatever: always assuming of course that the observer has sufficient experience to know what the symptoms of adenoids are: and therefore I would lay it down as a general rule

(and every rule has its exceptions) that there is no occasion whatever to poke the finger into the naso-pharynx in order to decide whether there are "adenoids" which require treatment. Every child has adenoid tissue in the naso-pharynx; the pharyngeal tonsil is a normal part of our anatomy; the question whether it is sufficiently enlarged to require any treatment should be determined solely by the symptoms to which it is giving rise.

Now to return to my previous point that adenoid tissue, whether that in the naso-pharynx or the faucial tonsil, even when distinctly hypertrophied, does not necessarily need removal.

It is to be remembered that both the naso-pharyngeal and faucial tonsils undergo an involution so to speak, after puberty, and this even where there have been catarrhal or inflammatory symptoms which might be thought to entail fibrous changes in them; no doubt the presence of such fibroid inflammatory change makes the involution after puberty less than it otherwise would be; but I wish to insist upon the fact that the occurrence of two or three attacks of tonsillitis, or of repeated "colds" as happens so often with adenoids, does not preclude the possibility of considerable diminution in the size of these structures after puberty.

If, therefore, adenoids or tonsils are producing no symptoms or are associated with symptoms which are not of serious gravity, for instance, merely with a rather greater tendency to "catch cold" than usual, or with the occurrence on one or two occasions of a tonsillitis, without any affection of the ears or other complication, there is no occasion to rush into operation.

It may be that further enlargement of these structures may occur, and produce symptoms which may make the operation necessary; but if so, the harm of waiting until this occurs is less than the harm of doing an unnecessary operation.

But, says one, if you wait surely there is risk of ear affection, which is perhaps the most serious of all the direct evils from adenoids and enlarged tonsils? To this I would reply that one of the admitted and immediate results of the operation, an occasional one it is true, but nevertheless an undoubted one, as I can testify from my own experience, is acute otitis media, set up no doubt by the inflammatory reaction in the naso-pharynx after the operation. This, however, is one of the lesser risks. The chief reason for avoiding the operation whenever possible is that, like other operations, it is not free from danger to life. I am assured by throat specialists that they have done so many hundred or thousands without a single fatal result. This no doubt proves that the risk to life is a small one; but it does not alter the fact that death has resulted. I myself could mention one in which it occurred from hæmorrhage, another apparently from sepsis, two from "death under the anæsthetic," and another in which it was possibly due simply to shock, and I could quote other cases from my own experience in which life was all but lost through this operation. No doubt such accidents are rare, but the rarity is no comfort to the parents when they have lost their child.

If it be true that "death under the anæsthetic" is to be attributed to the so-called "status lymphaticus," and if this status lymphaticus is evidenced partly, as we are told, by such an increase of lymphoid tissue as is seen clinically in enlargement of tonsils and the presence of "adenoids," it follows that the subjects of these affections are just those on whom operation is to be avoided as far as possible.

Let us therefore realise that although in comparison with some operations that for adenoid overgrowth and enlarged tonsils is a small one, it is not entirely free from danger to life; and therefore unless the symptoms produced by adenoids or enlarged tonsils entail grave harm to the child, operation is not to be recommended.

But what constitutes grave harm from adenoid hypertrophy and enlarged tonsils? Recurring earache, and still more, the slightest degree of deafness, even if it only recurs when the child has a cold, is one of the strongest reasons to my mind for surgical treatment of adenoids and tonsils; but I doubt whether one attack of earache necessitates operation; it is quite certain that a slight catarrh in the throat may cause earache which may never recur.

In some cases the chief harm resulting from adenoid or tonsillar obstruction is general ill-health: the child is pale, or of pasty complexion, partly no doubt owing to deficient aëration of the blood, but partly, I think, from lack of refreshing sleep, for many of these children with marked adenoid or tonsillar hypertrophy sleep restlessly, wake frequently, and are dull and heavy-eyed, perhaps nervous and peevish, in consequence. One of the gratifying results of operation in such cases is the marked improvement in the sleep, and I agree with parents who have told me that they thought the improvement in the child's general health was largely due to this.

Another class of cases which sometimes calls for operative procedure is that in which large tonsils are present, and there is a frequent and harassing recurrence of acute tonsillitis which can only be stopped by removing the tonsils.

Similarly there are cases in which catarrh starting in the naso-pharynx spreads to the bronchi repeatedly, so that it may be necessary to remove adenoids to prevent the recurrence of bronchitis. I have felt tempted to advise the operation in some cases of laryngitis stridulosa and asthma, where these affections were associated with adenoids and enlarged tonsils, but the effectiveness of medicinal treatment has deterred me unless there were other reasons for the operation.

Another sufficient reason for operation is deformity of the chest resulting from throat obstruction; the child with a constantly open mouth, rounded shoulders, head poked forward, narrow chest, pigeon breast or depressed sternum, is one that requires operation.

The probability of rheumatic infection through the tonsils has been mentioned; certainly it is very common to find enlarged tonsils associated with rheumatic affections. Whether rheumatic infection can be prevented by removal of the enlarged tonsils has yet to be determined. I have kept clinical notes for some years of cases bearing upon this point, but they only prove that the ordinary partial removal of tonsils does not prevent the recurrence of rheumatism in a child who has previously had rheumatism.

One serious result to which perhaps not enough importance is attached is enlargement of the glands at the angle of the jaw. Whether the tubercle bacillus gains access to these directly through the pharyngeal or faucial tonsils, or comes to them *via* the blood, I think there can be no doubt that the lymphatic glands, once enlarged, are particularly liable to become a nidus for tubercle; and therefore, especially in the child of tubercular family, or of known tubercular tendency, enlargement of the gland from these throat sources should raise the question of removal of adenoids and tonsils.

But here caution is necessary; the question is often not raised until the glands are already not only tuberculous but greatly enlarged, more or less evidently caseous, and perhaps on the verge of softening. It is a difficult point to decide whether under these circumstances immediate operation should be done upon the throat, even though the condition there obviously will necessitate operation sooner or later. There is a risk that by inducing a temporary inflammatory condition in the throat the glands may be stirred into further activity and the last chance of their subsidence be extinguished.

Probably it is best to wait and send the child to the seaside under close medical supervision. If the glands subside, all well and good; when the subsidence has reached a considerable degree the tonsils and adenoids can be removed. If the glands do not subside they can be removed, and after the wound has healed and the child has been away again to convalesce, the throat can be dealt with.

One hears of cases in which such troubles as night-terrors and enuresis have been stopped by removal of adenoids or large tonsils; I confess to a certain amount of scepticism as to the relation. I can say positively that the mere presence of adenoids or enlarged tonsils does not preclude successful treatment of night-terrors or enuresis by simple medicinal measures; I have seen it again and again. On the other hand, I have repeatedly known these operations to have no effect whatever on enuresis—sometimes apparently to aggravate it; moreover in the few instances where I have known the enuresis to stop immediately after the operation it has generally recurred subsequently.

Epilepsy, I think, is never caused primarily by enlarged tonsils or adenoid hypertrophy, but it seems only reasonable to suppose that the ill-health which is produced by any marked degree of naso-pharyngeal obstruction may aggravate the tendency in a child already disposed to epilepsy, and this, I think, is the case. I have seen a few instances in which operation on the throat under these circumstances seemed to diminish the attacks greatly.

It will be evident from what I have said that whilst the operation for enlarged tonsils and adenoid overgrowth is never to be done unless there is very grave reason for doing it, there are many cases in which such reasons are present.

But even when operation is necessary there is still need for careful consideration of the special requirements of the individual child—there is a time to operate and a time not to operate. The child's condition, apart from its naso-pharynx, must be taken into account. It is exceedingly common in all classes, and perhaps specially in these mouth-breathing children, to find carious teeth and foul stumps; the dentist should deal with such, and if possible get the mouth into a cleaner and healthier condition before any operation on adenoids or tonsils is done.

Then, again, I often see children who have recently had an attack of acute tonsillitis; the tonsils are greatly enlarged, and the question of their removal is raised. It is, I think, never wise to recommend operation very soon after an acute tonsillitis, for the inflammatory enlargement may gradually subside within three or four months to such a degree that operation is entirely unnecessary.

When a child is in a highly nervous state, as shown, for instance, by the presence of habit-spasm, it is better to wait unless there are any symptoms which make the operation urgently necessary, for there is no doubt that it entails a certain amount of nervous shock, especially to the timid, nervous child; I have seen habit-spasm markedly aggravated thereby, and I have known children to fall into a state of morbid nervousness, from which they did not recover until many weeks after the operation.

Another point which should be considered is the season of the year at which the operation should be done. Of course circumstances may leave us no choice of season, but where choice is possible I think it is undoubtedly safer to have it done during the warm months than in winter, for one of the sequelæ of the operation is a bronchitis apparently due to spread of the inflammatory reaction from above downwards into the air-passages.

Lastly, with regard to the operative treatment, I feel I must say a word on the actual doing of the

operation, and perhaps, being a physician, and having, therefore, no active part in the operative treatment, I may speak with the greater freedom. I am constantly—I use the word deliberately—constantly seeing children for conditions, generally catarrhal conditions, connected with the throat, who are said to have had their tonsils and adenoids removed. It is obvious both from the symptoms and from examination of the throat that the removal has been very incomplete; a certain amount both of the pharyngeal and of the faucial tonsils has been removed, and the stump left behind is perhaps even more ragged and irregular in surface than the original whole tonsil would have been, with the result that bacteria are still caught in its crevices, recurring catarrh with its attendant symptoms and complications still trouble the child, and the parents are grievously disappointed.

I would lay stress upon the importance of complete removal of the adenoids and tonsils by whatever method is found to be most practicable: the results of enucleation seem to me the most satisfactory, but I am well aware that in the hands of an expert tonsillotomy may be to all intents and purposes tonsillectomy; anyway, I feel sure the thing to be aimed at is complete removal of the tonsils. It is not for me to dilate upon the applicability of particular methods in particular cases; I feel sure that while no man will be equally successful in all cases, the success of this or that method depends very largely upon the individual using it.

I will even venture to insist that the operation for removal of adenoids and tonsils is not so simple an operation that anybody and everybody who happens to have a surgical qualification and a guillotine can do it as it ought to be done: it is one which, if done at all, ought to be done by the most expert skill obtainable.

So much for surgical treatment. I have said already that the mere presence of adenoid and tonsillar hypertrophy is not sufficient ground for operative interference. I have also stated that a mere slight tendency to naso-pharyngeal catarrh or an occasional attack of tonsillitis is not, *per se*, sufficient justification for operation. Is there any good to be gained from medical treatment?

The slighter catarrhal conditions associated with adenoids are undoubtedly amenable to treatment by local applications. Children of about eight years or more will use successfully a lotion to be sniffed up the nostrils night and morning; this method of application is less frightening to most children than syringing. I generally direct that a small medicine glass is to be filled with the warm lotion and the child is to "drink the lotion through his nose and spit it out through his mouth." A useful formula for this purpose is: sod. bicarb., borax, sod. chlorid. āā gr. xv, glycerin ʒj, aq. ad ℥j, (K. C. H. Pharmac.), a table-spoonful to be used with two table-spoons of warm water; or boroglyceride, a teaspoonful in four ounces of water, may be used. Probably an alkaline lotion is generally most useful as tending to wash away mucous secretion more readily. For those who are not old enough or are too timid to use a lotion in this way, a very fine spray or vaporiser can be used with one of the various apparatuses sold for the purpose. The fine sprays in which an antiseptic, such as oil of eucalyptus, is mixed with an oily base, such as liquid paraffin, are particularly useful, as the child does not feel the spray as fluid and consequently is not frightened, and the oily material remains in contact with the parts longer than a watery solution would do.

Whether the tonsils can be diminished in size by any local application is perhaps open to doubt. It is certain that they sometimes become smaller while astringents or antiseptic paints are being applied, but it is equally certain that tonsils which have become swollen by some recent tonsillitis may slowly decrease in size without local treatment of any sort, until after several months there may be practically no enlargement remaining—a point to be remembered when the question of operation is being considered. On the other hand, there are enlarged tonsils of hard fibrous appearance, the result of repeated inflammation, which are not likely to diminish in size either with or without applications. None the less I think that where tonsillar enlargement is associated with a tendency to recurring tonsillitis, a persevering application of some antiseptic or astringent twice daily for many months reduces the liability to a recurrence of the tonsillitis, and in this way, at least, favours the subsidence of the enlargement. In the slighter cases of adenoid and tonsil overgrowth breathing exercises may have some remedial value, partly directly by producing a healthier condition of the mucous membrane, and partly indirectly by improving the general health and so reducing the liability to catarrh. But the chief value of breathing exercises is after removal of enlarged tonsils and adenoids: too often one sees children losing in great measure the benefit of the operation because no directions have been given as to teaching the child to breathe properly afterwards, and the child continues a mouth-breather when the necessity is no longer present.

Lastly, the climatic environment may have to be considered. A cold, damp atmosphere, and especially the fogs which we have in London during the winter, are potent factors in the production of repeated naso-pharyngeal catarrh whatever may be the bacterial element. The ideal place of residence for children with the tendency to enlargement of faucial and pharyngeal tonsils is one which stands high and has a porous soil, allowing of rapid drying after rain.

November 7th, 1910.

ENDO-ANEURYSMORRHAPHY (MATAS'S OPERATION), WITH AN ACCOUNT OF THREE CASES.

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ENDO-ANEURYSMORRHAPHY is the treatment of an aneurysm from within. The sac is opened after arrest of its blood-current. By suture of the intima the openings of the main vessel and of other vessels entering the sac are closed, and the sac obliterated as far as possible. Under certain circumstances it may be possible and advisable to re-construct the natural lumen of the artery.

More than twenty-two years ago, on March 30th, 1888, the first endo-aneurysmorrhaphy was performed by Dr. Rudolph Matas, in the Charity Hospital, New Orleans. The case is fully reported in the 'Medical News,' Philadelphia (1). The patient, a negro, æt. 28 years, had a traumatic aneurysm of the middle of the brachial. Ligature above and below the sac was followed by return of pulsation on the tenth day. At a second operation, undertaken with a view to extirpation, the sac was freely opened, and the recurrence of pulsation then explained by finding in the sac the openings of several large vessels, besides the two openings of the main artery. It occurred to the operator that as the sac was intimately adherent to its surroundings and its extirpation would be difficult, cure might be obtained by suturing the arterial orifices from within. This was done successfully. The patient was seen and was well ten years later.

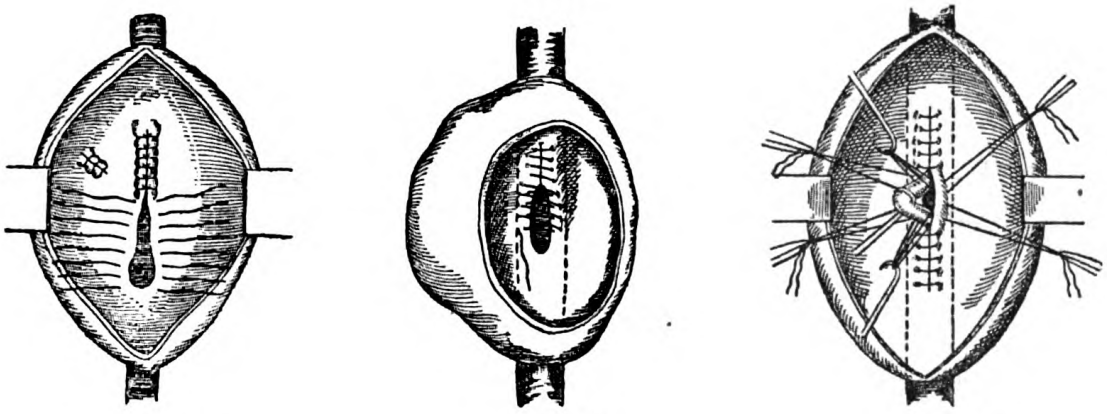
In 1900 Matas did his second operation.

Thereafter cases came, slowly at first, then more rapidly, the bulk of the operations being done by American surgeons.

Through the great courtesy of Dr. Matas, I have received an extract from his Presidential Address, given at the meeting of the American Surgical Association at Washington on May 4th, 1910 (8). In that address Dr. Matas records 145 cases of his own and other surgeons, and in a note kindly appended to the extract he mentions four more cases, bringing the total number to 149 in July, 1910.

The operation is of three types: Obliterative, restorative, and reconstructive. The obliterative operation is applicable to both sacciform and fusiform aneurysms, the restorative is only applicable to the sacciform, and the reconstructive only to the fusiform kind. The three types of operation are shown in the accompanying illustrations from Dr. Matas's address to the Alabama Medical Association (5), reproduced by his kind permission.

FIG. 1.



Obliterative. Restorative. Reconstructive.

In the obliterative operation the arterial openings into the sac and the sac itself are obliterated by suture.

In the restorative operation the single aperture of communication between the artery and the sac is sewn up, so as to leave an intact arterial lumen of normal size, and the sac is obliterated.

In the reconstructive operation the artery is reconstructed by sutures within the sac between the apertures of entry and exit, and the remainder of the sac obliterated.

Intact intima is present at and adjacent to the arterial orifices. Broad surfaces of intima can be approximated, and adhere readily like other approximated serous membrane surfaces. A groove with intact intima lies between the main arterial orifices in fusiform aneurysms and facilitates the reconstructive operation. Openings of collateral vessels, if present, are sewn up. Traumatic aneurysms acquire an intima, and can be operated on as readily as spontaneous ones. Pulsating hæmatomas resulting from recent injury are best treated by evacuation of the clot and arteriorrhaphy.

The sites and results of operation are best shown by quoting from the extract from Dr. Matas's address already referred to. "In my paper on the statistics of endo-aneurysmorrhaphy read before the Surgical Section of the American Medical Association at the Chicago meeting, June, 1908, I analysed the reports of 85 operations performed by this method. Since that meeting the statistics of the intra-saccular method have increased to 145. Of these, 127 involved the great trunks of the lower extremities from the iliac to the posterior tibial—the real testing-ground of the operative treatment of the accessible or peripheral aneurysms. These statistics include up to the present time (April 29th, 1910), 90 popliteal, 26 femoral, 2 external iliac, 7 ilio-femoral, and 2 gluteal aneurysms. Combining the statistics published in 1908 (85 cases) with the unpublished series of new cases, which have been reported from June, 1908, to April 29th, 1910 (60 cases), we find that the total 145 cases may be grouped into three operative types of endo-aneurysmorrhaphy as follows:

"(1) Obliterative group (old series 59); (new series 46) = 105 (72·4 per cent.)

"(2) Restorative group (old series 13); (new series 7) = 20 (13·7 per cent.)

"(3) Reconstructive group (old series 13); (new series 7) = 20 (13·7 per cent.)

145."

The note added by Dr. Matas contains the following:

"Since April, 1910, four more reconstructive operations (all successful) have been reported in the femoral-popliteal tracts. . . . In none of the cases has gangrene, hæmorrhage, or relapse occurred. The number of cases of all types of this operation up to July, 1910, would therefore be 149."

We learn also from the extract that four cases of gangrene occurred in the old series and one in the new series of operations—all in operations of the obliterative type. In three of the earlier four cases the popliteal vein was injured and ligated. "No gangrenes have occurred after the restorative and reconstructive cases, either in the new or in the old series, and no relapses in the cases reported of these two types since 1908."

Of the eighteen cases (145 less 127) not in the iliac-tibial tract I have only the localities of nine obtained from Dr. Matas's earlier paper. These are: Abdominal aorta, 2; external carotid, 1; subclavian, 1; subclavio-axillary, 2; axillary, 1; brachial, 2. The aortic cases both died.

In the eighty-five cases of the old series there were seventy-six recoveries and seven deaths. Amongst the recoveries were two cases of secondary hæmorrhage, four cases of gangrene, and four cases of relapse. The relapses were all in the reconstructive cases.(7)

Of the eighty-five operations in Matas's old series seventy-nine were performed in the United States and Canada, four in Spain, and two in Italy. Fifty-two surgeons operated, of whom forty-nine were American. The operation has apparently never found much favour with English surgeons. Two cases are reported by Mr. H. M. Rigby in the CLINICAL JOURNAL.(11) In his paper he speaks of one of Matas's eighty-five cases coming from an "English clinic," viz. Birmingham. The Birmingham referred to is, however, in Alabama, U.S.A.

Mr. Rigby's two cases did well, and he speaks favourably of the operation.

The cases I now record are three in number—two reconstructive (both popliteal) and one obliterative (femoral). In the first two cases a popliteal aneurysm had ruptured, and gangrene in the limb below was imminent. Gangrene necessitated amputation in one of the popliteal cases. The other two cases recovered and the aneurysms are cured.

CASE 1: *Popliteal aneurysm.*—R. M—, æt. 46 years, lamplighter, admitted Cardiff Infirmary, January 9th, 1910. Swelling behind left knee eight months. Swelling painful, with changed shape (larger, less prominent, more elongated), and leg swollen a fortnight. On examination left leg much swollen, with stretched, glazed skin; some dilated veins; no pulsation in arteries of ankle. Large diffuse swelling in popliteal space with thrill and bruit; no distinct pulsation. Circumference, left knee, 17½ in., right knee, 13 in. Operation, January 13th: Tourniquet. Sac opened vertically and much recent and old laminated clot evacuated; clot weighed altogether 10 oz. Sac had ruptured below and torn open a large space in adjacent tissues. Anterior wall thin, and bones showed through it. Two main arterial openings on postero-external wall an inch apart with groove

between. Adjacent intima smooth. Rubber tubing laid in the openings and groove, and intima closed over it by interrupted silk Lembert's sutures; tube removed before last sutures were tied. Tourniquet removed; leaking at lower end of suture line stopped by more sutures; pulsation seen in reconstructed artery. Four more layers of sutures used to close sac. Wound closed without drainage. No pulsation to be felt in arteries of ankle evening of operation. January 17th: Signs of gangrene of foot; wound healthy. January 24th: Gangrene extended; wound infected. February 1st: Amputation lower part of thigh. Wound healed with some infection. March 7th: Discharged convalescent.

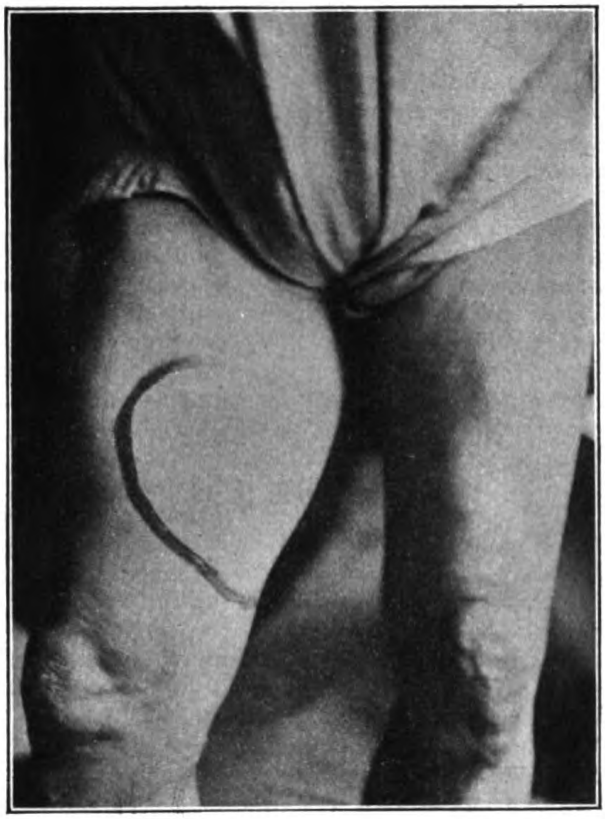
CASE 2: *Popliteal aneurysm.*—T. T—, æt. 57 years, collier, admitted into Cardiff Infirmary, May 28th, 1908. Pulsating left popliteal swelling six months. Left "dropped" foot seven weeks. On examination, aneurysm with usual signs; œdema of foot and leg. June 5th, midnight, sudden change. Man woke with great pain in limb; limb very swollen, painful, and quite helpless. Swelling increased in size hourly. June 6th, groaned continuously with pain. Temperature 101° F. Limb flexed and helpless. From knee to two thirds way down leg a red, very tender, fluctuating slightly pulsating swelling, extending almost all round limb below knee; foot and ankle swollen and œdematous; no pulsation in arteries of, and sensation dulled in, foot. Aneurysm had evidently ruptured in night. Operation immediately. Tourniquet. Sac laid open. Sac had burst below, and extravasation had extended half-way down leg beneath gastrocnemius. Arterial openings 2 in. apart, with groove between; artery reconstructed with interrupted silk Lembert sutures; slight leaking at upper end necessitated additional sutures; pulsation in new artery. Further layers of sutures could not entirely obliterate sac because of tension. Large drainage-tube put into sac and track of its rupture, and brought out through calf muscles half way down leg; main wound closed. After operation, leg warm and better colour; sensation better; no pulsation in foot arteries. Tube removed in forty-eight hours. Wound healed in a week. Eventually portions of great and second toes lost by gangrene. Long course of treatment by massage and by appliances for dropped foot.

October 6th: Patient left hospital. Still slight œdema of foot and leg, but can walk easily without assistance.

December 1st, 1908: Re-admitted with cellulitis of leg. Several incisions. Large abscess opened in calf. Wounds healed in time. McIntyre's splint used to combat knee flexion. Massage for a long period. April 19th: Left hospital with a useful leg, but still œdema and pain on much exertion. April 29th, 1910 (two years later): Re-admitted for persistent œdema and pain. No œdema on admission. May 5th: Lymphangioplasty. Four double strands of silk carried subcutaneously from ankle to hip. Wounds healed in a week, and patient left hospital on May 21st, 1910. When last heard of (July, 1910) patient still had pain and swelling in foot.

CASE 3: *Femoral aneurysm.*—W. T—, labourer, æt. 60 years, admitted into Cardiff Infirmary June 27th, 1910. Swelling of right thigh noticed one month; had grown. Pain in swelling and shooting down leg very severe fourteen days. Two nights before admission sudden severe pain and more swelling. On examination, large diffuse aneurysm inner side right thigh, with usual signs. Shown in photograph* (Fig. 2) partly outlined with Indian ink. Slight œdema of leg. Scar in groin; well-marked positive Wassermann's reaction. Skiagram: no abnormality.

FIG. 2.



July 4th: Obliterative endo-aneurysmorrhaphy. Upward and inward extension of sac amongst muscles probably due to recent rupture in that direction. Main sac filled with laminated adherent clot. Not sufficient healthy intima for reconstructive process. Sac obliterated as much as possible

* Kindly taken by Dr. Owen Rhys in the Electrical Department of the Cardiff Infirmary.

by continuous catgut sutures. Drainage-tube put into upward and inward extension by separate skin opening. Main wound closed. Primary union and excellent post-operative recovery. Patient left hospital on July 23rd. When seen at end of August, 1910, he was well. No swelling or pulsation at aneurysmal site. Slight œdema of foot on much exertion.

Remarks on the cases.—The first case was a failure, but I believe from the conditions present that such an operation as ligature of the femoral at the apex of Scarpa's triangle would almost certainly have been followed by gangrene of the leg necessitating amputation. In the second case—a very similar case to the first—the patient is slightly permanently crippled, but the aneurysm is cured and the limb saved—results I believe unobtainable under the conditions by any other procedure. The third case is well, but only a short time has elapsed since operation.

The value of the method I shall only briefly discuss. The reconstructive method must be of value in aneurysms of the aorta and common iliacs and in aneurysms anywhere where gangrene of the parts below is imminent or commencing. Whether the artery is always permanently reconstructed or not is doubtful, and is a question requiring further investigation. Temporary reconstruction may be of great value in staving off gangrene while permitting establishment of the collateral circulation.

In the ordinary aneurysm with intact coats and normal parts distally, is Matas's operation any better than ligature of the supplying artery external to the sac? Ligature at a distance, Hunterian operation or ligature close to (now known to be quite feasible), Anel's operation? Neither of these methods of external ligation certainly occlude possible smaller supplying arteries to the sac (collaterals) which may bring about return of pulsation. The operative examination of the interiors of sacs has shown that such collaterals are frequent (66 per cent., Matas).

Ligature at a distance, while more likely to occlude collaterals than ligature near to the sac, is also more likely to occlude vessels which do not enter the sac and are of value for the collateral circulation. Matas's obliterative operation occludes all the supplying arteries and all the supplying arteries only.

Numerous statistics are available showing the frequency of the occurrence of relapse, of gangrene, of circulatory and nervous disturbances and of otherwise imperfect cure after the external ligature operation. Rigby's may be quoted as an illustration. (11) Nineteen case of popliteal aneurysm operated on in the London Hospital: Of fifteen treated by ligature of the supplying artery, four showed subsequent gangrene, two of the four requiring amputation of the thigh. In one pulsation returned and the sac was excised with good result. Of the other four cases, two were ruptured aneurysms; both died, one from uræmia after excision of the sac, the other after amputation. The remaining two cases recovered after direct treatment of the sac and supplying vessels.

These statistics should be compared with Matas's (*vide supra*). Many more details may be found by reference to Matas's numerous papers.

The value of Matas's operation is undoubted. It is probably the operation of the future for aneurysms in which the circulation can be controlled. For ordinary aneurysms the obliterative operation at present appears to be the best, as the reconstructive operation is liable to be followed by relapse. The reconstructive operation should be reserved for certain special situations and conditions. The restorative operation for sacciform aneurysms is quite satisfactory.

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* *Specially reported for the Clinical Journal. Revised by the Author.*

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A LECTURE ON SYPHILITIC HEADACHES.

Delivered at the Polyclinic.

By GEORGE PERNET, M.D.,
Lecturer on Dermatology, Medical Graduates' College and Polyclinic, London.

TO-DAY I wish to talk to you about syphilitic headaches (1). In the first place I would say that, of course, headaches may be a symptom of a great variety of morbid conditions, ranging from constipation to cerebral tumour. It is not my intention to dwell upon the headaches of anæmia, Bright's disease, constipation, and so forth, or to insist on errors of refraction and other causes of headache, though all these conditions have to be taken into account in making a differential diagnosis. I need scarcely insist on this point, nor need I refer more particularly to such diseases as typhoid and influenza, and others in which headache is a symptom.

My intention to-day is, as I have said, to call your attention to headaches which occur in the course of syphilis. It is a subject which has been worked at especially by French observers, and my experience is that the matter is not so well recognised in this country as it should be, and as it deserves to be. Medical men do not sufficiently have in their minds the possibility that any particular headache which they may be called upon to treat may be the result of syphilis. They probably treat the patient with antipyrin or phenacetin or things of that sort, or, as is too often the case, the patient goes in for self-treatment by means of one of the numerous remedies which are so freely advertised in the press. It is very important that the medical man should not let the opportunity slip of treating syphilitic headaches, for the condition is not only of import in diagnosis, but is of moment from the points of view of prognosis and

treatment. By not recognising that syphilis may be the cause of a headache the practitioner often loses his opportunity for early treatment, with possibly disastrous results to the patient. I cannot too strongly insist, even by repetition *ad nauseam*, on the importance of this matter, nor on the fact, too, that the medical man must not allow the reputed respectability of the patient to stand in the way of the idea of syphilis. Over and over again I have had men say to me, especially in consultation practice, that the condition of a particular patient could not be syphilis because the patient had been coming to him for years and was utterly respectable and his reputation unblemished, just as if syphilis was something closely bordering on criminality, something very sinful in itself. Anyone may have syphilis, and if a person has a disease he must be treated for that disease, whatever his reputation may be. Remember, it is nothing to do with the doctor; it is not the doctor's duty to investigate reputations; he has to investigate and treat disease. In a recent case the Lord Chief Justice pointed out that a court of law was not a court of morals. Clergymen and priests seem to be ambitious to treat disease, but that of course is not their function—*ne sutor*. It is well to understand that syphilis is no more sinful than measles, typhoid fever, or smallpox, and to remember that it is of vital importance to the community that syphilis should be treated quickly and adequately. This feeling of reticence and that it is unpleasant to apply for treatment for syphilis to the medical adviser is responsible for so many people getting into the hands of quacks, who naturally exploit them for all that they are worth. We have all had experiences of this kind. Of course one symptom, such as headache, cannot be taken as sufficient for a diagnosis; other symptoms must be sought for, and that not only applies to syphilis, but to other diseases as well. Howbeit, there is one thing about syphilitic headache which should make you suspicious, and that is its nocturnal or vesperal character. This periodicity is analogous to what obtains in syphilitic bone disease. Osteocopic pains may be very severe when the patient is at rest in bed, but in the day-time when the patient is walking about the pain is usually not present. (I may say in passing that this nocturnal exacerbation in syphilitic headache has given rise to the idea that the parasites of the disease swarmed in the blood at that time.) Therefore, when you find on inquiry that the headaches come on and get worse at night or in the evening, you must think of syphilis as a cause. I do not advise you to make a diagnosis of syphilis on that ground straight off, but I say that if you can satisfy yourself of this nocturnal exacerbation, you will find that syphilis is at the bottom of the headache in a majority of the cases.

As far as early syphilitic infection is concerned, women are especially liable to these headaches. This is a very important fact. Women will go on for several weeks with these headaches, and investigation shows they have the disease in a florid state, yet in some instances no one appears to have taken the trouble to examine them to see if they had a rash or sore throat. The headache which they suffer from may be very severe. Some patients, when they come with these headaches in the early stages, hold their heads in describing the pain. Perhaps the reason why women come with such severe headaches of syphilitic origin is that they go longer as a rule without specific treatment than men do. Women, especially in England, are much more reticent about such things. Some are very loth to consult a doctor about anything concerning their pudenda, and will often suffer great torments and try all sorts of things before seeking advice. In hospital, even, a patient may attend with headache, is given some headache mixture until a rash is noticed, and then she is sent to another department where the character of the rash is discovered, and the whole condition recognised. It is often the case in syphilis that until she finds a rash coming out a woman will put off consulting a doctor. But, of course, in the case of women the syphilitic primary sore may be out of sight; it may be on the cervix uteri and thus escape notice. I have seen women who have consulted on account of a florid rash, but when the vulva was examined there was a state of affairs which ought to have led them to seek advice weeks earlier than they did. I remember seeing a most innocent-looking girl, whose vulva, however, was in a virulently infective condition. But even then she did not come about her vulva, but simply because she had a rash on her face. She had been going about disseminating syphilis broadcast.

Now, the syphilitic headache is not merely a localised tenderness about a particular region of

the head, such as you find when the periosteum is involved. Of course, in syphilis there may be tender parts about the head in the early stages, simply because the periosteum is interfered with. Nor is the syphilitic headache like the ordinary neuralgic headache. Sometimes there are pains shooting about the head, or on one side of the head, or about the jaw, but that is not the sort of pain which I want to draw your attention to particularly. The headache of syphilis, I mean, is usually very severe, and I cannot insist upon that point too much. So distressed are these patients by it that you can often make the diagnosis when they walk into the room. Apparently the pain in syphilitic headaches is deeply situated, and in fact it was this deep-seated character which led Fournier (2) to give it the name of encephalalgia, although of course he does not attempt to explain it. Here is a note of a typical case, and some of the cases I shall mention occurred in my practice some time ago. A woman, æt. 28 years, came under observation on account of a rash which resembled in appearance pityriasis rosea, not a marked rash of a florid or severe character such as one sometimes meets with, but she had a headache which was very severe indeed. So the relation of the rash to the headache is not a direct one. A woman may have a rash which is really mild looking and yet have a headache which is very severe; there is no relation between the two. There had been no treatment, for she had not troubled about anything until the eruption occurred. At the time she was first seen the rash had been present six or seven weeks. Mercurial treatment in that case made all the difference as regards the excruciating headaches. The patient suffered soon after from a severe syphilitic ocular condition, viz. central retinitis, a very rare condition according to ophthalmologists, with subsequent severe ocular complications of other kinds. I want to insist on this, because if the patient had been seen earlier and the diagnosis made, proper and adequate treatment might possibly have prevented the central retinitis and the hypopyon which followed. The ophthalmologists who saw the case regarded the eye as hopelessly bad. Intra-muscular injections were most valuable in this instance and saved the eye (3).

In another case there was a rash and marked adenitis, with sore throat and severe headache, which came on in the middle of the night, about 2 a.m. When patients tell you they have no headache during the day, but that it comes on at a definite time in the night, that should make you suspicious that syphilis is at the bottom of the trouble; certainly you should investigate such a case carefully from that point of view.

These severe headaches of patients usually give way to intra-muscular injections. In a case of this kind the severe headaches at night were immediately conquered by intra-muscular injections of benzoate of mercury. It is sometimes best to inject soluble preparations of mercury daily with the idea of quickly getting the patient under the influence of the mercury.

A young man began to lose his nocturnal headaches directly after the first injection. The same night for the first time he had no headache. In men, as a rule, the headaches are less severe than in women, because men as a rule get treatment for their syphilis much earlier than do women. In another case, a young woman, a factory girl, with recent macular rash and adenitis, complained of headache, which came on at 5 p.m., so that it was vesperal in character. She had headaches at no other time. As I have said, syphilitic headaches may come on in the evening.

These syphilitic headaches, then, vary in intensity, as well as in the time of their onset, though they are mainly vesperal and nocturnal. In some cases, however, the headache may not only be present at these times, but continuous, or practically so, with evening exacerbations.

Women suffering in this way look very ill. Some are probably already anæmic before contracting their syphilis, and the infection leads to more anæmia still. In addition to looking ill from pain and sleeplessness due to the headaches, they usually have an earthy tint of the face. There may also be rise of temperature, what is known as syphilitic fever, but that is a subject I have not time to dwell upon now; at some future time I will deal with it here. Therefore, if you examine a patient with a bad headache, and you find he has fever, that must not put you off your diagnosis of syphilis, because syphilis is sometimes accompanied by fever in the early stages. A patient with severe headache should be overhauled and not treated in a routine way. Remember the effects of syphilis on the circulatory system, a point which is often lost sight of. Syphilitic

cerebral arteritis may occur in the early stages of the infection, and it is therefore necessary for the medical man to be on the alert for this and be ready to act. Nowadays, as we have the Wassermann reaction and the examination of the cerebro-spinal fluid, they should be resorted to. Remember the headache may dominate the scene. In the process of excluding the primary sore in a woman you must of course examine the cervix uteri; you cannot do it by simply looking at the vulva. Moreover, it is well known that in women, too, the rash may be very slight and transitory. A woman may have only a mild roseolar rash, and that is why so many cannot give you a history of a rash. After the rash has disappeared there may be very little in the way of corroborative evidence.

At a later period we may have to deal with so-called tertiary syphilitic manifestations. Cerebral arteritis may be accompanied by an attack of headache, and this may be shown by an exacerbation at night. If this is not treated with vigour time may be lost, and the result may be disastrous. Examination of the patient, if carefully carried out, will generally reveal syphilitic mementoes, such as old scars, results of old iritis, synechiæ, leucoplakia of the tongue, Argyll-Robertson pupil, absence of patellar reflexes, etc. But apart from these the character of the headache is often practically sufficient to make the diagnosis. Where examination of the cerebro-spinal fluid and the Wassermann reaction are not possible, no time should be lost on that account in resorting to treatment. If treatment is delayed in such cases serious conditions of the brain may result.

I would like to insist on the utter fallacy of relying upon a history of syphilis. I constantly hear medical men say there was no history of syphilis in the case. When you get a positive history of a primary chancre and secondary symptoms, that may be of use to you, but a negative history is of no value at all. A negative Wassermann does not mean that a syphilis is cured—far from it. A man who may give you a Wassermann negative at one time may give a Wassermann positive three or four months later. With regard to men, I investigated a number of cases of early and late syphilis from the point of view of history; it was an investigation carried out with a certain object. The subjects of it were men in whom the diagnosis was never in doubt. In this series the history was sometimes not only negative, but in some instances the possibility of syphilis was indignantly denied, I have no doubt in good faith. It was Sir William Gowers (4) who asked me to make this inquiry from a dermatological point of view, for he wished to compare the figures with his own in regard to tabes. When I sent my figures into him I did not work out any percentages, but he worked them out, and peculiarly enough he found them come to the same percentages as his own male tabes cases—that is to say, there was a positive history of syphilis in 80 per cent. In women the history of repeated miscarriages is valuable evidence.

A typical case of syphilitic headache in late syphilis was that of a man, aged 57 years, an alcoholic to some extent. When first seen he had headaches, which were mainly localised to the occipital region. He complained that the back of his head seemed as if it were going to open. Iodide of potassium always cured these pains. He was advised to abstain from stimulants. Some time after he had turned over a new leaf with regard to his habits I met him, and he said he had not been troubled nearly so much by his headaches as formerly, and his intervals of freedom from them became longer. Iodide of potassium, 20 gr. three times a day, was found to give great relief. You will see from that that it is necessary to give good doses of iodide of potassium; 5 gr. doses are of no use. In addition to iodide of potassium mercury should always be administered, and continuity of treatment is essential. In another case of headache the patient said he had never had syphilis, but improvement followed directly intra-muscular injections were resorted to. Several courses have been given since, and he has had no return of the headaches. Although he was not cognisant of having had syphilis, the character of the headaches pointed to syphilis. No Wassermann test was made in this case, because it occurred before that method was much in vogue. If I see the case again I shall recommend him to have the Wassermann test done. In another case, on the other hand, a young married woman had headaches which were persistent and severe, but not specially characteristic of syphilis. There was no corroborative evidence of syphilis, but the headaches resisted all forms of treatment, including iodide of potassium. She had not had mercury so far as I know. I saw her twice; errors of refraction were excluded as a cause; there was no albumen in the urine; the cerebro-spinal fluid was negative. This was before the Wassermann test came out. The diagnosis arrived at was neurasthenia. This is another case in which the

Wassermann reaction will be advised if the patient turns up again. Although treatment by mercury and iodides has given good results in many cases, the headaches are sometimes very refractory and obstinate. In some cases improvement only occurred when intra-spinal injections of mercury in the colloidal form were given. Some cases of this kind have been treated on these lines in Paris after experimentation on animals, but the method employed led to alarming symptoms for some days, such as paresis of the legs, retention of urine, etc.(5) You can well imagine that injecting mercury in that way into the spinal canal is a thing which requires very great care. In patients who present early syphilitic headaches, iodide of potassium should be given for the arterial system in addition to the mercury. In the later stages iodides should be given and mercury too. Intramuscular injections of calomel or the soluble preparations of mercury are most useful in such cases where there are no contra-indications. You should not be content with mild treatment. I have seen hemiplegia result in a case of this kind which was treated by means of pills only—a quite inadequate treatment. I saw the patient for the first time a year after, but he was in a hopeless condition then. For severe syphilis I do not regard the administration of mercury pills as a proper treatment. There is no doubt that the Ehrlich-Hata remedy is most valuable, and may help us considerably not only in dealing with syphilis generally, but with the severe syphilitic headaches I have been discussing.

Tapping the cerebro-spinal fluid alone has sometimes stopped syphilitic headache (6), but specific treatment should also be employed.

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November 14th, 1910.

THE TREATMENT OF CENTRAL OSTEITIS BY METAL DRAINS.*

By CHARTERS J. SYMONDS, M.S., F.R.C.S.,
Senior Surgeon to Guy's Hospital.

GENTLEMEN,—As president of the Medical Society of London it became my duty to prepare an address, and after some consideration I decided to offer you a communication on a surgical subject rather than on matters of general interest. The subject which I propose to bring before you has interested me for some time, and finding on inquiring amongst my surgical friends that the method of treating central osteitis, which I have adopted, and found so very successful, was at least not in common use, it seemed to be a suitable matter to bring before the Society. To myself it will have the special interest of ascertaining, I hope, from other Fellows the result of their experiences.

The subject I propose to bring before you is "The Treatment of Central Osteitis by Metal Drains."

The cases submitted to treatment fall into two groups. In the first is the chronic abscess. This well-known form of central osteitis was first described by Sir B. Brodie in a paper read before the Medical and Chirurgical Society in 1832. In this communication it was that he proposed the use of the trephine. Neither in this paper nor in his 'Lectures on Pathology and Surgery' does he make any reference to the after-treatment. Nor can I find in the ordinary works on surgery any mention of the difficulties that may attend this stage of the case. The subject, indeed, seems to be passed over in a way that suggests an unsatisfactory experience. The usual practice, after evacuating the pus, is to drain by a rubber tube or to pack with gauze. All who have watched these cases will agree with me that the cavity takes a long time to close, and the most careful attention is required to prevent septic changes.

The second group contains those cases with a sinus leading into the interior of the bone. In these there is a history of suppuration some months or years before, with occasional closing of the sinus. In one the sinus was closed for four years,

* Presidential Address delivered before the Medical Society of London.

and in another for twenty years. All these patients show thickening of the bone from chronic infective osteitis and periosteitis, and form the chief number of those treated.

Two cases of chronic abscess will be related, one of which will be shown. Of these one was probably tuberculous in origin, the second—that shown to-night—was certainly the result of septic infection occurring thirty years before. Two other cases, both infective, will also be referred to, and both will be shown. These exhibit the occurrence of abscess in a bone infected in early life, the second abscess arising while the first abscess was under drainage. Both are examples, I believe, of widespread infection in early life, recovering for the most part, but leaving foci at certain points in the bone. These foci lead to painless osteitis, and only after some years reach the surface and become painful.

The morbid anatomy of chronic osteitis shows us that the bone is hard and enlarged. The wall of a chronic abscess is composed of dense bone; the wall of the cavity resulting from central osteitis of the acute or subacute variety has irregular hard walls. The compact layer of bone in these old cases of sinus has become porous from rarefying osteitis, and is blended with the new periosteal bone.

Now for the recovery of such a condition it is manifest that the redundant bone must be removed, and the parts re-modelled to the normal. The walls cannot collapse and become adherent as in the soft parts. Moreover, especially in the old septic cases, there are many small chambers and tracks in the thickened bone containing septic material. It is impossible to search out all these corners in the manner of the dentist and render the part aseptic. So that for this second group drainage will be required for a long time.

In the course of recovery two processes would appear to proceed side by side; there is absorption of all the parts away from the septic area, and softening of those parts immediately around the septic area. Hence the necessity for prolonged drainage.

The usual method of drainage is, I have said, by rubber tubes and gauze packing. My experience of this method has been no more satisfactory than that of others, especially in hospital cases. It is prolonged, unsatisfactory, is attended by recurrent septic osteitis, is very difficult to maintain, especially as a daily dressing is required. The tube is left out by some wiser and younger mind, septic osteitis ensues, and in time the neighbouring joint becomes involved. As is well known, the periosteal forming bone rapidly closes the orifice. There results a long period of invalidism, and not infrequently of poor health. In a few closure will take place perhaps for some years, but the septic focus is causing mischief all the time, though unperceived.

It was to meet these two conditions, viz. prolonged drainage and invalidism, that the metal tube was employed. And I hope to show you that such a tube maintains the opening, gives rise to no irritation, can be easily attended to by a patient even in poor circumstances, and enables the wearer to carry on his work. As all these cases occur in the tibia or femur, the importance of a method permitting the free use of the limb cannot be exaggerated. The tubes are best made of silver, but German silver does well and so does aluminium. This last-named is somewhat cheaper, and as soon as the flange can be dispensed with answers admirably. The tube at first is fitted with a flange, but as soon as the skin has closed round the tube this is dispensed with. It is important to replace the tube as soon as possible after cleaning, for closure occurs in a few minutes, and may give rise to difficulty. This has led to removal of the tube on several occasions. It is well, therefore, to fit the tube with a conical pilot especially for the patient's use. Box wood or German silver does quite well for this purpose. The best plan of all is to have a pair of tubes.

The size of the tube should bear some proportion to that of the cavity. Theoretically it should equal the diameter, but this is unnecessary. In a large cavity a diameter of half an inch is sufficient. In the smaller cavities it is well that the diameter be of a larger ratio to that of cavity. After six months or a year the tube can be reduced in size, the reduction depending upon the extent to which the bone has moulded down. Finally, a rod of metal having a flat nail-head is worn for some time, or for cheapness a length of silver wire, such as is used for uniting the patella, with the end turned over. At the operation the diameter and length are measured by a piece of large rubber tube. This is sent as a pattern, a similar piece

being temporarily used as a drain. These solid drains answer quite well, and I think might be used after the first six months. Practically all that seems necessary is a vent for a small amount of secretion, and this can escape by the side of the rod. The tubes can be made or drawn by any instrument maker or working silversmith. Most of those I have used have been made by Messrs. Down Brothers.

I will now show you some of the cases in which this treatment has been carried out, giving a brief description in each instance.

CASE 1.—The first case I propose to bring before you is one that well illustrates the treatment of chronic abscess. This woman came under my care in Guy's Hospital in December, 1908. Her age was 42 years. She came for swelling and pain in the lower end of the right tibia, or "ankle," as she called it. For three months the pain had varied in degree, but was never so severe as to cause her to lay up. The enlargement pointed to a much longer history, and on being pressed she acknowledged that for ten years there had been rheumatic pains from time to time. The lower end was enlarged and was tender, though there was no specially tender spot as described by Brodie. The X rays showed in the centre of the enlarged lower end a clear space, indicative of the abscess. When the bone was trephined from the inner side pus was evacuated at some depth, the most superficial part of the abscess being on the side next the fibula. The pus was thick and yellow, quite unlike that due to tubercle, and there was no sequestrum. The walls of the cavity were hard and smooth, and the lower part was close to the ankle-joint. A silver tube, half an inch in diameter, was inserted. The flange rested on the skin, and the deeper end did not quite reach the bony wall. This is the most comfortable setting for a tube. This patient did not walk about so soon as was intended, partly on account of the Christmas season and partly because I was away. However, she soon resumed her work, which involved a good deal of walking. The tube gave rise to no trouble, and she attended to the dressing herself, removing the tube daily and boiling it. The first tube was removed and a smaller one introduced in about twelve months. The tibia slowly diminished in size, and at the present time, one and three quarter years from operation, is, as will be seen, nearly normal (*see* Fig. 1, Supplement).

The history throws much light on the pathology of the disease in this and in many other cases. It shows that the cause, there can be little doubt, is a streptococcus infection in early life. At eleven she had an acute illness with necrosis of the left tibia, a smaller loss of bone from the lower end of right femur and both humeri. In 1906, when she was thirty-nine, a small sequestrum separated from the old site in the lower end of the femur. Since the abscess on the right tibia was opened, a sinus leading into a small cavity has appeared in the old site in the left tibia.

I take it that the septic deposit that led to the abscess took place during the original illness at eleven and remained quiescent for many years. Then, very slowly, acting more or less as a foreign body, it set up osteitis, which became painful as the surface was approached, the time occupied in the process being over thirty years. It is to be noted that at no time was the pain of that deep-seated agonising form said to be present in such cases. Time will not permit my dealing further with this interesting part of the case, or of comparing the history with those so-called cases of rheumatic fever, in which over a period of twenty or more years one sees small sequestra separate from the surface of the bones. In one case I counted as many as a dozen scars.

In this instance pain was not experienced for nearly thirty years; nor had she observed the gradual enlargement of the bone, a circumstance she attributed to the use of shoes.

CASE 2.—The second case was a solitary lesion, and occurred in a lady, æt. 30 years. She was a healthy woman otherwise, and though somewhat spare, showed no signs of a tuberculous tendency nor any old lesions. She came to see me in May, 1905, with an abscess over the lower end of the left tibia, which appeared to be periosteal. The history given was, that in April, 1904, she fell into a grate and bruised the skin. At the site of injury a swelling appeared and remained tender; in February, 1905, the same part was struck by a cycle. The swelling enlarged, and the leg became "inflamed." A month later, after another blow, the swelling still further increased. She then had to lay up on account of the pain, and had the part treated by hot poultices.

On May 17th, 1905, when first seen, there was a fluctuating swelling over the shin, the skin was

dusky-red, the part tender, and there was some œdema, so that the question as to enlargement of the bone could not be decided.

An incision evacuated thick pus of a tuberculous character, but no opening into the bone was exposed. A gouge, however, passed with very little pressure into the medullary canal, entering a cavity of considerable size. This extended towards the articular surface, and from the lower part a rounded and yellow sequestrum was removed. The walls were everywhere hard and smooth, and a layer of new periosteal bone overlay the front of the tibia. The condition appearing a suitable one in which to attempt immediate closure, the cavity was carefully cleaned without removing the layer of dense bone, then wiped out with pure carbolic acid, and the skin brought together. On the fifth day serum exuded, and a sinus persisted. Closure of the sinus from time to time was followed by some return of the swelling and pain, and to prevent this a small silver drain was inserted in September of the same year (1905). This tube the patient soon learned to remove and clean daily. The immediate effect was an improvement in the condition of the skin, the disappearance of the granulations, and a reduction in the amount of the discharge. The fusiform enlargement of the tibia began to subside, and she was soon able to walk without any pain.

At first the tube was about the size of a No. 10 catheter. It was reduced in size after six months, and finally a small solid rod having a flat head was substituted. For two years she wore this drain, and by this time the tibia had reached its normal dimensions.

This patient came to see me in July, 1909, having had a blow on the old situation. I found the parts quite sound.

Besides the question of treatment, that of the pathology of this case is of great interest. Was the whole process started by an injury twelve months before the operation?

I am inclined to think that the sequestrum had existed for some time and was a result of early epiphysitis. Looking at the case in the light of others that will be mentioned this seems the more probable explanation, and is confirmed by the rounded character and yellow appearance of the sequestrum as well as by the smooth and dense condition of the wall. That the patient had not observed an enlargement is compatible with such an explanation, for the same point occurred in regard to another case. There was no history of an acute illness in this instance, as will be found in other cases, so that I am disposed to regard this as an example of tuberculous epiphysitis.

Case 3.—Perhaps the value of this method was best shown in the case of a young man, æt. 19 years. In July, 1891, he had what was supposed to be an acute periostitis of the lower end of the femur, associated with sharp pyrexia. The abscess was opened and recovery was rapid, so that in August he did light duty and even danced.

In September pus slowly accumulated without pain, and was evacuated through an incision on the outer side of the lower end of the femur; a surface of hard bone was found, but no sequestrum. The limb was kept in a splint, and he left hospital on crutches with a sinus and much thickening of the bone.

In February, 1892 (seven months from the onset), this young man was brought to me for an opinion as to the cause of the osseous disease, the possibility of recovery within a certain time, and as to the best course to pursue. These questions were of the first importance, for he had been told, and it was feared, that the disease was tuberculous, would take a long time to recover, if it ever did, and that his prospects of remaining in the Navy were small.

There was a sinus with the usual projecting granulations: the lower third of the femur was much thickened and enlarged as much on one aspect as on another. The movements of the knee were not painful and were fairly free. On probing, bare, but no loose bone could be felt, nor did the examination reveal a central cavity. The patient seemed quite clear that the bone had diminished in size, and on this information it was decided to wait before carrying out the operation I had proposed. I took the view that the condition was not tuberculous, but was a subacute central osteitis, and that the sinus led into a cavity. I could not altogether exclude the view that the enlargement might be due to periostitis and might subside. No improvement having resulted the sinus was enlarged, and a channel found running into a cavity with thick irregular walls. No loose bone was found, though a careful search was made along the epiphysial line and the line of the

compact layer of the shaft. A large metal tube was introduced and he was allowed to get about on crutches. This precaution was taken as the cavity was large enough to weaken the bone. After a few months the femur had moulded down most satisfactorily, and he was able to walk and to bend the knee with the tube still *in situ*. He was, after a year, able to re-enter the Navy, and rising rapidly became the youngest commander of his time. When seen in 1908 there was very little difference between the two femora.

I believe the history of this case to be not uncommon, and to indicate invariably central osteitis, not periostitis, and that early drainage of the bone by a metal tube will bring about a more rapid recovery than any other method. It must be admitted that whatever be done it will not be possible to remove completely the septic focus, and that consequently allowance must be made for the continuance of a certain amount of osteitis and periostitis. If during this period free drainage be maintained by a tube that will not of itself favour septic change—as I believe rubber does—the extension of the osteitis will be limited. The summary of this case is worth setting down :

An onset which must be described as subacute when compared with those cases of early life. Rapid healing after the first incision, permitting the man to resume duty, the femur during this period becoming enlarged. Recurrence of suppuration without pain or fever. Persistence of a sinus with progressive enlargement of the bone. Finally rapid recovery after the maintenance of free drainage combined with free exercise.

Case 4.—A further illustration of the same form of disease and of the danger to the joint from the existence of a sinus is the case of a man, æt. 32 years. In November, 1906, he had a dull pain in the lower third of the femur for three or four weeks. Some days before this his horse had fallen and his thigh had been slightly injured. It was trivial, and he had forgotten the incident until pressed for the history of an injury. Slowly a swelling formed and was opened ; a good deal of pus was evacuated and a search made for dead bone. The surgeon who carried out this procedure in India wrote me and said he had looked upon the case as one of acute periostitis.

In March, 1907, he arrived in England with a sinus on the inner side of the thigh discharging thin pus, and a marked enlargement of the femur. The movements of knee were much limited and he used crutches. A probe encountered bare, hard, irregular bone, and entered well into the centre of the shaft.

I looked upon the case as primarily one of central osteitis without necrosis, and that the present condition was determined by the septic contents of the cavity. The joint movements were limited but there was no synovitis, so that if the cavity which I took to exist could be drained there was a good prospect of recovery, and of his being able to rejoin his regiment. The sinus being on the inner side it might be difficult for him to ride while wearing a tube, but this I thought could be met by a suitable pad. The radiogram showed the bone to be greatly enlarged in its lower third, but there was no evidence of a cavity. Rough, hard bone was exposed and an aperture found leading into the centre of the shaft, where a cavity of considerable size existed. The track was enlarged till the forefinger passed in easily. The walls of the cavity were irregular and were covered mostly by granulation-tissue. Projections of bare, hard bone could be felt but no sequestrum. A metal tube as large as the forefinger and reaching to the bottom of the wound was introduced. The case seemed therefore to be one of subacute osteitis giving dull pain for three or four weeks before pus reached the surface. Occurring in a man over thirty, without the formation of a sequestrum, with no early history of a septic illness and without any serious injury, it was difficult to arrive at the cause. One can only presume that the fall had produced concussion of the bone with rupture of a vessel, and that with the blood escaped an organism of low infectivity, harmless while contained within the vessels.

He was sent away within four weeks and encouraged to move the knee and to begin walking. Three weeks later he reported that with daily massage the knee could be bent nearly to a right angle. At this date the silver tube slipped out, and re-introduction being difficult, a rubber of smaller size was substituted by the surgeon in charge. Hearing this, I advised him to come up at once, when I found the sinus less satisfactory, with more granulations at the orifice. A fresh silver tube was inserted. There was a great tendency for the tube to be thrust out by the movements of the muscles, and as the cavity

extended close to the outer surface of the shaft, I thought it would be better to drain from this aspect. On May 31st, through an incision on the outer side, the cavity was opened and a large drain inserted. This in the end did not prove so satisfactory a site for drainage as the inner, for there was a still greater difficulty in retaining the tube. On this account it was returned to the inner side. The patient was able to attend to the dressing and see to the sterilising of the tube.

In October, 1907, about eight months after the first operation, I found the cavity distinctly smaller and a marked diminution in the size of the femur. He was walking long distances, playing golf, and taking various kinds of exercise. A rather smaller drain was substituted about this time. He went on well during the winter, and just a year from the operation he could carry out all the ordinary duties of life. Had he been in a foot regiment there was no reason as far as effectiveness went why he should not rejoin, but being a mounted officer, riding would be difficult, and, moreover, he could not pass the Board with a sinus.

Now comes an instance showing the danger of removing the drain. In July, 1908, the tube was left out on the suggestion of a medical man, and for a little time all went well, but towards the end of August he had pyrexia with general pains, not specially in the femur. However, pus formed and was evacuated by incision through the scar and a counter-opening behind. The incision in the scar healed, but a sinus behind remained. In this condition he returned, with some loss in the movements of the knee. I reopened the scar and found the cavity smaller, but still irregular. There was a marked decrease in the size of the bone. A further search was made for a sequestrum with negative results. The slow progress suggested the existence of a sequestrum buried somewhere in the centre of the thickening bone, or of possibly a small amount of non-irritating pus. I therefore impressed upon him the necessity of wearing a small drain. With a view of aiding its retention, a bulb or enlargement was added, and this for a time did well. I had given a forecast that it would be about eighteen months before he could dispense with a drain. That limit had now been reached and a drain was still necessary, though he could do anything, and even ride. I advised him, if he could obtain the consent of the Board, to rejoin his regiment and protect the sinus with a pad, as he was in every other way efficient. A radiogram taken at this time showed an irregular thickening above the original cavity. My fear was that in this situation a small cavity existed, which might some day give rise to trouble.

In June, 1909, he left for India, wearing a solid metal drain just sufficient to keep the track in the bone open. It was fitted with a bulb near the deeper end to prevent its escape, and he was directed to leave it alone, merely dressing round the tube once a day. I feared a tube would be removed by someone not so impressed by the necessity of drainage. In a month came a letter from the Army surgeon saying he proposed to remove the tube and dress with gauze packing to make the sinus heal from the bottom. I quote this remark to show the view still held on the subject.

In the early summer of 1910, two years and three months from the first introduction of the metal drain, this patient returned from India. He had been able to ride and carry out all his duties, the knee could be flexed to the full extent, and though there was still a dark shadow above the old cavity suggestive of a relic of disease, the general moulding had been so satisfactory that I thought the time had come for the removal of the drain. It had been reduced to a piece of silver wire about the size of a No. 1 catheter. When last seen in September, 1910, he was quite well.

A case of this kind shows the necessity of trephining and draining the centre of the bone when making the primary operation. A history of dull pain for three or four weeks unattended by swelling is not consistent with a simple periostitis. Had this been done there can be little doubt much time would have been saved—a remark equally true of the previous case, where the course was rather more acute.

Case 5.—I will next take two cases illustrating the value of the metal drain in old, and I may say neglected, cases of central osteitis. The first is that of a poor woman, æt. 64 years, who came into Guy's Hospital in June, 1907, for amputation through the thigh. This had been advised on account of a swollen and painful knee, associated with a sinus on the inner side, just above the joint. On examination, however, the knee was not particularly painful, and the woman could do a day's charing. Moreover, the sinus occupied the centre of a scar. representing the site of an incision made when she she was a girl of fifteen for the evacuation of an abscess. At this time she had pain in the knee, but walked with it for two weeks, when the incision was made. For nine years she was free from pain,

the sinus discharging all the time. Then she began to have pain in the knee and swelling from time to time, associated with cessation of discharge. Three days before admission she had one of these attacks of pain and swelling. So that for close on fifteen years there was a sinus, and for forty, recurring attacks of septic infection, gradually leading to destructive changes in the knee.

A probe entered the interior of the bone, which was greatly enlarged in its lower third, a condition well displayed by the radiograph. The knee contained some fluid, and showed chronic marginal changes, and the movements were a good deal restricted, but it was a useful joint and she could work on it. This being just the case for a trial of the metal tube, the sinus was opened up and a large cavity found in the interior of the bone, but no sequestrum. The walls were hard and irregular, with pockets of varying depth. A large drain, having a good half inch internal diameter, was inserted, This was made of aluminium, the flange being rivetted to the stem (*see* Fig. 2, Supplement).

The improvement was most satisfactory, for within a few weeks there was a marked reduction in the size of the knee, an increase in its usefulness, and an improvement in the general health. It is now three years since the operation, and you can see the great improvement that has taken place. There has been no attack of pain or swelling since she left the hospital, the femur is smaller, the joint more movable, and she can do a full day's work when she can get it.

Though a very poor woman indeed, she removes and boils the tube, and applies a dressing herself. At first this was done by the district nurse, but now she can carry it out quite well. I would call attention to the absence of granulations and the small amount of suppuration.

The tube was changed after a year for a smaller one which is now worn. It may be further reduced, but looking to her age I doubt whether it will ever be possible to dispense altogether with a drain, even if reduced later on to a rod of silver wire.

Time will only permit a passing reference to the pathology of this interesting case, and it will suffice if it be placed with the two previous examples. These three form an interesting series; all were subacute and occurred at different ages, this last at fifteen, another at nineteen, and a third at thirty, and, moreover, the lesion was in all a solitary one. In this last particular, these cases stand out in contrast with others where the infection was general and the lesions multiple.

Case 6.—Similar in type, the next case, while illustrating the value of this method of drainage, shows the evil of removing the tube too soon. This patient was a man, æt. 48 years, in fair circumstances, who stated that he injured his "knee" when eighteen. This was followed by suppuration in the lower third of the femur, and for three or four years he went about on crutches. Several small pieces of bone came away during this period, after which the wound healed and remained closed for twenty years. The leg was half an inch shorter than its fellow and the knee somewhat stiff. In 1901, when forty-four years old, he was laid up for several weeks with swelling of the "knee" and thigh as the result of a twist. He recovered without the escape of pus, but a year later a sinus formed in the old scar and has been discharging ever since. In July, 1905, when first seen, there was a sinus on the inner side through which a probe could be passed to the centre of the bone. There was marked and irregular enlargement of the lower third of the femur. The knee-joint was swollen and the movements restricted; it could not be brought quite straight, and showed the chronic changes resulting from repeated attacks of synovitis. He was subject to attacks of pain and swelling in the knee and thigh, during which he felt feverish and ill, and after each attack the range of movement of the knee decreased. This, again, was a typical case of central osteitis, notable especially for the closure of the sinus over a period of twenty years. Though he gave no particulars of this period, there can be little doubt in the light of the later history that the leg was never comfortable, and that there was pain of a kind from time to time—in other words that there was a septic focus in the centre of the bone. Besides the local condition, this patient, now æt. 48 years, looked pale and flabby, and complained that his brain power was not so good as it had been. He was, in fact, anæmic, and though lardaceous disease was suggested by his appearance there was no albuminuria.

In September, 1905, after a good deal of persuasion, he submitted to operation. I opened the sinus and found a large cavity in the bone, the walls rough and bare, but no sequestrum was met with. A silver tube, ¾ in. outside diameter and 2½ in. long, was introduced, and he was directed to walk and resume his duties as far as he could. Four months later he reported himself as much better in general health; he had more energy, and looked a healthy colour. The knee was smaller, and the range of movement had increased, while there had been no further septic attacks. The femur had somewhat diminished in size, and the cavity seemed smaller. Though deriving so much benefit from the free drainage, which did not interfere with his work as an architect, he found the tube a trouble, and with the consent of his medical adviser, removed it. Very soon the old septic attacks returned, the joint movements became restricted, and the general health suffered. I was allowed to re-introduce the tube, and again he improved. After a few months he removed the tube again, so that there has been no drainage for over two years. Writing in reply to an inquiry last

month (September, 1910), he says that there is still a discharge of pus, that he has occasional septic attacks with increased stiffness of the knee. Unless he submits to the use of a metal drain there can be little doubt that the general health will decline. The interesting part in the history of this case is the closure of the sinus for twenty years, then an attack of septic infection without the escape of pus, and later, the formation of a sinus. In this particular one may compare the case with that of the chronic abscess related as Case 1.

During the twenty years the sinus remained closed the septic focus was there, and acting as a mild irritant kept up a chronic osteitis, which led to thickening of the bone and gradual joint changes.

Case 7.—One other example of the effects of early osteitis I would like to bring before you, partly because it illustrates the usual history of such cases, and partly because I am able to show you the patient, that you may see what he can do with the metal drain in his tibia.

This man, Gavin M—, is now æt. 32 years, and says that at thirteen he was laid up with acute necrosis of the tibia. Incisions were made, and later bone was removed, and later still fragments escaped. He was able to work for some years without much inconvenience, though always having a sinus. He came into Guy's Hospital in 1901, when he was twenty-four, with a sinus and some enlargement of the lower end of the tibia. He was operated upon, the sinus enlarged and drained, and after two months' treatment was discharged wearing a splint. This man came under my care first in 1905 at the age of twenty-eight. There was considerable enlargement of the lower end of the tibia; a sinus discharging a good deal of pus existed on the outer side, and there was some œdema about the ankle, with excoriation of the skin. He had not been able to do any work on account of the sinus. A cavity in the bone was found, the aperture enlarged, and a metal drain, two and a half inches long, introduced. He was told he might work, but must retain the tube, and for a time he remained well. Falling into other hands the tube was removed after six months, and for four and a half years he remained well and worked. Then pain and swelling recurred; an abscess broke through the old sinus. He returned to my care in the present year, 1910, in much the same condition as in 1905. The sinus being on the outer side over the fistula, I opened the cavity through the tibia in order to shorten the distance to its centre, and introduced a metal drain. The infection of the skin very soon disappeared, together with the œdema, and the bone already shows some reduction in size. This man is doing his full work as a motor-bus driver, using the breaks with his bad leg. So long as he retains a metal drain till the bone has assumed the normal size, and, indeed, for some time after, the outlook is good. The fear is that some one may advise the removal of the drain before it is wise. It is so very natural to assume that a piece of silver wire is not of much service and may be dispensed with. Yet experience of these cases shows that it takes a long time for all septic material to be removed. In this man I fully expect it will be three years before the bone will be sound. His condition is not so satisfactory as it should be, the skin showing some superficial ulceration. This is due to the fact that the dressing is made every two days only. I have directed him to remove and clean the tube after his work is over, and this, I believe, will get rid of the cutaneous troubles.

There is another variety of acute central osteitis in which the attack is more acute, and affects the greater part of the bone. These are the cases of acute necrosis occurring in childhood. The special point I wish to lay stress upon in this variety is the formation of central abscess some years after the primary infection. Several of these patients have been treated by metal drains, one of whom is here for examination. Her case is of interest in that she came in with a diagnosis of tuberculosis of the tibia, a view supported by the fact that she also had a fixed and dislocated hip.

Case 8.—Annie H—, æt. 25 years, came to Guy's Hospital in 1906 with a sinus in the upper end of the tibia associated with considerable enlargement. This had existed for eight years, during which period there had been repeated attacks of pain and swelling associated with general illness. The knee was movable and had not been affected by the septic attacks. The radiogram shows, besides the enlargement of the upper end, where the sinus existed, a swelling lower down the shaft. This was looked upon as probably another septic focus. Four years later, as will be presently related, an abscess was opened in this situation. The left hip was displaced, but there was no scar of former suppuration. It appears that at the age of twelve she had an acute illness, in which her sister tells me her life was despaired of. From this she recovered with a stiff hip, the loss of the sight of one eye, and of the hearing in one ear. This is the history of an acute septic process, not of tuberculosis. At the time I thought myself that this case was tuberculous, a view strengthened by finding the wall of the sinus soft instead of hard. The cavity was drained by a metal tube, and was followed by a cessation of the septic attacks, a decrease in the discharge, a diminution in the size of the bone, and a marked improvement in health. Her work as a cook was not interrupted by the tube, which she looked after herself. After a year the tube, was left out, but the attacks recurred. In July, 1908, at the expiration of a second year, removal of the drain was again followed by recurrence with the formation of a large abscess. Since then she has always worn a small rod of silver wire.

In 1909 this patient came into hospital during my vacation with pain and swelling at a spot two inches below the old sinus. An incision was made, the bone drilled, and though pus was not found, the condition settled down. The following year, that is, the present year, 1910, this patient returned with recurrence of pain in the same situation, and on this occasion the pus was found and evacuated. As mentioned above, the radiograph taken in 1906 showed the tibia to be thickened at the spot where the pus was found, and it is possible that the presence of this septic focus has had a good deal to do with the non-recovery of the first focus.

The occurrence of an abscess a long time after the initial infection has been referred to in other cases. This instance points to the existence of a focus quiescent for over thirteen years.

CASE 9.—A similar though more severe case I would allude to, because, as far as I can gather, it was the first in which I used the metal drains. This man, Penn by name, came first to see me in 1893, when he was thirty years old. He had a sinus in the lower end of a tibia showing the typical features of an acute necrosis of early life. Since 1883, when the acute attack occurred, he had been more or less under treatment, and had had several operations. The ankle was fixed in a position of extension, and though the leg was weak it served him while he worked as a clerk. The sinus was enlarged and a cavity found in the bone. Into this a silver tube, carrying a flange, was inserted, and was worn continuously for three years. An attempt at removal was followed by an accumulation of pus. The tube was replaced, and was worn for some fifteen years. Another abscess formed in the upper end of the shaft. This also was drained, so that for some time two metal drains were in use. At the present time—seventeen years from the insertion of the first tube—he shows his condition to be satisfactory. For two years he has been without discharge or a drain, the first time since his initial attack in 1883. Even now the thickening seen above the site of the first drainage suggests the existence of a quiescent focus.

If this be the case an adequate explanation of the duration of the drainage that has been required is afforded.

Except for the week or two necessary for the insertion of a drain his work has not been interrupted.

It is not uninteresting for a moment to glance at the cases from the pathological standpoint. It will be noticed that nearly all were due to some form of acute or subacute infection. In several the disease occurred in early life, limited in some to one bone (Cases 3, 4, 7), in others several bones or other parts being involved (Cases 1, 8). Next come two cases of subacute infection, the one at nineteen (Case 3), where there was dull pain for some days, the other at thirty (Case 4), in which three weeks elapsed before an incision was made. In both these the lower third of the femur was the part affected, and in both there was a history of very slight injury. As to the cause of such cases one can, in the absence of bacteriological evidence, only speculate. Probably a staphylococcal infection may explain such cases, or the bacteria of pneumonia or influenza.

Then come those interesting cases of multiple lesions in the same bone, as in Cases 8 and 9, suggesting a widespread but attenuated infection, so mild that it may lie almost dormant for years, and give rise at intervals to local abscess over a period of twenty years or more.

Is there any other explanation for such phenomena? I do not myself see any other explanation, and I conclude that in such an example as Case 1 the septic focus must have existed close upon thirty years. In one instance of an abscess containing two small, rounded, black sequestra, there was a clear history of sixty years.

Then come the cases of long-standing sinuses with enlargement of the bone from a chronic septic osteitis and periostitis, associated in some with secondary joint changes, and all examples of acute disease in early life. In these, perhaps, one sees the most striking results from the metal drains (Cases 5, 6, and 7).

Lastly, the tuberculous cases. I have referred to one only (Case 2), and I am by no means confident that this was tuberculous. There have been others, notably that of a little girl æt. 6 years (Vera), who was also treated by bismuth and made a rapid recovery. One point I would like to insist upon is that several cases sent in for tuberculosis, and on a superficial examination appearing to be of this nature, proved to be due to other causes. I think in all probability a good many such conditions have been set down to tubercle without sufficient evidence. The error is due, I expect, to the need of some other and reasonable explanation for the recurrence of abscesses and necrosis throughout a period of ten or twenty years. The resemblance is in many cases somewhat close, as in Case 8, where there was an ankylosed hip.

The explanation I have suggested, that these foci were all deposited during the initial attack of acute osteitis in early life, and have remained more or less dormant, seems an adequate explanation, and one in accord with what we know to occur in other localities, as, for example, in the brain. It is further supported by several cases related in this paper, where a sinus has remained closed for several years, with the septic focus still there, but quiescent. These foci are never really quiescent, for a slow, painless osteitis associated with organisation is constantly going on. Some day the surface is reached, and then there is pain. The protective layer of dense bone forming the wall

of these old abscesses is the best evidence of chronicity.

The duration of the drainage required will depend a good deal upon whether it is begun when the abscess is opened or whether it is undertaken after a sinus has existed for some time. For example, a wide drain, say half the diameter of a cavity, inserted at once, will so prevent septic osteitis as to lead to rapid remodelling of the bone. If a sinus has existed for some months or years the septic changes in the pus will have led to further osteitis with increase of periosteal bone, taking a long time to recover, and the formation of pockets in the medullary canal and in the rarefied bone.

The duration of drainage is also influenced by the localisation of the disease. Where the abscess is chronic from the first and solitary, as in the tuberculous varieties, and the bone enlarged in an elliptical fashion, reduction soon follows; but where the osteitis is of an acute or subacute septic variety, recovery is delayed by the septic infection of the neighbouring bone, there being no limiting dense wall.

In some cases recovery is delayed by the congestion of the neighbouring osteitis, due to the existence of other septic deposits (as in Cases 8 and 9), which may give rise to abscess even ten or twenty years after the primary disease (Cases 8 and 9).

The duration of the drainage is, however, of less moment if the patient can go about his work and if the wound can be kept free from septic changes.

The cases show that a man can do a full day's work with a tube of fair size in the lower end of the tibia. In another a very poor woman is able to attend to a large metal tube worn in the lower end of the femur, and carry on her charing.

The age of the patient is also an important factor. In two very similar cases, the one æt. 19 years (Case 3) and the other æt. 30 years (Case 4), the latter took nearly a year longer to recover.

In cases of very long standing, as in the old lady shown to-night, where a sinus has existed and secondary changes have been going on for fifty years, a tube will have to be worn permanently.

Of other methods of dealing with central osteitis, whether tuberculous or of other origin, I have myself made trial of two.

Where the bone is superficial, as in the lower end of the radius, it may be possible to remove the wall and create almost a surface and thus prevent pus decomposition by gauze packing. This plan is, however greatly aided by the insertion of a short metal tube into the lower part of the cavity, and in this way I have obtained fairly quick results. Again, in the lower end of the tibia, where there is much new bone, removal of this by gouging away the surface after reflecting the periosteum. In the case of a girl, æt. 17 years, who came under my care with a foul sinus, much ulceration of the surrounding skin and a stiff ankle, all from the effect of inefficient drainage, I carried out such a measure. The bone was removed till the upper part of the cavity was a shallow depression, the periosteal flaps brought over, laid in the cavity, and the lower part drained by a tube. The result is a good and useful limb. Such a method is impossible in the femur and in the upper end of the tibia. In opening a solitary abscess in the lower end of the tibia I have also tried the plan of inserting an osteo-periosteal flap, but the recovery was by no means so satisfactory as that following the use of the metal drain.

Finally, there is the ideal method of obtaining an aseptic cavity and filling it with some absorbable material, to adopt, in fact, the method employed by the dental surgeon, with this difference, that the stopping must be such as can be absorbed. The conditions are not the same, for the bone must be remodelled as the cavity is reduced, and therefore any material introduced would be absorbed.

I cannot offer any experience of these methods, and from what I can gather they have not been very successful in the hands of others. Moestig's results seem to have been most satisfactory, and he gives success in a large number; the cavity after being curetted, etc., is finally dried by hot air and filled with a special material—spermaceti and oil of sesami containing iodoform. Such methods are very applicable to unopened abscesses, with hard walls, for it would seem impossible to produce asepsis in the old septic cases with sinuses, to which I have called attention.

By most of us at present some method of drainage will be adopted, and I hope I have been able to demonstrate the value and applicability of the metal drain to your satisfaction.

In conclusion, let me say that the employment of metal drains in these cases is not original with me. Perhaps Cassaignac, who introduced the first drain for soft parts—the wire coil—employed it in bone cases also. My first knowledge was gleaned somewhere, I cannot recall where, in the course of my reading. I know it was mentioned in a paper or address by Sir Thomas Smith, and some time before this surgeon's illness I mentioned the matter to him, and I gathered that he had employed the metal drain with frequency and with success. The merit for the suggestion must, therefore, rest with this distinguished surgeon, who was of all men least disposed to publish his experience.

Sir WATSON CHEYNE, F.R.S., spoke highly of the value of Mr. Symonds' address, and of the lessons which it taught. He had very little criticism to offer, because criticism implied dissent. There could be no doubt that the healing of cavities in bone, whether resulting from chronic inflammation or otherwise, was a matter of very great difficulty; it was one of the most perplexing

things with which a surgeon had to deal. He (Sir Watson Cheyne) was in the habit of stating to his students—he was sorry to put it in the same way to such a learned assembly as the Medical Society—that there were two ways in which a bone cavity could heal: one by the organisation of blood-clot, and the other by the growth of granulation-tissue. The first of those methods scarcely came into play in those cases of which Mr. Symonds had spoken, because it demanded absolute asepsis, not asepsis in the ordinary sense, but a wound absolutely bacteria-free. In cases where there was a cavity, such as after scooping out myeloid sarcoma, healing by blood-clot was satisfactory—either by leaving the blood-clot alone, or by helping to entangle the clot with decalcified sponge, catgut, etc. But in connection with the cases under discussion the trouble was that there was a septic cavity; one might try to render the cavity aseptic, and then resort to the measures to which he had referred in aseptic cases. But his own experience was that it was very difficult, and often impossible, to render those wounds aseptic. However thoroughly one scooped out the cavity and chiselled the bone away, or cauterised with carbolic acid or any other strong antiseptic, bacteria seemed to be lurking in some cavities in the bone. Anything which might be put in so as to encourage the blood-clot would be a foreign body, and it would have to come out. He had tried the various things which had been recommended for the purpose, including plaster-of-Paris, but they had had to come out again. The only things which had succeeded in the matter to any extent were those which Mr. Charters Symonds had mentioned—iodoform and spermaceti paste and bismuth paste. In one or two cases he had used them, and had been able to watch by means of skiagrams the gradual disappearance of the iodoform. The same was true with bismuth also, though that applied more to tuberculous cases. It was remarkable how well bismuth helped the closure of some of those cavities. But that line of treatment was seldom applicable to the treatment of the cases to which Mr. Symonds had alluded. One was left either with drainage, on which Mr. Symonds had laid stress, or what he (Sir Watson Cheyne) thought was the first thing to be tried, namely, to cut away as much of the cavity as possible, level it down so that the soft parts lay over the bottom of the cavity. The difficulty in healing was two-fold. In the first place, if one left the cavity with a drain in it there was the chance of imperfect drainage and accumulation of septic material setting up further trouble. The second difficulty was that the bone which formed the wall of the cavity was so dense that it took a long time to undergo granulation. He always tried as far as possible to chip away the whole edge of the cavity and the bottom of the cavity and to leave a surface of clean and raw bone, to which the tissues could come down. Then one could put in a drainage-tube and it would heal very quickly. Still, there were cases in which if one tried to do that one fractured the bone or left bone which would not be strong enough, and drainage had to be resorted to after all. He was very much impressed with Mr. Charters Symonds' methods as described that evening. The troubles associated with other methods were very great. Packing with gauze was an abomination; he did not believe in it, and regarded it as bad surgery. India-rubber drainage-tubes were not satisfactory, as they came out; he had never been able to keep an india-rubber drainage-tube in as long as he wanted to. If one cut holes inside such a tube the granulations grew into those holes and prevented escape of discharge, whereas if holes were not cut the whole tube would slide out. He was therefore glad to hear that evening of the results of Mr. Symonds' practice in such cases, and he would certainly try them himself in the future when he was not able to render the cavity aseptic.

Mr. A. E. BARKER said that when invited to take part in the discussion he did not know what was likely to be the scope of Mr. Symonds' communication, and therefore he had not been able to look up cases of his own on which to found his remarks that evening. Mr. Symonds had shown a method which was certainly of very great value in the class of case which had been described, in which there could be no hope of producing asepsis. The division of the subject which had been made was one of very great importance in regard to treatment, namely, into tubercular foci in bones, and into secondary foci of septic character from bygone inflammation. That classification would, he thought, affect practice on the subject considerably. He (Mr. Barker) thought it should be possible, and, indeed, was possible, in regard to tubercular foci in the ends of bones, supposing the focus had not yet reached the surface, to open the soft parts and take out the tubercular matter and leave a cavity capable of healing up if the soft parts were closed over it without any drain. It would be filled with blood-clot, but it would not be septic in the usual meaning of the term, and the blood-clot would organise under the soft parts, and that method of treatment had given him results which in many cases had been encouraging. He thought that in the tubercular cases the same result might be obtained if the cavity were filled with bismuth paste and the soft parts closed over it, also without drainage. In that case there was a germicide acting mildly, and it was a material which he believed was capable of assisting in the organisation from the surroundings of the bony cavity. But there was a difficulty in those cases of chronic abscess which had followed a long while after an acute attack. Such cases could scarcely be regarded as aseptic. But in old chronic abscesses

he thought it would be possible to divide the soft parts and clear out the cavity and close the parts again, also without drainage. With regard to the use of the metal drain he could not speak of that except in reference to a few cases. There seemed to be a possibility that by the use of the paste made with bismuth the necessity of drainage at all in some cases might be obviated. Except in freely suppurating cases he would be encouraged to persevere on those lines. It was a material which was, while warm, able to penetrate into the recesses, and while acting as a germicide to form a sort of framework for the growth of the granulations until the whole condition had organised. In one of the first cases in which he tried it he had used it in some desperation; the patient had been operated upon by one of his colleagues two years before for a bad empyema. He drained in the usual way. The empyema remained, and then the patient had attacks of fever, and it had to be opened and drained again and again. At the end of three years nearly, he thought he would try the method of injection with bismuth paste. There was the deep cavity and track leading into the thorax and probably into the lung. He (Mr. Barker) injected that cavity with the bismuth paste, as introduced to the profession by Dr. Beck, of Chicago. There was a hard form and a soft form of the material. He injected the softer one, and in a very short time, that is to say, within two or three weeks, the large cavity, of which a skiagram was shown, had closed for the time. It broke again and discharged a little and finally closed. He had seen the patient recently quite well. The case seemed to be an encouraging one, especially as it was obviously septic, as the patient had a considerable rise of temperature on many occasions during the previous treatment. If such a result were possible in the case of ramifying tracts left from extensive empyema, it should encourage surgeons to attack similar cavities in bone. He had no doubt that the cases which had been published were familiar to surgeons where bone tracts had been injected with bismuth for the purpose of estimating their extent by radiography, and to the amazement of those who saw the skiagrams the whole condition had healed up very rapidly. Many such series of cases had been published by various observers, and those would encourage him, because he thought it would be admitted that even with the drainage-tube the process of healing was a very long one. But if drainage must be carried out, he did not think there could be any question that the tubes recommended by Mr. Symonds would enable the surgeon to achieve his object in the last way.

Sir A. PEARCE GOULD said he thought the meeting must feel very much indebted to Mr. Charters Symonds for showing the effect of the treatment of troublesome cases of chronic deep suppuration in bone by means of metal tubes. One or two of the cases which Mr. Symonds had shown and described were illustrations of a rather wide infection of a bone, and the metal tube which was introduced through a trephine hole did not, at any rate at first, effect the escape of the infective material from the whole of the diseased area of bone; further, small abscesses formed in some cases and were opened, and a second drainage-tube put in. That seemed to him to give an indication of one of the main difficulties of the treatment of these cases: the disease is not a single focus or area, but is often rather widely diffused and consisting of several more or less separated foci. He supposed that all kept before themselves as the primary object of the treatment of these cases the eradication of the infective material and the infected area of bone. Whether it be tuberculous material or a focus due to pneumococci, or the influenza, or pyogenic micro-organisms, if one could succeed in the primary operation in eradicating the whole of the infection, the subsequent management of the case was bereft of a great part of its difficulty. But that first step was a very difficult one, not so much so in the tubercular as in the other cases. The foci were often numerous and separated by bridges of firm bone. The true treatment, he thought, consisted in a very efficient and oftentimes laborious attempt to eradicate the whole of the infected area, and if possible to flatten down the walls of the cavity, as Sir Watson Cheyne had emphasised, so that it might granulate up from the bottom and soundly heal; but that where that could not be done, and a dense, sclerosed-walled cavity was left behind, long drainage was evidently required, and the metal tubes shown by Mr. Symonds would find a very useful field indeed. But he could not help thinking that if the impression were to get abroad that it was a satisfactory way of dealing with a chronic deep infection of a long bone to trephine and introduce a tube and let it be worn for an indefinite length of time it would be rather misleading, though he would not like to say that that was the impression which Mr. Symonds had intended to convey.

Dr. PALMER seconded the resolution with great pleasure, and complimented the President upon the great success of the cases which he had shown.

The resolution having been carried by acclamation,

The PRESIDENT acknowledged the thanks, and said he was still working with bismuth paste and hoped to get some still better results. In answer to Sir A. Pearce Gould's criticism, most of the cases were treated with a gouge and not with a trephine. The difficulty he had had in dealing with other foci was that there was no evidence that there was anything but a solitary focus, but further foci became manifest later, and it was not as if cases of early infection were being healed.

November 14th, 1910.

TO ILLUSTRATE THE COMMUNICATION BY MR. CHARTERS J. SYMONDS ON THE TREATMENT OF CENTRAL OSTEITIS BY METAL DRAINS.

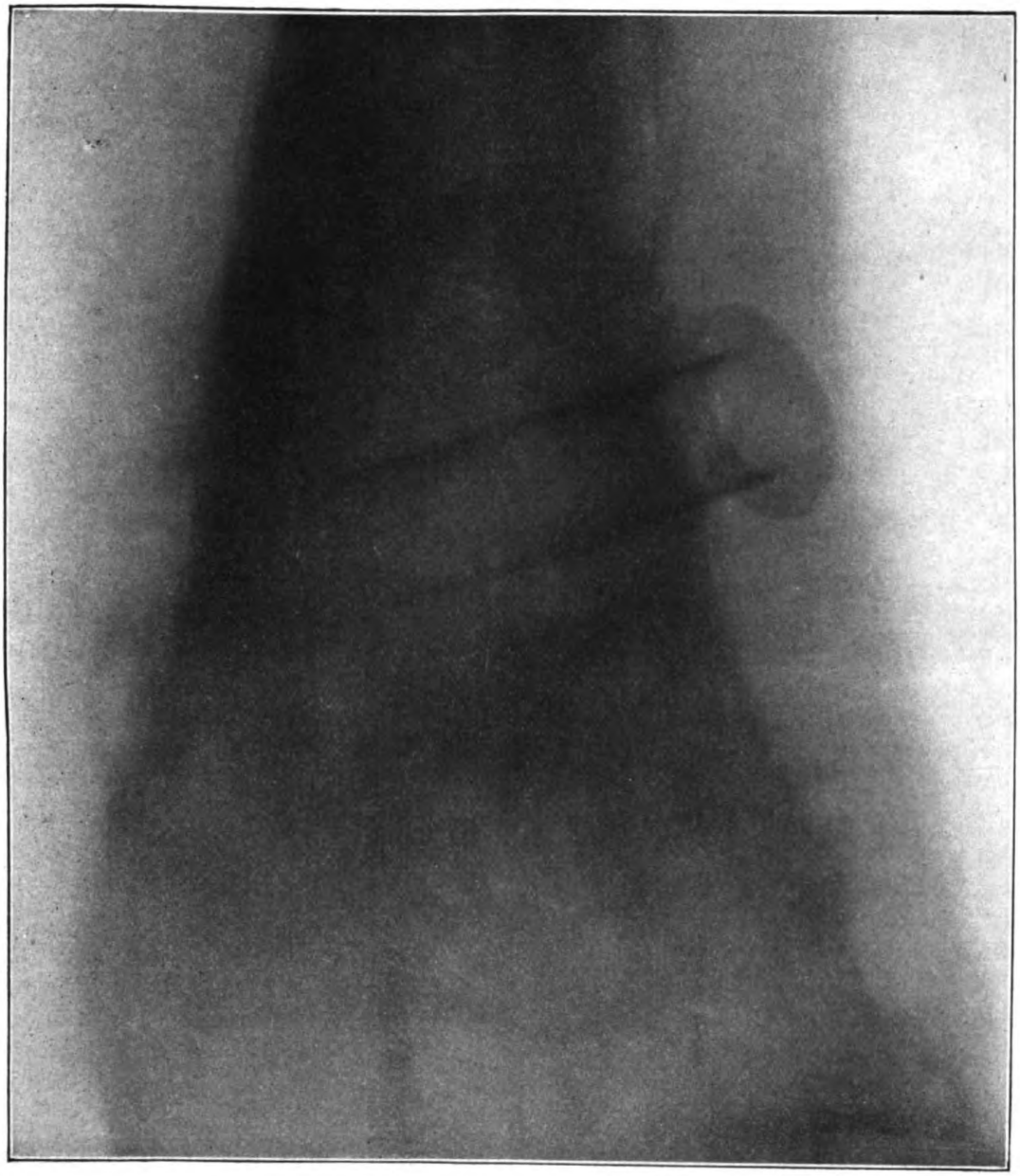

FIG. 2.—Case 5, p. 90. Showing the tube in the lower end of the femur.

TO ILLUSTRATE THE COMMUNICATION BY MR. CHARTERS J. SYMONDS ON THE TREATMENT OF CENTRAL OSTEITIS BY METAL DRAINS.

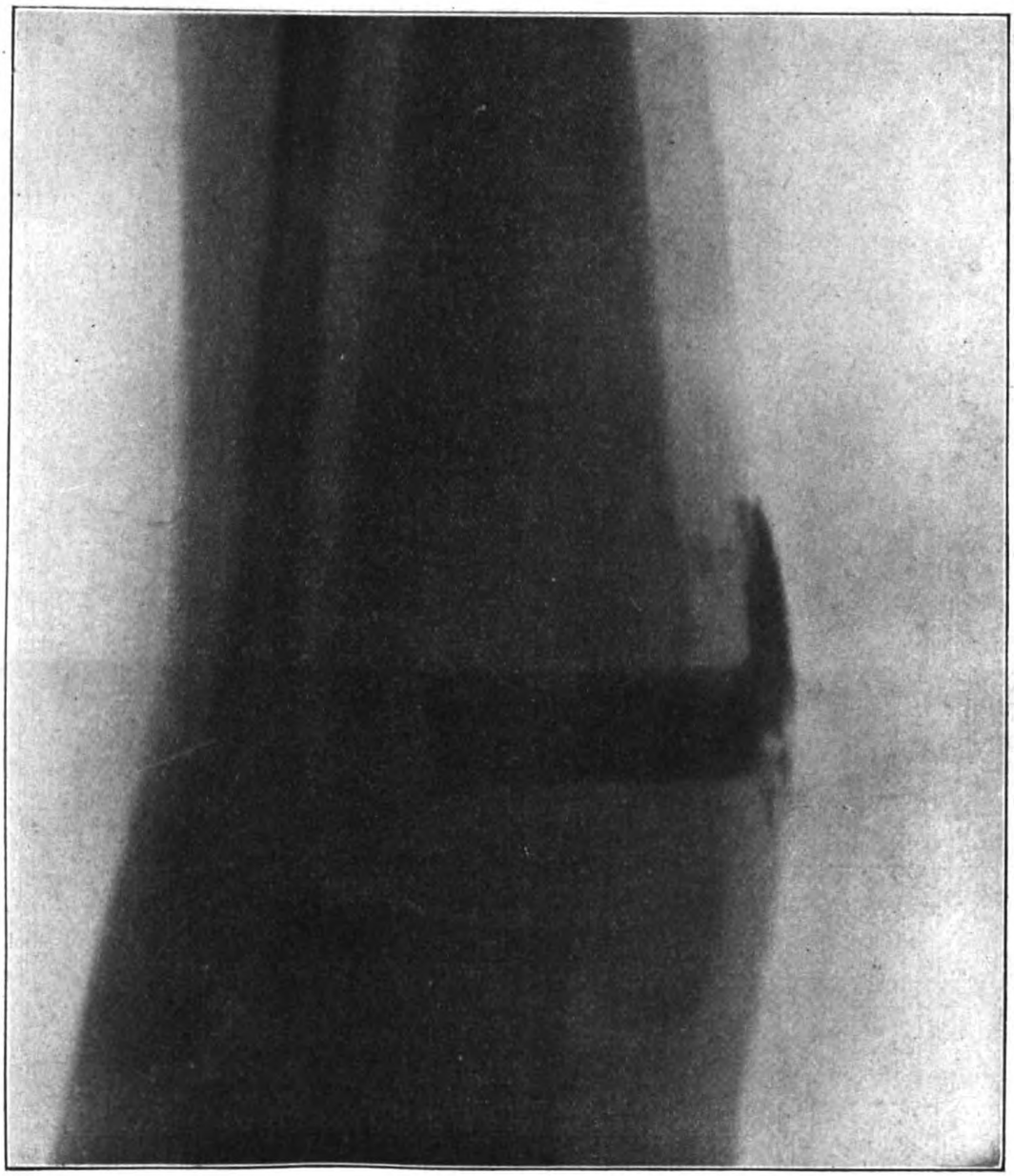

FIG. 1.—Case 1, p. 87. The channel for the tube has been filled up with bismuth.

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TWO CLINICAL LECTURES

ON

TYPHOID FEVER.

Delivered at Guy's Hospital.

By W. HALE WHITE,

Senior Physician and Lecturer on Medicine to the Hospital.

LECTURE I.

THE TREATMENT OF TYPHOID FEVER.

GENTLEMEN,—You will remember that when I was in the general wards—Mary and Addison—we had some cases of typhoid fever, and there are several in John and Miriam at the present time, and so it seemed to me that this afternoon it might not be profitless if we talked over the treatment of this disease. I think any of you, especially if you went to see the cases now in, would think it did not matter very much about treatment. They lie quietly in bed, and apparently we are doing nothing for them. But if you think that seriously I advise you to let the next half dozen cases of typhoid fever you see treat themselves, and I will undertake to say you will have a considerably better result in those which you treat. There is no disease in medicine in which the proper treatment is better for the patient than typhoid fever. Anyone who has had any experience is quite sure of that. Supposing you are certain that your patient has typhoid fever, the first thing you must do, undoubtedly, is to put him to bed. Like so many other abdominal conditions, typhoid fever does very badly if the patient does not go to bed. Sir William Jenner, who was a very acute observer, always said that wherever a typhoid fever patient was taken ill, there he must remain; that is to say, if he was taken ill in a friend's house he must not go home. I well remember a man who was taken ill in a doctor's house about thirty miles from

London. He had gone down there for a week-end, and while he was there it became clear that he had typhoid fever. When it was discovered he said that he must be moved, because the lady of the house was about to be confined, and he knew he would be in the way. His friend said he should not be moved. But the patient overcame the friend, and the friend consented to his being moved. He was moved up in an ambulance carefully, and was put in Bright Ward. Three days after he was brought into Bright Ward he died. We could find no particular reason, except that apparently the journey had so exhausted him. You will say the next case I am going to quote to you as illustrating that you should not move the patients is excessive; it is. But it only shows what people can do when they try. It was that of a man who had typhoid at Folkestone, and, unfortunately, somebody told him there was no place like Guy's Hospital, and so that poor creature walked from Folkestone to London with typhoid, and came here, and was admitted under my care, but died a few hours after admission. Please remember the immense importance of putting the patients to bed. The reason why bed is so important is not quite clear. Of course, the patient's temperature is elevated, and to maintain a raised temperature requires a certain number of heat-units; a certain amount of energy must be expended. Supposing a man weighs, we may say, 66 kilos, and his specific heat is ·8; he will need only 55 calories to raise his temperature 1° C. So that even if all the rise of temperature is due to increased production—some may be due to diminished loss—the amount of energy which is required to raise a man's temperature does not seem sufficient for that alone to exhaust him so much that he must go to bed to rest. I rather think that it probably is because some of the products of the fever act as direct cardiac poisons; in fact, the weak pulse shows that they do, and hence he cannot be up and about and doing things. But whatever the explanation may be, remember that there is probably no disease in which it is more necessary to put the patient to bed than enteric fever.

Leaving that, we now come to the very important question of food, and in that matter I want you to follow me carefully, because it is of great moment. The first thing you have to bear in mind in connection with a man suffering from typhoid fever is that he must have plenty of fluid. The reasons for that are clear. In the first place, the blood-pressure is low, and the patient requires to have plenty of blood in his vessels to keep up the blood-pressure. In the next place, the patient's mouth is dry and he is thirsty, and so fluid is pleasant for him to drink; he feels the want of fluid very much. Another reason for giving fluid food is that it is easy to swallow, and because the mouth is dry the patient frequently cannot masticate his food efficiently, and something is required to be taken which will go down easily. So, both on the general grounds of maintaining plenty of fluid in the body, and also on the local grounds of the dry condition of the mouth, you see it is very desirable that there should be a large amount of fluid in any diet for a prolonged fever such as typhoid fever.

The next thing about the diet is that it must be easy to digest. That is clear, because the juices, saliva and gastric juice, are not secreted in fever as freely as they are in health. Digestion is somewhat difficult, and nobody in his senses would dream of giving a fever patient a diet which it was difficult to digest. Many fluid diets are, fortunately, easy to digest, milk for instance, and therefore that is a suitable food for typhoid fever. And not only must the food be easy of digestion, but it must be nutritious, and it is to that that I wish to draw your attention now, for many people treat typhoid fever, I think, wrongly, because they half starve the patient. Let us go into that a little more fully. You will remember that an average man who is up and doing his work requires, roughly—depending, of course, on how much work he is doing—at least 2500 large calories a day of food. That seems to be fairly generally allowed. I admit it is impossible to estimate exactly how many calories of food a patient suffering from typhoid needs, because on the one hand he is not up and about while he is suffering from his disease, and therefore requires less on that ground; on the other hand, a large proportion of his pyrexia is due to increased production of heat, so that he requires some energy for that purpose. And then, again, some of his energy is utilised in making him breathe fast and making his heart beat faster than the ordinary person's. So while, on the one hand, you may argue that the patient requires less energy when he has typhoid fever, on the other hand, it is easy to point to reasons for which he requires more energy. There-

fore probably we shall be about right if we say a man with typhoid fever requires at least 2000 large calories of energy a day. Many cases of typhoid fever are fed exclusively upon milk. Now very few patients can possibly take more than three pints of milk a day. In the first place it gets nauseating after a time, and in the next place it is difficult to digest more than three pints a day. So we may take three pints as the maximum of milk which a fever patient can drink, or will drink. But that is only 975 calories. I assure you that you may see many patients who have typhoid fever who are getting only three pints of milk a day, but such a patient is being starved, and he is not receiving half the quantity of energy he should. And the marvel is that some of the typhoid fever patients do as well as they do. But worse than this, formerly patients with typhoid fever were sometimes restricted to whey, and the amount of energy in three pints of whey is microscopical. And yet patients dragged on for some time on this diet until they died. It is perfectly clear that if you put your patient on milk, inasmuch as he cannot with comfort take more than three pints a day, you must give him something else. And for years I have been in the habit of pointing out to you in the wards that the practice in many hospitals—I believe it was first done here—is that the best way, if you are to keep the patient on a milk diet, is to add carbohydrate to it to get more energy into it. And the form which this carbohydrate should take is preferably sugar, because in that case the body has not the exertion of converting the starch into sugar. I am in the habit, as many of you who have worked with me know, of adding lactose. It is natural to do that because lactose is the natural sugar of the milk, and very often you see typhoid fever patients who are under my treatment having much lactose in their milk. Some of them dislike it. I confess I do not mind it at all, and I have tasted it. If they do dislike it they will do almost as well by having maltine stirred in the milk, or honey, both of them being very nutritious. In passing I may say if you go into my wards at present I believe you will find that some of the patients are not having any addition to their milk. But they are all children, and in them three pints of milk alone goes a long way. They are mild cases, and as they are doing well it did not seem to be necessary to alter their diet. But it is clear that a man who is going through a long course of fever requires more than three pints of plain milk to sustain him: such a food would be starvation diet.

Let us go back to the sugar question for a minute. Take maltine. A drachm of maltine is worth 60 calories, so if you give a patient 4 to 6 drm. of maltine you get 240 to 360 calories. And these patients can often take 6 drm. of maltine in the course of twenty-four hours without the slightest difficulty, so you will be able to add 360 calories. In that way you will get a little nearer your 2000. Honey has much the same effect. A drachm of honey is worth 70 calories, as against 60 calories for maltine. Take now lactose. The caloric value of a grm. of sugar is 4. Supposing you give a patient two ounces of lactose, one ounce is 30 grm., so two ounces equal 60 grm. Thus there will be 240 calories. If you stir two ounces of lactose in each of his three pints of milk you will be giving him 720, and that added to the 975 already given in the milk equals 1695. So by stirring up these sugars into the milk you will get very near to your 2000 calories. And the simplest way, if you have to keep the patient on milk, is to stir in two ounces of lactose, equalling 240 large calories into each pint of milk, so that the three pints would give you 720 calories from lactose in the day, thus bringing the milk up to almost 1700 large calories, and you wanted 2000. So in that way your patient is not badly off, and if you add a little cream to the milk you will get nearly the 2000, because you know that the caloric yield of fats is very high. You might ask why do I not give more fat as its caloric value is so high? But you cannot get a fever patient to take much fat; these patients cannot digest it. If your temperature were up I ask you what you would think if you were given fat, or anything of that sort? It would be positively nauseating. It is because it is so necessary to add carbohydrates that it is customary to tell the patients with typhoid fever that they may have plenty of chocolate. I think it is a little more scientific to put a definite amount of a substance whose exact caloric value you know into the milk, rather than to allow the patient to nibble chocolate at random during the day, but the idea of giving the chocolate is the same. Another great advantage of giving plenty of carbohydrate is that it is a well-established fact in pathology that if you

feed fevered animals on large amounts of carbohydrate, the fats, and still more the protein tissues of the body, are considerably saved. If you take animals in whom pyrexia has been induced by some inoculation, and contrast those that are taking carbohydrate with those which are not, the loss of weight on those which take carbohydrates is very much less. And experiments show that the protein tissues are very considerably saved. It seems as if, in a fevered person, if you give carbohydrate, the temperature is kept up by the utilisation of ingested carbohydrate, the protein and fat tissues of the body being spared. So that you will see on all hands that is the best thing to do. You might ask, Why not give them a good deal more protein and so save their proteid tissues? The energy value of proteids is small and they are difficult to digest, and people do not seem able to digest them very well when they have fever. And, further, the body is already so flooded with products of proteid metabolism that it hardly seems wise to give more proteid. But the practical point is that much proteid cannot be easily digested.

We will now pass on, as we do almost insensibly, to the question whether you should give typhoid fever patients alcohol. You know that in some people alcohol slows the pulse and strengthens the heart-beat and renders the digestion more easy, and at the same time makes the people drowsy. All those are good things in typhoid fever. But the great objection to alcohol in medicine is that you cannot foretell whether it is going to suit the patient, or whether it is not. Some people get headache from it, others are kept awake by it rather than being made to sleep. In others it makes the pulse faster instead of slower, but when it succeeds I quite admit the importance of giving alcohol to some fever patients is very great. But the next patient to whom you give it you may get the harmful effects, and it is the uncertainty of it that has made alcohol largely go out of fashion of late years as a drug. But it has this very great advantage, that it is a food; it is an ideal food, because it requires absolutely no preparation, can be absorbed as such, and used directly in the body, and if some chemist could take away from alcohol the deleterious properties of it and leave us only the food, it would be the best food that could be conceived. An ounce of pure alcohol is worth two hundred calories. Most spirits, brandy for example, are about half water, so if a patient is taking six ounces of brandy he will be taking six hundred additional calories in the day. So if the pulse is slowed and the heart strengthened and the digestion improved, and he sleeps as the result of alcohol—and all those symptoms are good—it is a good food to give him, because it is easily absorbed and requires no digestion, and is to some extent a fat sparer. But if it excites him and gives him a headache, does not make him sleep, makes his pulse rapid, then leave it alone. The best spirit clinically to give, as it is generally allowed, is brandy rather than any other spirit. In none of the cases of typhoid fever at present in hospital have we thought it worth while to give any alcohol at all, because they are mild cases and they have got on very well without it. One point in connection with diet which I have not mentioned is the amount of fluid you should give. The three pints of milk which you give is not nearly enough in the way of fluid, and you should let the patient drink barley-water if he is very hot and feverish. You ought not to give the milk oftener than every two hours, because if you do the stomach never has a rest. All this is very well, but I have had typhoid fever myself, so I can speak emphatically, and I say I have found this milk diet horribly monotonous, and towards the end of the week one loathes the sight of it. And we have learnt of late years how much proper digestion depends on whether we have a good appetite and relish our food. For a long while an attempt was made to get over this terrible monotony of the milk by flavouring it, and the best way to do that is to add some black coffee. That was my preference when I had the disease. Some people prefer vanilla. I have tried them both when I had typhoid fever. But you get tired of those even after a time. And that is why people have of late years thought of other things. But let us finish speaking about the milk. One objection to the milk is not only its monotony, but it often curdles in the stomach, and if it does curdle in the stomach it frequently gives the patient diarrhœa. Whenever a typhoid fever patient has diarrhœa you must look at the stools, and then you will probably find that they are full of curds which are the cause of the diarrhœa. That diarrhœa can be stopped in such a case by citrating the milk, two grains of citrate of soda to each ounce of milk, or by peptonising it. Citrating it is

the better course, for unless the peptonising of the milk is well done it has a peculiarly disagreeable taste. Another advantage of milk is that in large institutions you must have regular rules. There are many fever hospitals which have numerous typhoid fever patients. I have made a post-mortem examination myself in a case in which I have found free watercress in the peritoneal cavity in a typhoid fever case, and that makes you determine that ever after you will be very careful with the diet. What happened was that the patient's friends surreptitiously introduced watercress on visiting day. I do not know whether it led to perforation, but it passed through the intestine into the peritoneal cavity. And several times since I have been in the hospital I have known nurses turn out raw apples from patients' lockers after visitors have gone. And in a large hospital it is often desirable as a protection from their friends to keep the patients on a very strict and rigid diet. Some of the patients who take so much milk complain of the lack of salt, and that lack can be got over by adding salt to the milk. That is thought by some of them to be unpalatable, and it can be better done by giving beef-tea or chicken broth and adding salt to that. In private or hospital practice, if you can depend on the friends not giving anything without your consent you can make the diet very much more pleasant than this monotonous milk diet. I do not object to typhoid fever patients having bread and milk, custard, eggs beaten up in milk, a little beef-tea, or chicken broth, provided they have no diarrhœa. Or the patient may have milk puddings or fish which has been passed through a sieve. You can vary the food much better in private than in hospital. Some of our adult patients who are in the wards at the present time have had a most varied diet and they have done very well. One thing which women like most when they have typhoid fever is a cup of tea in the afternoon, and there is no harm, as far as I know, in that. I have brought to read to you samples of diets that have been given when watch could be kept over the patients, to show that they were taking just the diet and nothing else. For instance, one of the first diets mentioned, in which there was something more than milk, was that which was suggested by a Russian physician, who allowed meat and bread and eggs. I have no objection to any of those. The mortality of his cases was not increased. Here was the diet for an average case: At 7 a.m. he allowed tea with a roll; at 8 a.m. oatmeal or barley-water, or liquid porridge with butter; one or two boiled eggs. Then milk with roll. At 12 to 12.30, chicken soup; 3 o'clock, tea with roll; 6 p.m., chicken or beef-tea with semolina pudding; 8 p.m., small piece of chicken, milk with roll, and during the night coffee or tea, with a litte brandy, and in addition the patient had some egg and brandy every two hours. That is an excessive diet, but there is nothing there that a typhoid patient cannot take. It is a simple food, and it will all be fluid by the time it reaches the ulcers, and so there is no risk of perforation. There is nothing like a hard piece of apple or watercress in that to lead to perforation. Here is another, one which is used at the Massachusetts General Hospital. Dr. Shattock allows milk and cream, milk with water and egg, buttermilk, koumis, any preparation of milk, soup, Horlick's food, Mellin's food, malted milk. Notice in all these carbohydrates the starch has been converted into sugar, and that is desirable in all the diet of typhoid fever, so as to save the body the trouble of doing it. He gives gruel, and ice-cream, and raw eggs, and sometimes very finely minced or scraped meat. A large number of cases were collected, and on the liberal diet a mortality of 9·47 per cent., and on the milk diet 10·5 per cent. obtained—that is to say, 1 per cent. more on the old-fashioned milk plan. I do not think that is surprising, because we have seen the old-fashioned milk plan frequently meant only 970 calories of food a day when 2000 were required. Of course, no typhoid fever patient will get well unless the ulcers in the intestine heal, and the ulcers are less likely to heal if the patient is being starved. And probably many of the ulcers do not heal because the patient is starved, and that suggestion is borne out by the fact that on a liberal diet there were only 4·77 per cent. cases of hæmorrhage, on a milk diet 8·8 per cent. It is the ulcers which are not healing which lead to the bleeding, and so we should expect hæmorrhage to be more common on milk diet; and again, perforations were 1·37 per cent. of the cases with liberal diet, 2·4 on the milk diet, presumably because on the latter the ulcers did not heal so well. So when the larger amounts of food were given the ulcers were apparently doing better, which I should have expected.

Now we will pass on to consider the question of temperature. As a rule in fever you do not need to treat temperature. The pyrexia is a protective mechanism, an attempt on the part of the body to cure itself. But experience has shown that the typhoid fever patients bear a high temperature badly. They easily become delirious, their pulse easily becomes rapid, and they certainly do not do well when they are delirious and they have a rapid pulse. Thus typhoid fever differs from other fevers in that experience has shown that it is well to control the temperature. That the temperature is really a good thing, speaking generally, in fevers is shown by many collections of cases, of pneumonia for example. If you take a large collection of cases those with a temperature of 102° to 104° F. are found to do better than those with a lower temperature, because the body is defending itself better with the higher temperature. But typhoid fever is a disease in which the patients do best with only a moderate temperature. Hence we try to keep the temperature of typhoid fever patients somewhere a little below 103° F., and that is best done by sponging the patient when the temperature mounts higher. If you sponge the patient systematically with cold water in an average case of typhoid fever it is easy to keep the temperature at about 103° F. You must not have a fixed point unless you are dealing with a great epidemic of typhoid fever, because if you see that a patient gets delirious, and has a rapid pulse with a temperature of 102·5° F., you should order him to be sponged whenever his temperature is 102° F. But if he seems fairly well and does not have a rapid pulse or is not delirious at 103° F., you need not order sponging until his temperature is 103·5° F. So you must use your judgment in selecting the point at which to order the patient to be sponged, but on the average it is a good thing to keep temperature as far as you can about 103° F. You will ask me if I have any proof of this. Enormous collections of statistics have been made, tens of thousands of cases, because typhoid is such a common disease; there are books devoted solely to this one aspect of typhoid fever, and most people who have collected large numbers of cases have found that the mortality is reduced 6 per cent. to 8 per cent. by bathing or keeping down the temperature by means of cold water, or in some way. But as I said just now, choose your case; do not necessarily bathe or sponge every case. And you have to remember that those figures are fallacious, because supposing that the cold bathing killed a certain number of people, but saved more, by merely stating that the benefit of the treatment was expressed by the fact that you had 6 per cent. better results when you used it, you would take no account of the patients who were killed by the cold bathing.

Now for a few practical hints about how to bathe. Sponging I need not describe to you, but frequently it happens that the temperature is difficult to control by sponging. In the summer I saw a little boy whom it was useless to sponge, for doing so made no impression on the temperature. I will tell you how to give a cold bath for typhoid fever. As a rule the temperature of the water should be about 90° F, and you put the patient into it naked. You may carry the patient to the bath in a sheet and drop him into the bath sheet and all. Then put ice into the water and so cool the bath, but never let its temperature fall below 70° F. While you are doing this you should from time to time take the rectal temperature, and when the rectal temperature is about 3° or 4° F. lower than the temperature at which you put the patient in, take him out, because the temperature will continue to fall for some time after you have taken him out. If you put him in at a high temperature such as 106° F., take him out when the temperature is 102° F., and always, whatever the original temperature, take him out before the rectal temperature falls below 100·5° F., because it will fall much afterwards. Then put the patient back to bed and wrap him in blankets. If the patient shivers on getting back to bed this can be stopped by putting in plenty of hot-water bottles. The temperature in any case of typhoid in which sponging or bathing are likely to be required should be taken every two hours. And if in any case the temperature has been in the habit of rising, it is better to sponge or bathe when it begins to go up rather than wait until it is a considerable height before bathing. The persistence with which you may have to stick to the bathing is remarkable. Sometimes the patient has required thirty or forty baths before getting well. You know I am frequently telling you that until you have had to deal with a severe case of typhoid fever you have no idea what hard work a single case may be. One other point is, you often go

into a private house where a bath is a great difficulty to give. The bath-room is some way off the sick-room, or there is no bath-room. You can then put two large blocks under the head of the bed so that the bed tilts, then pass a large mackintosh sheet under the patient, bank it up at the side by pillows, pour cold water in at the top and let it flow out at the bottom, and with the sponge splash water over the abdomen. Thus you can control the temperature as efficiently as if you put the patient into a bath. Never use anti-pyretic drugs; they are all-powerful cardiac depressants, and fond as I might be of any of you, I should not defend you if you were using antipyretic drugs in a case of typhoid fever.

The diarrhœa I have already mentioned. If it is not due to curds and is very intractable so that you think it is weakening the patient, the best way to check it is by lead and opium pill, or by a starch and opium enema. Sometimes the patients are constipated, and then you can give an enema of olive oil, and this on the whole is better than giving an aperient by the mouth. But in the early stages many people give calomel or castor oil. You will be on the look-out for bed-sores. Formalin, 1 in 500, with a little glycerine and myrrh makes a good mouth-wash. Do not undertake a case outside the hospital unless the patient has one or two nurses, because the nursing may be far the most important feature of the case, and when bathing is required frequently several nurses are required, sometimes as many as four, two night and two day.

Fortunately severe and fatal hæmorrhage is rare, but when hæmorrhage from the ulcers becomes serious you should stop all food by the mouth. Give morphia or plumbi cum opii pill, because lead acts as an astringent and opium keeps the bowels still. Prescribe plenty of opium to paralyse the bowel and prevent peristaltic movements, and so give the blood-clot which is stopping the vessel a chance to remain *in situ*. An empirical remedy for hæmorrhage is turpentine; I do not know how it acts; it may be given every few hours in capsules, and I do not know that it does any harm, and I do not know that it does good. Another serious trouble is perforation. Nowadays there is only one treatment for that, and that is immediate surgery.

Patients with typhoid fever are in the third week liable to sudden attacks of cardiac collapse, which may resemble perforation. If you are in doubt as to whether a patient has perforation or not, supposing, for example, one of the clinicals to-night was in doubt whether perforation had occurred and telephoned to me to come down, it would be a good thing if at once he did a blood-count, and another in an hour, and so by the time I arrived there would have been two blood-counts. We should then wait another hour and have another, and if the number of leucocytes were mounting there is no doubt that the perforation has occurred, and the abdomen should be opened at once, for every half hour lost is a disadvantage to the patient.

Another serious matter is cardiac weakness. If typhoid fever does not kill by perforation nor by hæmorrhage, the patient will, if he dies, probably die from cardiac weakness. That often comes on in the third week, as you would expect, because the patients are by then exhausted, and it has many times happened to me to be asked to see a patient in the hospital with supposed perforation, and one has been able to demonstrate that no perforation has occurred, but the patient's heart has suddenly become weak. The pulse becomes small and perhaps uncountable and the patient looks ill. In such a case I think the best thing you can do is to put the patient on a mixture of liquor strychninæ and tincture of digitalis and caffeine, ♏ x of each of the first two and 5 gr. of caffeine, and 2½ of sodium salicylate to render the caffeine soluble, and give this every three or four hours. You may give your strychnine subcutaneously. If the collapse is serious you should give injections of pituitary extract, which picks the pulse up and raises the blood-pressure in the most wonderful way.

In the other points regarding treatment there is nothing to specially allude to. Sometimes these patients cannot sleep. Bromides will often make them sleep; 30 gr. of choralamide in brandy, or morphia injections will do so. A very troublesome symptom sometimes is preliminary headache. I know that this headache may be really awful; my own headache was so bad that I did not think I could stand it. What relieved me most was 20 gr. of bromide three or four times a day. Delirium as a rule in typhoid ought not to trouble you much, because generally it is associated with a raised temperature, and if you sponge or bathe the case the delirium will pass away. But if it does not, and if the delirium is really troublesome, I believe the best drug you can use is paraldehyde, 2 drm., flavoured with orange and with a good deal of water, because you remember it does not mix well with water.

I hope I have shown you that typhoid fever is a disease for which you can do much by treatment.

November 21st, 1910.

THE CLINICAL ASPECT OF SECONDARY (METASTATIC) CANCER OF THE OVARY.*

By J. BLAND-SUTTON, F.R.C.S.Eng.

THE subject of cancer of the ovary has interested me during a quarter of a century; during the whole of that period I entertained the gravest doubt in regard to the existence of primary cancer of the ovary. I am still in doubt concerning it. Observations published during the last eight years have established the fact that solid tumours of the ovary large enough to be perceptible to eye and hand in the hypogastrium, which on microscopic examination exhibit the structure of carcinoma, are nearly always secondary to primary foci of cancer in the breast, gall-bladder, or some part of the gastro-intestinal tract, especially the pylorus and colon. The observations of Schlagenhaufer, Cuthbert Lockyer, Briggs and Walker, and my own have demonstrated that many large solid tumours of the ovary, unilateral and bilateral, which exhibit the structure peculiar to cancer, and regarded as arising primarily in the ovary, are, in truth, secondary to cancer in some other organ. The relationship of the ovarian masses to the primary cancer is demonstrated by the fact that the minute structure of the tumour in the ovary varies according to the situation of the primary growth.

Among the specimens exhibited one was as big as a child's head, and presented the structure of a mammary cancer. The patient had had both breasts removed for cancer some years previously. The ovarian mass was so smooth and hard that an experienced gynæcologist regarded the tumour as a common uterine fibroid.

Another specimen, as big as a bladder of lard, has the structure of an intestinal cancer. Before the operation I was suspicious that the tumour was metastatic, especially as the patient vomited persistently. After removing the tumours (they were bilateral), a careful search was made for a primary focus; a firm contracting growth was found obstructing the ileo-cæcal valve. A coil of ileum was anastomosed with the ascending colon, and with success. The patient survived the operation and lived comfortably for twelve months, and died from general dissemination of the cancer in the abdomen.

A third tumour, the size of a small cocoanut, exhibited the microscopic structure of the gastric glands. An examination of the patient led me to believe that she had some disorder at the pylorus. In the course of the operation the stomach was examined and a cancerous lump, too advanced to justify any surgical interference, was found. The patient only survived the operation eight days.

The practical outcome of such observations is very important, and amounts to this: When patients are submitted to operation for solid ovarian tumours, especially if the tumours are bilateral and accompanied by vomiting and ascites, a careful examination should be made of the abdominal viscera, especially the stomach and sigmoid flexure of the colon.

It is a significant fact that whenever surgeons have made inquiries into the remote results of ovariotomy, they have been astonished to find that many of the patients who apparently recovered from the operation have perished from a recurrence in the abdomen, or from intestinal obstruction. The fact that many solid tumours of the ovaries are metastatic offers an explanation of some of these unhappy sequences.

During the last two years I have been able to extend these observations. Some pathologists who willingly admit that the solid cancerous tumours, such as have just been considered, are metastatic, still believe that many cystic ovarian tumours with a cancerous structure are primary. On this subject I have some new observations to offer.

We formerly believed that the generalisation of cancer was due to infection of the blood-stream—that minute fragments of cancer were conveyed by it and lodged as emboli in distant organs. Handley has amply demonstrated the insufficiency of this theory, and in great measure supplanted it by his observations of a process known as permeation. For my purpose it comes to this: When cancer attacks the stomach, cæcum, colon or gall-bladder, the cancerous cells permeate the muscular and serous coats in relation with the primary growth and find their way into the general peritoneal cavity. Many of these cells are conveyed by the peritoneal fluid into the recesses of the pelvis and lodge on the ovaries, Fallopian tubes and uterus. Cells

* Delivered before the Medical Society of London.

which fall upon the ovaries implant themselves, grow, replace the ovarian tissue, and in some instances become large and easily recognisable tumours.

From my observations I have come to the conclusion that when the cancerous cells implant themselves upon a normal ovary, the result will be a solid tumour, but it occasionally happens that cancer arises in the gastro-intestinal tract of a woman who has a cystic ovary. In such circumstances the cancerous particles implant themselves on the wall of the cyst and grow quickly into large and formidable masses. In addition to the organs impeached by Schlagenhaufer I have to add the Fallopian tube.

Previous to 1888 primary cancer of the Fallopian tube was unknown; since that date so many carefully observed cases have been recorded that Doran has been able to collect the reports of one hundred cases. It is a very malignant form of cancer, and the operative treatment of this disease has rarely been followed by enduring success.

Since attention has been drawn to the existence of this disease we have realised that it is by no means uncommon, and in many instances it has occurred in association with uterine fibroids and ovarian cysts.

A patient was placed under my care with a large ovarian tumour which appeared to have undermined her health. I was suspicious that it might be malignant and made a careful examination of the stomach and the abdominal organs, but failed to find anything confirmatory, though I had a strong suspicion that it was an example of implantation cancer. In the course of the operation the lower pole of the tumour was found firmly adherent to the uterus and the pelvic tissues. The Fallopian tube was as thick as a thumb and stuffed with soft growth: its mouth was widely open.

After the operation I made a careful examination of the parts which had been removed. The ovarian cyst was of the ordinary multilocular type: the ampulla of the Fallopian tube contained soft growth, and a "stream" of this material had escaped through the ostium and infected those parts of the ovarian cyst adjacent to it. The tissue in the tube proved, on microscopic examination, to be cancerous, as well as the tissue in the wall of the ovarian tumour adjacent to the mouth of the tube. The remainder of the ovarian cyst exhibited the usual microscopic characters of an innocent multilocular cyst. The patient recovered from the operation, but died a year later with generalised malignant disease of the abdomen.

A more striking example came under my notice in 1909 A spinster lady, æt. 50 years, was placed under my care on account of an abdominal lump, which she had noticed many months. She sought advice because the tumour had of late grown more rapidly. I suspected the tumour to be malignant. At the operation the mass reached high above the umbilicus; its pelvic section adhered to the uterus, broad ligaments and rectum. When applying the ligature to the pedicle I noticed that the tube was unusually thick.

After completing the operation, an examination of the parts removed disclosed a large Fallopian tube with its ampulla stuffed with growth, continuous with a mass of similar tissue on the adjacent parts of the ovarian cyst. On microscopic examination the growth in the tube and on the wall of the cyst proved to be cancerous. The history of the case compared with the conditions found in the course of the operation made it clear that this patient had an ovarian cyst, and subsequently became the victim of cancer in the Fallopian tube. The cancerous material oozing from the cœlomic mouth of the tube infected the parts around, including the wall of an ovarian cyst which lay in contact with this cancerous tube. The patient was reported in good health twelve months after the operation.

It is a well-known fact that when the mucous membrane of a Fallopian tube is infected with pathogenic micro-organisms, the changes in the tubal tissues resulting from such infection lead to the complete occlusion of the cœlomic ostium of the tube.

Some years ago I was able to demonstrate that an occluded ostium occasionally occurs in cancerous Fallopian tubes, and that the closure of the mouth of the tube exercises an important influence on the course of the disease. When the ostium of the tube remains open, the cancerous particles are poured out cornucopial fashion into the pelvis, and soil all the adjacent tissues and organs.

Nearly all the patients who submit to operation for the removal of a cancerous tube, in whom the mouth of the tube remains open, die with recurrence of the disease within a year.

A woman under my own care had primary

cancer of the tube with the cœlomic ostium firmly occluded. I removed the uterus as it contained a large fibroid and both tubes. She was alive and in good health three years after the operation.

Glendining has recently published an interesting paper dealing with the spread of carcinoma by the Fallopian tube, founded on a case under my care.

A woman had two movable lumps in the lower abdomen and pelvis associated with symptoms of gastric cancer; no lump could be felt in association with the stomach. At the operation the stomach was found to be the seat of cancer, too extensive for removal. One ovary was converted into a tumour the size of a football, and the other into a solid lump as big as a bantam's egg. These were removed, and on microscopic examination revealed the characters of carcinoma.

To the naked eye the Fallopian tube attached to each cancerous ovary appeared normal, but, when examined microscopically cancer particles were found free in its lumen; the sub-epithelial and plical folds were extensively infiltrated with cancer.

From a careful consideration of the matter Glendining came to the not unreasonable conclusion that the infection of the Fallopian tube was brought about by cancer-cells swept into the Fallopian tube through its cœlomic ostium engrafting themselves on the mucous membrane, and subsequently penetrating to the deeper tissues.

This observation is interesting in another direction. Primary cancer of the Fallopian tube appears to affect both tubes in a larger proportion, of instances than is common in other bilateral organs liable to cancer. There may be an error of observation, for, without great care, a Fallopian tube stuffed with soft dendritic warts is sometimes erroneously considered to be cancerous. It is, however, conceivable that cancerous particles escaping from the open ostium of a cancerous tube into the peritoneal cavity may be carried into the open ostium of the companion tube and implant themselves on the tubal mucous membrane.

It is a noteworthy fact that bilateral cancer is more common in the Fallopian tube than in any paired organ of the body. This bears out Glendining's view that many cases of bilateral cancer of the tubes supposed to be primary are really secondary to a focus in some part of the gastro-duodenal tract.

It must not be imagined that the implantation of cancer on the walls of ovarian cysts is an uncommon event. Within a period of eighteen months (from March, 1909, to October, 1910) seven examples came under my observation. The primary foci were distributed among the following organs: stomach, 2; hepatic flexure of colon, 1; sigmoid flexure of colon, 1; uterus, 1; Fallopian tube, 1; and in one the primary focus was undetermined.

The undetermined case happened in a woman, æt. 73 years. She had an ovarian cyst containing ten pints of fluid, and as it seemed quite innocent and simple I did not inspect the gastro-intestinal tract. Some hours after the operation I examined the cyst, and was astonished to find a hard lump in its wall as big as my fist. This was inspected microscopically; it consisted of a mass of gland-spaces lined with columnar epithelium identical with those characteristic of cancer of the colon. This old lady has a primary focus of cancer somewhere in her colon and I missed it. Her subsequent history will be closely followed. I am sure many similar cases are missed because ovarian cysts are not examined microscopically as a matter of routine.

The question naturally arises as to the advisability of operating in many cases of this kind. The same argument applies to operations performed for the relief of cancer in any situation. When patients come under observation with a tumour the size of a football in the lower half of the abdomen which is interfering with the bladder, the rectum, or both these organs, producing pain and great distress, perhaps accompanied by vomiting, the surgeon is bound to give the patient relief. No surgeon hesitates to perform colotomy in patients with acute intestinal obstruction caused by cancer in the upper part of the rectum. The surgeon will rarely have any difficulty in satisfying his conscience concerning the necessity of an operation, but he will at times be sorely tried when determining how much or how little he should remove. On several occasions I have removed bilateral ovarian cancerous masses and the uterus, and in the same operation removed a cancerous obstruction in the sigmoid flexure and joined the ends by circular enterorrhaphy. In some instances the patients have survived the operation and lived in comfort two or more years. It is more common for death to ensue within a year of such an operation.

One of the most striking features in the after-history of patients from whom malignant ovaries

have been removed is the rapidity with which the cancer recurs in the pelvis, even in cases where the uterus and pelvic peritoneum seems free from infection at the time of the operation.

Lockyer made a valuable observation in regard to this. He examined microscopically ovarian tumours removed for bilateral carcinoma; the Fallopian tube and mesosalpinx belonging to each tumour appeared normal to eyes and fingers, but when examined microscopically were found to be extensively infected with cancer. I have on several occasions confirmed this observation.

The study of these facts teaches many things. When surgeons remove bilateral ovarian tumours which appear to be malignant, they should carefully examine the gastro-intestinal tract, the gall-bladder and the Fallopian tubes for evidence of primary cancer. If the disease is in a situation favourable for excision it should be removed; and if there is no obvious evidence of dissemination beyond the pelvic organs, it is a useful proceeding to remove the uterus as well as the ovaries. When the primary disease is too advanced for removal, the patient's condition is often made more bearable, if not comfortable, by the removal of the ovarian masses, but it must be borne in mind that in attempting to do this the lives of patients are often shortened.

Finally, a prolonged study of this question strengthens the conviction I formed many years ago, that primary carcinoma of the ovary remains unproven.

THE PATHOLOGY OF CERTAIN OVARIAN TUMOURS.*

By BERNARD H. SPILSBURY, M.D.,
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St. Mary's Hospital.

I PURPOSE in the first place to give an account, with comments, of a case of primary malignant growth of one ovary, with peritoneal metastases and with a secondary deposit in the transverse colon. This produced a stricture of the colon, which was responsible for the death of the patient.

A woman, æt. 63 years, was admitted into St. Mary's Hospital on November 25th, 1909. A history was given of loss of flesh and of constipation extending over a period of several months. The constipation had been more obstinate lately; there had been some distention of the abdomen for a fortnight; for a week before admission the obstruction had been absolute, and had been associated with constant vomiting for several days.

On admission the patient was extremely ill; the abdomen was distended and was resonant, and there was bulging in both flanks. No mass could be felt on abdominal examination.

An exploratory abdominal operation was performed at once. A mass was found in the pelvis, nodules of growth were scattered over the peritoneum, and the cæcum was greatly distended. This last was opened and a Paul's tube tied in, but the patient rapidly sank, and died within a few hours.

At the autopsy the right ovary was replaced by a spherical mass of growth, which was about the size of a cricket-ball and which occupied the pouch of Douglas. The growth had a slightly nodular surface, and was smooth except where firm adhesions stretched to the posterior surface of the uterus and to the peritoneum covering the upper part of the rectum. The right Fallopian tube was stretched over the mass. On incision no trace of ovarian tissue was visible; the tumour was white in colour and rather soft, and showed extensive central necrosis, with small cavities containing myxomatous material. The left ovary contained no growth, and the other genital organs were normal, but several small deposits of growth were present in the right broad ligament.

The peritoneum was studded with small white deposits of growth, which projected from its surface; these were more numerous on the peritoneum covering the coils of small intestine and their mesentery, but a few were present on the surface of the liver and spleen.

In the transverse colon was found a stricture; this was situated at a point about three inches from the hepatic flexure, and the lumen of the bowel at this spot barely admitted the tip of the little finger. No ulceration of the mucous membrane of the bowel at the site of the stricture could be distinguished.

On incision, the wall of the stricture in the whole of the circumference of the intestine was

* Delivered before the Medical Society of London.

considerably thickened, and was firm and of a white colour, and the peritoneum covering the bowel for some distance on each side of the stricture was thickened and had a similar appearance.

Beyond the stricture the large bowel was empty, and there was no hypertrophy of its muscular tissue, although the upper part of the rectum was somewhat stretched over and flattened by the growth in the pelvis.

There was considerable muscular hypertrophy of the wall of the large bowel behind the stricture and of the greater part of the small intestine, and there were several stercoral ulcers in the cæcum and in the lower end of the small intestine.

No deposits of growth were found in the substance of any of the other viscera.

On microscopical examination the ovarian tumour had the structure of a columnar-celled carcinoma. The tumour cells had generally a regular glandular arrangement, and the lumens of the glands were filled with myxomatous material; a little fibrous stroma separated the glands from one another. Remains of the original ovary were found in some places spread in a very thin layer over the surface of the tumour.

The peritoneal deposits of growth had a similar structure, but the fibrous stroma was more abundant, and these nodules were found to invade the structures upon which they were growing.

In the stricture in the colon similar growth was found; the peritoneal coat was much thickened and there was an abundance of fibrous stroma mingled with the carcinomatous tissue; the muscular coat was hypertrophied and contained large acini of growth which were occupied by mucin; the submucous coat was thickened in some parts and was infiltrated with similar growth, but the mucous membrane was for the most part free from growth and was quite normal, being separated from the submucous coat by the hypertrophied muscularis mucosæ.

In a few small areas, however, the growth had penetrated into the deeper part of the mucous membrane, but in no place had the mucous membrane been replaced entirely by growth.

This disposition of the growth in the transverse colon indicates, I believe, that the growth had its origin in the peritoneal coat of the bowel, resembling in that respect the other peritoneal deposits, and that its greater distribution and invasion of the bowel was due to frequent and prolonged contact with the primary growth in the ovary. We have to deal here with the phenomenon of contact infection from the ovary to the colon.

That such contact between an ovarian growth and the transverse colon is not only possible but probable has lately been demonstrated by Dr. Hertz, who, in a paper on "The Passage of Food through the Human Alimentary Canal," published in January, 1898, showed that, in the normal mechanism of defæcation, the greater part of the large intestine was pushed to a lower level in the abdomen, and that the transverse colon in particular sank into the pelvis, and that this last part of the large bowel only regained its previous position very slowly after the completion of the act.

In this way contact infection of the transverse colon from a growth in the pelvis may be explained, and such a contact infection will be further facilitated by the rise of intra-abdominal pressure which accompanies the act of defæcation.

Contact infection of the peritoneum from ovarian tumours has long been recognised, deposits of growth on the lateral pelvic walls, on the surfaces of the uterus and of the rectum which come into contact with the growth, and sometimes on coils of small intestine, being attributed to this process.

But by Dr. Hertz's work a wider area of contact infection from pelvic tumours is rendered possible, and this area must include the cæcum, the transverse colon, and the sigmoid colon.

A second case which has recently come to me for investigation affords another example, in my opinion, of contact infection of the large intestine.

In this case, a woman, æt. 40 years, was admitted into St. Mary's Hospital on September 6th, 1909.

She gave a history of having had both ovaries removed for a growth at another hospital in June of that year, and was discharged from that hospital on July 24th. Since that time she had continuously lost flesh. Abdominal swelling had developed and she had suffered from diarrhœa. On admission there was a considerable amount of fluid in the peritoneal cavity. An exploratory abdominal section was made and a condition of malignant peritonitis was found.

Afterwards the patient became slowly worse, the wasting and cachexia becoming more marked, and the abdominal distention being greater in spite of

frequent paracentesis, and she died about three months after admission.

At the autopsy there was about three quarters of a gallon of clear fluid in the peritoneal cavity. Both ovaries and Fallopian tubes were absent. The parietal peritoneum lining the pelvis and the lower half of the abdominal cavity was studded with numerous small white nodules of growth and similar nodules were present in large numbers on the peritoneal aspect of the stomach, the small and large intestines and their mesenteries, and the surface of the uterus. A few nodules were visible on the spleen, but none over the liver.

In addition to these changes the pelvic peritoneum was considerably thickened and had a dull white colour, and a similar appearance was presented by the peritoneum covering a large part of the cæcum and over a part of the sigmoid colon and rectum.

No new growth was found within any of the solid organs. The small and large intestines were contracted and the walls of the cæcum and of the lower part of the large intestine were much thickened, but there was no stricture in either situation, and there was no visible alteration in the mucous membrane of these parts of the bowel.

On microscopical examination the nodules of growth in the peritoneum were found to have the structure of a malignant endotheliomatous growth.

In the areas where the peritoneum was thickened and white, the change was due to infiltration of the peritoneum by the growth, and in the cæcum and rectum the growth also extended through the muscular and submucous coats of the bowel, and in the rectum the mucous membrane was found in several small areas to be replaced by growth. The condition of the wall of the rectum thus closely resembled that of the transverse colon in the first case, but without any stricture, and with no evidence of a visible primary growth in the bowel.

These two cases, gentlemen, are illustrations of a type of peritoneal metastasis which is commonly found in solid primary growths of the ovaries as it is in the new growths of the other abdominal viscera which have a close relation to the peritoneum, and in some cases it is the only form of metastasis which is found.

But there is another form of peritoneal metastasis, and a not very uncommon one, in which there is incontestable evidence that the peritoneal cavity has been suddenly flooded with tumour-cells, and innumerable deposits originate on all parts of its surface.

This state of affairs may be brought about by the rupture of a solid tumour, generally a soft and very malignant one, most often a sarcomatous, but occasionally a very malignant carcinomatous growth. But it is in the cystic ovarian tumours that flooding of the peritoneal cavity is most likely to occur as the result of the rupture of a cyst and the escape of the fluid carrying tumour-cells into the peritoneal cavity. The fluid, even if it escapes only in small amount, may rapidly penetrate throughout the greater sac of the peritoneum by the capillarity exerted by the viscera which are in close contact with each other, and the tumour-cells are widely distributed, and may settle down and start a metastatic growth upon any spot in which the flood may have left them high and dry.

There is one structure in the abdominal cavity, namely, the great omentum, which appears to exercise a great attraction upon tumour-cells which have been set free in this fashion, and it is not uncommon to find the great omentum converted into a thick heavy mass studded with closely set masses of growth, which sometimes give it an appearance resembling a bunch of grapes. Whether this is due to a folding of the omentum and greater capillary attraction, or whether to absorption of fluid by this structure or to some other cause I know not, but it is often very striking.

The behaviour of the tumour-cells, which are scattered broadcast over the surface of the peritoneum, depends upon their character—as to whether they are malignant or non-malignant in type.

If a non-malignant tumour bursts into the peritoneal cavity the metastases will be confined to the surface of the peritoneum on which they settle, and will show no capacity for invading the organs which underlie the peritoneum; this is comparable to a ruptured mucocele of the appendix.

If, on the other hand, the cells have malignant characters, they will, as they grow, invade the peritoneum and will spread to underlying structures; when the spread is at all marked, the process of invasion can be seen on inspection, especially in the great omentum, the tissues of which may become largely replaced by the penetrating growths. But the two processes, *i.e.* malignant and non-

malignant, can always be distinguished by microscopical examination.

Malignant metastases exercise a more marked irritant action on the peritoneum than non-malignant deposits, and a rapid accumulation of fluid in the peritoneal cavity is one of the modes of expressing that difference, and is generally associated with a rapidly developed cachexia and with usually a speedy fatal termination.

Finally, gentlemen, I have the notes of a third case, which illustrates yet another mode of production of metastases in primary ovarian tumours.

This is a *case of non-malignant cystic tumour of the ovary with transplantation within the uterus.*

The woman, æt. 49 years, was admitted into St. Mary's Hospital on April 26th, 1909. There was a history of only nine weeks' illness, the patient complaining of pain in the abdomen and chest; she had been in bed for three weeks prior to admission suffering from shortness of breath, and for a fortnight she had noticed swelling of the abdomen.

On admission the woman was extremely ill and was very dyspnœic. The abdomen was greatly enlarged; a large tumour was felt in the lower abdominal zone and was connected with the right ovary; it gave signs of encysted fluid in the abdomen.

There were signs of fluid also in the right pleural cavity, and two and a half pints of clear fluid were withdrawn. This afforded only very temporary relief; the dyspnœa became more marked, and the patient gradually sank and died a few days afterwards.

At the autopsy a large amount of clear fluid was found in both pleural cavities and in the peritoneum, and the lungs were greatly compressed.

In the lower part of the abdomen was a large tumour weighing over six pounds, which had replaced the right ovary.

The tumour had formed some adhesions to the posterior abdominal wall, the anterior surface of the uterus, which organ was retroflexed, and to the sigmoid colon and rectum. The right Fallopian tube was spread out over its surface.

On separation from its surroundings the surface of the tumour was generally smooth and polished and was faintly lobulated. A few large cysts filled with clear fluid were also visible.

On incision the tumour was found to be in part cystic and in part solid, but the solid parts appeared to be due to the filling up of the cysts with masses of papillomatous growth. The growth was soft and had a pinkish colour and in the central part it had undergone necrosis.

The left ovary and Fallopian tube were normal.

There were no deposits of growth in the peritoneal cavity and none were found in any glands or viscera, with the exception of the uterus. This organ was slightly enlarged and was retroflexed. On incision the endometrium was uniformly thickened, measuring about a quarter of an inch in thickness, and was soft and had a white colour. This thickened endometrium was sharply marked off from the muscular wall, which showed some hypertrophy, but which otherwise appeared to be normal.

On microscopic examination the ovarian growth had the structure of a non-malignant cystic adenoma with abundant papillomatous growth into the cysts. The tumour-cells were deeply columnar and were regularly arranged around a central lumen; a small amount of fibrous stroma separated the cysts from each other.

The endometrium was found to consist of closely packed tubules, which were cystic, and had histological characters exactly like those of the primary growth. This growth had completely replaced the mucous membrane of the uterus but did not penetrate into the muscular wall.

This case is an example—possibly a very rare one—of metastasis production on the inner surface of the uterus from a non-malignant cystic tumour of the ovary by rupture of a cyst into the Fallopian tube.

I regret that I am unable to illustrate these three cases by specimens of the deposits of the growths, but I unfortunately destroyed their value for that purpose by my zeal in prosecuting microscopical investigations upon them, and I have only the microscopical specimens which are placed under the microscopes.

In the study of the ovarian tumours which I have been enabled to investigate, I have been unfortunate in that I have been unable to obtain confirmation of the view which has had such a brilliant advocate in Mr. Bland-Sutton, that malignant ovarian tumours are frequently metastatic, and I have only encountered such growths in the ovaries in very malignant cases in which the peritoneal cavity has been flooded with the cells. I have

such an example in this specimen, a case of generalised melanotic sarcoma, in which the secondary nature of the deposits in the ovary and surrounding structures is quite obvious, and I am inclined to the view which I have expressed already to account for some, at any rate, of the combinations of visceral with ovarian tumours.

Dr. F. J. McCANN said one of the important points was the question of prognosis. When such cases were encountered, what opinion should be given to the patient or her friends? He thought the opinion should not be a very hopeful one. Mr. Bland-Sutton said certain of his cases lived two years and others sixteen months. His (Dr. McCann's) own experience was that no patient on whom he had operated for ovarian carcinoma had lived longer than two years except one, and that was a case in which there was metastatic ovarian growth secondary to primary cancer of the uterine body, and he would be glad to hear what Mr. Bland-Sutton's experience was in regard to such cases. It was now three years since he operated upon this patient and she was still perfectly well, and there was no evidence of local growth. He did not think it was accurate to say the patients whose cases were under discussion remained perfectly well for eighteen months. He thought that one of the most important indications that an operation was going to do well with a patient in the future was her recovery immediately after the operation. Patients with metastatic ovarian cancer usually remained much in the same general condition after operation. They did not gain flesh, and afterwards they developed a fitful appetite, were subject to irregular diarrhœa and constipation, and it was a gradual process of emaciation and losing ground which preceded the fatal termination. It seemed that the prognosis in those cases was even worse than in uterine cancer, and he would be glad if Mr. Bland-Sutton would say how long his patients lived after operation. With regard to treatment, he (Dr. McCann) believed it was the best practice to remove the uterus as well as the ovaries in those cases, to do a thorough radical operation, because it had been frequently demonstrated that the broad ligament lymphatics were infiltrated with cancerous deposit, and unless one made a clean sweep of the internal genitalia one was not freely removing the disease. He admitted that in the light of recent researches it was the duty of every surgeon who treated such cases to examine carefully for a primary focus; but many of the patients were exceedingly ill, and examination for such primary focus might make all the difference between success and failure, and an attempt to remove all the disease might kill the patient at the operation.

Mr. J. D. MALCOLM said the evidence brought forward by Mr. Bland-Sutton had impressed him very much. The statement that Mr. Bland-Sutton had found what was practically mammary cancer in the ovary after removing mammary tumours from the patient, and cancer in the ovary resembling that of the colon associated with a cancer of the colon, was very convincing. Since he knew those views he had kept a look-out for the occurrence of such things, and his observations tended to support Mr. Bland-Sutton's opinion. In one case he removed cancer of the sigmoid flexure with an ovarian tumour, and glands resembling those of the bowel were found in the ovarian growth, just as Mr. Bland-Sutton had described. With regard to the cystic tumours, although Mr. Bland-Sutton's views were generally well founded, he (Mr. Malcolm) was not fully persuaded that cancer in them was always secondary. Some years ago he (the speaker) said at another society that he had never seen a tumour of the ovary removed and found to be cancer microscopically without there having been recurrence within three years. Soon after that he had a patient who lived into the fifth year, and within the past month he had operated upon a woman from whom he had removed an ovarian tumour twelve years ago. The growth was very large, and consisted of thin-walled cysts full of glutinous material; only a very small solid part was discovered, and a section showed that if it was malignant it was a sarcoma, not an epithelial growth. The second ovary appeared healthy, but was also removed. That patient, who had passed the menopause, was in perfect health until about a year ago. When she came back in September, 1910, the abdomen was greatly enlarged. On opening it much free fluid was found, and the whole pelvis and many other parts of the peritoneum were covered with a growth which closely resembled the original ovarian tumour. It was difficult for him to think that that recurrent growth was associated with malignant disease in any part of the intestine at the first operation. He would like to know what Mr.

Bland-Sutton's explanation of such a case would be, whether possibly a solid part was overlooked, or whether it was a case which should be placed in a different category altogether. It was interesting to find two directly opposite views put forward by Mr. Bland-Sutton and Dr Spilsbury. The latter held what might be called the older view, but he (Mr. Malcolm) was inclined to think that the teaching of Mr. Bland-Sutton was applicable to many cases, possibly to all.

Mr. CHARTERS SYMONDS remarked that Mr. Bland-Sutton had said it would be one's duty before removing such a tumour to examine for a primary focus, and he (Mr. Symonds) supposed that if one found that primary focus, and it was inoperable, one would not go on with the other proceeding.

Mr. BLAND-SUTTON, in reply to the President, said that if on opening the abdomen of a patient for a malignant ovarian tumour clearly metastatic, and an inoperable focus of primary disease was found in another abdominal organ, it would be unwise to remove the ovarian masses unless they were a cause of great distress. Removal or otherwise would depend on the judgment and experience of the surgeon. The results of removing the uterus for cancer which had implicated the ovaries were unsatisfactory. It should be remembered that bilateral papillomatous cysts of the ovaries sometimes co-existed with primary cancer of the uterus, and might mislead the operator. Mr. Malcolm's case belonged to a different category. Mucin cysts sometimes ruptured and infected the peritoneum, the cell-elements engrafting themselves and forming mucin cysts which discharged secretion into the belly until it became distended with gelatinous material. He had a case under observation in which the abdomen required clearing out at intervals of three years. The patient, a cook, had been kept alive in this way fifteen years. One of the cases read by Dr. Spilsbury was primary colic cancer with secondary ovarian implantation. Contracting cancer of the colon was really cancer undergoing spontaneous cure. The second case was incomplete and could not be used as evidence. Although Mr. Bland-Sutton felt he could never claim Dr. Spilsbury as an apostle, he felt sure that now he was interested in this question he would soon be a supporter of the view that carcinoma of the ovary was invariably metastatic.

Dr. B. H. SPILSBURY, in reply, said he would like to draw the attention of Mr. Bland-Sutton especially to the second case which he (Dr. Spilsbury) had brought forward where the ovaries had been removed before he saw the patient, and the tumour was an endotheliomatous one. In that case there was no mass of growth in either the cæcum or the rectum, but the growth was spread for a long distance over the peritoneum, and was an endothelial growth reaching to the mucous membrane in the rectum. It was not the type of growth which one could associate with ordinary carcinoma as originating in the mucous membrane of the gut. And even if Mr. Bland-Sutton's view was right—which he did not admit—with regard to the first case, he did not see how it could explain the second, especially as it was in two areas of the gut, widely separated from one another. He put the other side of the question because he thought the two cases he brought forward illustrated it.

Mr. BLAND-SUTTON, in further reply, said the primary tumour in Dr. Spilsbury's case was not forthcoming. All tumours in the past in regard to this question must be regarded as false facts, that is, facts in regard to which no positive statement could be made, because the specimens were no longer in existence. The question would be decided, not upon past records, but from the acquisition of new facts.

November 21st, 1910.

THE Le Touquet Golfing Tournament for the professions of the Church, Navy, Army, Law, Medicine, and others will be held at Le Touquet, France, at the week end, December 10th to the 12th, 1910. Play will be as follows: Eighteen holes match play *v.* bogey under handicap. Class I: Handicaps 12 and under. Class II: Handicaps over 12. Players can take out as many cards as they wish. Prizes will be given by the Le Touquet Golf Club. Intending competitors should send names and addresses as soon as possible to L. ELIOT CREASY, M.R.C.S., L.R.C.P., 36, Weymouth Street, London, when information concerning vouchers for cheap week-end tickets will be forwarded. By the term "profession" is meant an occupation that properly involves a liberal education, or its equivalent, and mental rather than manual labour—especially one of the three so-called learned professions; hence any calling or occupation involving special mental and other attainments or special discipline, as editing, acting, engineering, authorship, etc.

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A LECTURE

ON

GYNÆCOLOGICAL THERAPEUTICS.

Delivered at the Polyclinic.

By FREDERICK McCANN, M.D.Edin., F.R.C.S.Eng.,

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LADIES AND GENTLEMEN,—I have chosen as the subject of my lecture this afternoon that of gynæcological therapeutics. But owing to the limited time at my disposal, I can only deal with certain general and local remedies which I have found useful in the practice of gynæcology. One cannot really over-rate the importance of general treatment in a gynæcological case. The usual practice when a patient comes to one is to make a local application, and perhaps write a prescription for some medicine. But, as a matter of fact, it is absolutely necessary, in all these cases, to go into the habits, the diet and various other facts in connection with each case, in order to get a grasp of what really is the daily life of the patient. And first of all with regard to the question of diet. You will frequently find that a large number of women afflicted with pelvic disease suffer at the same time from dyspeptic symptoms, and that the careful dieting of the patient will often do her more good than many of the local remedies. You must inquire into the quality and quantity of the food taken, and the intervals at which it is taken. Many women who are really invalids, or approaching the stages of invalidism, have a tendency to be always eating, to be frequently taking food between their ordinary meals, and especially numerous patent preparations. One constantly sees patients whose acquaintance with all the patent foods and liquids is astounding. You will frequently find that the

stoppage of them will do your patient a great deal more good than by any treatment of the uterus. Another important point is the question of alcohol. So many women who are afflicted with uterine diseases unfortunately sooner or later take to alcohol. It has been said that men drink from feelings of conviviality, women because of physical suffering. This is especially met with in two types of cases: in cases of chronic uterine discharge and cases of uterine hæmorrhage. Most of you, I daresay, have seen examples where curetting has been repeated once or twice, or oftener, but no benefit has accrued, and when all the time alcohol was the cause of the trouble. The patient feels weak in consequence of the blood loss; she takes alcohol to relieve her weakness, and that makes the blood loss worse. It is curious how often that is the case in practice. Time after time one sees cases where the influence of alcohol has been entirely forgotten, and energetic treatment has been directed towards the uterus when the proper treatment was to stop the taking of alcohol. You have so frequently to deal with the secret drinker, who denies ever taking alcohol, whose husband, even, may be unaware that she takes alcohol, and frequently, I am sorry to say, the doctor is unaware of the habit, but she takes it all the same. Always be on the alert for, and always be mindful of, the influence of alcohol in association with pelvic diseases. The next points in the general treatment are exercise and rest. At present there is a growing craze for exercise; whatever you have wrong with you, you are told "to take exercise." But you will find you have not infrequently to curtail the amount of exercise which the patient is having rather than to prescribe it. There is, however, a tendency in the treatment of patients who are suffering from certain pelvic troubles, notably dysmenorrhœa and chronic pelvic pain, to advise them to avoid exercise, and so to make them invalids. If you have a bad case of dysmenorrhœa, for example, exercise is useful between the attacks to prevent the attacks recurring, and the best form of exercise in these dysmenorrhœa cases—and I speak now of the spasmodic type of dysmenorrhœa—is horse-riding. But, unfortunately, that is an expensive form of exercise, and bids fair to be still more expensive in the future, and therefore it is not within the power of every patient. The next best exercise is fencing, and the next best is golf. There remains another form of exercise, namely, cycling, which is not so good, but it takes people into the open air, and so benefits their general health, provided they do not cycle too much or too far. It is, then, important in these cases to encourage the patient in the intervals of her attacks to take exercise rather than to make her an invalid. I have had only recently brought to me quite a young girl, who was the only child of her mother, who was evidently very fond of her, and this girl was being made an invalid, not only by her mother, but, I am sorry to say, by her doctor also, in advising that the child should be kept at rest a great deal when she ought to have been encouraged to romp about and take exercise. I am certain many young women are made invalids through following that course of conduct in daily life, and so come to look upon their menstruation as a disease rather than as a normal physiological function. Speaking of exercises generally, it is important in many gynæcological conditions to prescribe exercises, especially so in chronic constipation, and in that general descent of the abdominal contents which is associated with prolapse, and which is termed "enteroptosis." This method of treatment is largely practised by advertising quacks. They certainly are successful, and they do people good, because they have grasped the importance of treating many of these conditions by regular graduated exercises. But you can do the same if you write down for your patients the exercises you intend them to have, and show them how to do them. There is no doubt that exercises to strengthen the abdominal muscles are of immense advantage in chronic constipation in women, and of very great advantage in cases of displacement where it is not too pronounced. They tighten up the abdominal muscles and restore the retentive power of the abdomen, which is so important in all these conditions. That is why so many of these exercise people are making fortunes, and it is you who ought to be making them. It is a serious contemplation, but it is a fact that the profession has not yet grasped the importance of those exercises and what you can produce as a result of carrying them out. It is also obviously necessary in many cases to prescribe rest. Rest is of great importance in cases of chronic pelvic inflammation and in cases of bleeding. You would hardly credit it, but I often see

patients who get given to them from time to time various hæmostatic drugs for the purpose of stopping or diminishing hæmorrhage when the doctor has forgotten to tell them to rest. Resting in bed, keeping quite still, is the best treatment for hæmorrhage, whatever part of the body it may be coming from. These are the two important indications for rest: chronic pelvic inflammation and hæmorrhage. In the case of chronic pelvic inflammation it is very necessary for the patients to rest at the times of their periods. But if you exclude those cases, and also those in which the pain is so severe at those times that the patient must rest, treatment of painful menstruation should be to encourage the patients to be about as much as possible in the intervals. Another method of general treatment is the taking of baths. You can suggest two very good varieties of baths which are useful in many cases. One is the pine bath—that is, by adding to an ordinary bath liquid pine-solution, which you can buy at all chemists. And the other is the salt bath. That is made by adding Tidman's sea-salt or any other variety of salt which you may fancy. Both of them are useful in many pelvic affections. Indeed, the hot salt-bath is very useful in cases where there is chronic discharge, and it thus takes the place of sea-bathing. There can be no doubt that to those patients who are strong enough to have it sea-bathing is most helpful, provided they do not stay in the sea longer than ten or fifteen minutes, and that they do not bathe on very cold days. I think you will find that sea-bathing is productive of very much good in many women who have a chronic discharge. There is another variety of bath which is very good in pelvic inflammation, and that is the peat-bath. These are very well provided at Harrogate—quite as well as anything I have seen abroad—and the patients are kept in our own country, which is, of course, an advantage.

Another important method of general treatment is massage. It comes in useful especially in those cases of what is called "chronic endometritis." The usual treatment is either local applications to the cervix or endometrium or curetting, but you will find that the patients are frequently neurasthenics who have lost weight, who look ill, and who are ill, and no local treatment to the uterus is really going to effect a cure in those cases. It is the combination of the local treatment with the general treatment, which should consist in these particular cases of general massage and a certain amount of forced feeding, according to the Weir-Mitchell method, which is so useful. If you can fatten those patients, and give them another stone to their weight, your curetting will have produced a wonderful result which probably is not due to the curetting, but it may get the credit for it. There is far too much curetting done nowadays. Curetting is an operation which has very limited indications. Yet in this country, and I am sorry to say in the colonies, the number of women who are curetted is simply astounding; curetting alone, remember, in those cases of chronic endometritis is not going to cure the patient. She has the operation done, she is not cured, she is disappointed, and so would you be disappointed. But if in those cases you combine your curetting, when it is indicated, with massage and with feeding, you will get very much better results, in fact you will really do your patient good.

After these general remarks I must say one or two words about certain drugs that are useful in gynæcological practice. First of all with regard to drugs of the tonic class.

Iron.—Many of your gynæcological patients are probably anæmic and suffer from painful dyspepsia, and if you begin by giving them iron, certainly one of the strong preparations, you will make the dyspepsia worse, and the patient will probably go elsewhere. You have to treat the painful dyspepsia and get that pain removed before you prescribe iron. I shall not tell you how to treat dyspepsia, but for painful dyspepsia there is nothing like bismuth with a little opium in it. Stop the pain, whatever views on therapeutics you may hold. When you have done that you can start with your lighter preparations of iron, citrate of quinine and iron given in an effervescing mixture, which is a delightful preparation to take, and please do not give mist. ferri aperiens; it is awful stuff to take. It is a very good thing to get a reputation for prescribing nice medicine, especially when you are dealing with the fair sex. You can make excellent effervescing mixtures with these scale preparations, and as the condition improves you go on to give the old-fashioned steel drops, which, given with glycerine, still remain, in my opinion, one of the best ways of giving iron.

Another drug which is very good for gynæco-

logical patients is quinine, but do not give more than a grain at a time. We have all had patients who come to us and say they cannot take quinine because it upsets them. As a general rule too much quinine has been given. But only give one grain of sulphate of quinine, acid sulphurici dil. ♏v, syrupi aurantii ʒj, and aq. destil. ad ʒss. Patients nowadays know a good deal about medicine, and worse still, they know the limitations of the medical man, and they say "I cannot take iron, and I cannot take quinine." For such there is nothing like glycero-phosphate syrups, teaspoonful doses three times a day immediately after meals. Many will take glycero-phosphates who will probable refuse iron or quinine.

Drugs for the relief of pain.—There is no doubt about this, that you should if possible relieve the patient's pain. I remember I was very much impressed by an old doctor, who practised for many years, telling me one day, "I never let my patients suffer pain." That is a very good rule to follow. There seems to be such a fear of prescribing opium or morphia nowadays because of the danger to the patient of becoming a morpho-maniac. But it is as inconsistent to believe that a patient to whom you give morphia will become a morpho-maniac as it is to believe that every woman who has a glass of beer will become a drunkard. In taking opium one of the best preparations is liquor opii sedativus (Battley's solution), and another good preparation is nepenthæ. Battley's solution seems to be a better preparation than the officinal (liq. opii sedativus); why I do not know, but it is so. These drugs, given in appropriate doses, are very helpful in the relief of pain. Another drug, which is equally reliable in many cases, is bromide of potassium. So that, given a painful condition, you would first allay the pain with a preparation of opium and follow on with bromide of potassium, either giving the bromide alone, or giving the three bromides—ammonium, sodium, and potassium—together. There is, further, the whole group of pain-killing remedies which are largely used in dysmenorrhœa—antipyrin, phenacetin, pyramidon, etc. I shall not go into details with regard to them, save to say that the method of administration should be to give ten to fifteen grains at once, and then five grains every hour until thirty-five to forty grains have been given. If you work on that principle with those drugs in dysmenorrhœa, you will get a much better result because you get a graduated effect. And while I am on this subject remember never to prescribe antipyrin except with a cardiac stimulant —either caffeine or ammonia. *There are a number of drugs used in the treatment of hæmorrhage.* The best drug in the treatment of hæmorrhage is opium or morphia. When a patient is bleeding to death if you give her morphia you will probably save her life. It is the old remedy, which is given in the battle-field and used to be given a good deal in the Crimean war. It is infinitely superior to ergot or any of the other hæmostatics which have ever been produced, because it affects the nervous system; it allays that terrible fear of death which comes over the bleeder, and so, quietening the circulation, there is less hæmorrhage produced. But you cannot go on giving opium constantly. You give it at once in a bad case, and then follow with another drug, generally ergot. You can increase the efficacy of ergot considerably by giving it in combination with sulphuric acid in a prescription such as the following: Ext. ergotæ liq. ♏xx–xxx, acidi sulphurici dil. ♏x, magnesii sulphatis ♏xx, aq. cinnamomi ad. ʒss. It is better to begin with a smaller dose, and go on increasing your dose of ergot, and then gradually diminishing it. Bigger doses of ergot tend rather to make the hæmorrhage worse Cinnamon water is supposed to mask the taste of the ergot, but it is only substituting one form of nastiness for another. You cannot make ergot palatable. If the chemist gives a good fluid extract, I find this mixture very efficient in the treatment of hæmorrhage. The combination with sulphuric acid, also a hæmostatic, is very useful. In giving this for menorrhagia in the case of a fibroid where the patient objects to operation, or from some other cause, you should start giving it two days before the expected period, continue it during that period and for two days afterwards, and in the intervals of the period give bromide of potassium. Bromide of potassium was used in the treatment of bleeding associated with fibroids long before ergot was used. It is one of the oldest remedies, and it is said it is the potassium which benefits, acting as a uterine sedative. But there can be no doubt that it helps those patients very much. And incidentally I may remark that in the hæmorrhage associated with the pregnant uterus, if the

patient has pain and hæmorrhage of considerable extent you will give opium, but in those little hæmorrhages which occur and which may lead to a miscarriage, there is no drug to compare with bromide of potassium. *There are certain drugs which are useful for chronic inflammatory conditions, and foremost amongst these is iodide of potassium.* This, in chronic tubal disease, works wonders, and you have to give it and make the patient take it with great regularity, just as anyone would have to do who had syphilis. Tell them it is to become a religion with them taking the medicine regularly. You can prescribe it with gentian, giving grs. iii–v, but I still adhere to the old style sarsaparilla. Another remedy which is useful in these cases is mercury, liquor hydrarg. perchloridi, given in small doses. These are very efficacious in chronic pelvic inflammation, and also efficacious in cases of metrorrhagia, some of these cases being, I believe, syphilitic in origin. We hear very little about syphilis in the female generative organs, but it must be comparatively common for a large number of women to contract syphilis, and not a few of those cases of bleeding associated with a somewhat fibrotic condition of the uterus are, in my opinion, syphilitic in origin, and I have seen wonderful results from pushing iodide of potassium without giving them ergot. So that these are the two types of cases in which you give iodide; chronic tubal disease, where operation is not indicated, and secondly, to cases of suspected syphilitic trouble in the uterus.

So much for drug treatment. And now one or two words about *local treatment.* First of all with regard to douches. In the treatment of chronic discharges you can make a great reputation, but you must be precise in your directions as to what the patient is to do. First of all with regard to douching, you must write out exactly what you want the patient to do, because most women do not douche themselves properly, and the result is your treatment is not carried out rightly. To begin with, they should possess a syphon douche—Higginson's syringes are useless—and they should douche themselves lying on their back. The patient should have a bed-pan underneath her hips, into which the fluid can run. The douche should be given very slowly, and the height of the douche-can should be not more than two feet above the level of the patient's body. You want to bathe the vagina and not flush it. After the douche has been used the patient should remain ten to fifteen minutes on her back before she gets into the erect position again, when the remainder of the fluid is voided into the bed pan. The douching material should consist of two varieties. There should first of all be a solution of salt and water to clear away the mucus and pus. First add a heaped-up teaspoonful of salt to a pint of warm water, for it is ridiculous giving a medicated douche unless the muco-pus present in the vagina has been washed away, as your medicated douche does not come in contact with the mucosa. You clear away the muco-pus with salt solution. Indeed, patients are so enamoured of the salt solution that they give up the medicated douches, and it can be continued without harmful effects. It is well to write out a prescription for the salt, otherwise you do not get the patient to use it with that determination and assiduity which you require. A pint of that fluid should be used, and after that you use a pint of medicated solution. I need not waste your time by going over the drugs which are used for douching, but I will tell you one or two which I believe in myself. One of the best is tannic acid and sulphate of zinc, of each 20 gr. to the pint. I use much weaker solutions than are generally suggested and I advise you to follow that practice. I am convinced that many discharges are kept up by using strong remedies, and if you use a weaker solution you can always increase it if you find it is not having the desired effect, whereas if you start with a strong solution first of all you may do harm with your remedy. Potassium permanganate is better to use than ordinary Condy's fluid, 5 gr. of permanganate of potassium to the pint of water, or permanganate of zinc, 5 gr. to the pint, or the sulpho-carbolate of zinc, 10 gr. to the pint. The potassium permanganate is best for ordinary cleansing purposes and for women who are wearing a pessary. A very good combination is a mixture of alum and borax; that is a very old-fashioned prescription which you probably all know. Use of each half an ounce to the pint. An alkaline douche which is useful in cases of irritation associated with discharge is bicarbonate of soda a drachm to the pint, or bicarbonate and biborate of sodium, of each a drachm to the pint. You will find the latter useful in cases where the discharge is so irritating that it causes pruritus.

I must mention the great value of *soluble pessaries*. They have recently been improved very much, because certain chemists have been able to make a very useful glyco-gelatin base, which is much better than the old cocoa-butter. The patient wonders what is wrong with her when pieces of melted pessary come away. Many of the chemists have manufactured these pessaries of egg-shape, so that they are easy of introduction, and they act very well. One of the best pessaries to use is oxy-chloride of bismuth 10 gr. It is particularly useful in irritative conditions of the vagina, where there is a discharge and little bleeding points all over the vagina. The pessary should be introduced at bed-time and pushed inwards with the end of the douche tube, because if not inserted high enough they fall out again. They should be introduced every other night. If you are using a pessary with a cocoa-butter base the patient must have at least once a week an ordinary warm water douche, to clear out the remains of the cocoa-butter. Tell the patient beforehand that the melting pessary may come away in the discharge, otherwise you will receive a telephone message to say the patient is worse. It is the continuous action of the melting pessary on the vaginal wall which is so useful. There are other pessaries besides the oxy-chloride of bismuth; there is the sulpho-carbolate of zinc, 10 gr.; iodide of lead, 10 gr.; tannic acid, 10 gr. A very useful one is cocaine, which you use in cases of dyspareunia, half a grain, beginning with a quarter of a grain, because patients react so differently to cocaine. Another is conium, which is exceedingly useful, half a grain in cases of pruritus. Yet another important method of local treatment is by means of *powders* which you blow in through a speculum, or you can put the material into a little bag of gauze with a gauze tail, so that it can be withdrawn by the patient. The powder is placed up against the cervix. One of the best powders for this purpose is subgallate of bismuth, or a mixture of boracic acid and starch, equal parts. Boracic acid should not be used pure, although it is said to be not irritating. As a matter of fact it is frequently very irritating, but I do not know if this is due to some impurity. If you mix starch with it you diminish the chances of irritation. Another method is by means of the use of plugs. Remember all plugs should consist of medicated wool or lint, not ordinary absorbent cotton. If you leave the plug in the vagina for a day or two it becomes the happy hunting-ground of all the organisms in this locality. Remember you can grow organisms in glycerine, so that both the glycerine and the wool must be medicated. Use sal alembroth wool, or iodoform, although the objection to the latter is that it smells badly.

Another method of treatment—application of the chemicals to the interior of the cervical canal for the treatment of erosions—I have not time to go into now, but I pin my faith on pure carbolic liquefied from the crystalline state, and in a case which does not react to that, chromic acid just liquefied. The galvano-cautery for touching those erosions is most useful. In worse cases still, excise. So it is: (1) carbolic, (2) cautery, (3) curette, (4) excise. That is all you want to know about erosions. In such cases you must not persist in local treatment indefinitely; if you use carbolic it needs to be done twice a week, and if in six weeks' time there is no marked improvement, give it up. Do not go on continually making applications, because if you do you will keep up the local condition. In conclusion, I want you to take away at least one thing from this lecture, and that is, the great value of general treatment in gynæcological cases. Do not go on playing about, painting the uterus, whilst forgetting the woman who possesses it.

November 28th, 1910.

Culture of the Spirochæta Pallida.—Schereschewsky possibly does not regard the discovery of Ehrlich's arseno-benzol compound as final in the treatment of syphilis. He has spent a long time in attempts to secure pure cultures of the *pallida* in the interest of a vaccine therapy of syphilis. Cultures thus far obtained seem to represent an admixture. It is a striking fact that numerous other forms of spirochætes are readily cultivated, while the *Spirochæta pallida* is peculiarly refractory. It is, therefore, possible that in a given culture fluid containing the *pallida* the latter may not survive, while other forms accidentally present may flourish. At any rate such cultures cannot be made to communicate syphilis to any of the susceptible animals. Various other explanations are available. Thus the *pallida* may lose its virulence in the process of culture.—*Medical Record*, vol. lxxviii, No. 20.

A CLINICAL LECTURE
ON
PILES.

Delivered at St. Mark's Hospital.

By ASLETT BALDWIN, F.R.C.S.,
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GENTLEMEN,—There were pile doctors in Egypt long before the days of Moses, so that many causes given as due to our modern civilisation cannot be quite the correct ones. I think the real cause which is at the bottom of piles is the anatomical structure of that region. You will remember the vessels supplying the lower part of the mucous membrane of the rectum pierce the muscular coat two or three inches above the sphincters, and then ramify between the mucous membrane and the muscular wall of the bowel. The mucous membrane is very loosely attached, so that the vessels get no support, In addition, these veins have no valves whatever, so that there is nothing to relieve the pressure between them and the heart. There can, therefore, be no doubt that this is primarily the great cause of piles. Of course there are also secondary causes, and the first of those I think is constipation. Many people think it is the upright position of man which causes piles. But I had lately the case of a young lady, a board-school teacher, who about two years ago had a mass of ceiling fall on her back while she was at work. Up to this time she had had no trouble with piles at all, and declared she had had no signs of them. She had to lie in bed for a year, and after some time her digestion was upset and she had severe constipation. While she was lying up in this way piles developed, and they were so severe that I had to operate on them for the purpose of controlling the hæmorrhage. Of course anything else which causes back-pressure, such as disease of the liver, heart, or lungs, predisposes to piles. People who do not have sufficient exercise are greatly predisposed to back-pressure, and so are people who over-eat and over-drink. In the treatment of piles one thinks over the possible causes, and if any are present an attempt should be made to remove or modify them, and in addition local treatment will often be necessary.

First let us say something about external hæmorrhoids. There is an external form of pile which is very common, called thrombosed external pile. When you inspect this variety you find there is a swelling, more or less painful and tender, purplish in colour, radiating from the anus. These piles are generally caused by a strain, possibly at the water-closet, or by lifting weights, and the patient often notices first some pricking sensation at the part. Eventually it becomes more and more painful, and on examination there is found this purplish tender swelling radiating from the anal orifice. Far and away the best treatment for those cases is to sterilise the parts by washing them with an antiseptic solution of lysol, a drachm to the pint, or cyllin lotion, a drachm to the pint, or similar antiseptic, and if the patient is very sensitive, spraying external to the pile with a little ethyl chloride, and then injecting the pile with novocain or eucaine and adrenalin solution. That makes the part quite anæsthetic, and then the pile should be slit up, the incision radiating from the anus. One finds two varieties of clots in these piles; there may be one large clot occupying a cavity which can be turned out with more or less ease, or there may be a number of ramifying tortuous vessels completely filled with clot. No doubt the first variety is caused by the rupture of some small vessel and is produced by the strain which I have mentioned. The second kind is produced by infection of blood-vessels in the part by the *Bacillus coli communis*. When you have a pile laid open you should remove the whole of the clot. This is easily done if there is only one, but if there are a number of ramifying clots you will have to cut down on them by little snips with the scissors or knife, and pull the clots out with forceps or scoop. After swilling them over with antiseptic solution I put a compress on, and a piece of wool and a T-bandage, and by next morning the edges have healed together, and the patient gets no further trouble. I give ammoniated mercury ointment to be smeared on for a few days after washing with water. If they are left alone several things may happen: either the clot may get organised into fibrous tissue, or it may become encysted, or it may become calcified, or it may suppurate. In the latter case a fistula may result, and in the other cases it acts as an irritant, in many cases like a foreign body. In the first instance which I mentioned, where fibrous tissue forms, it remains as a

so-called fleshy pile or skin-tag. These skin-tags to many people give absolutely no trouble. In others they cause much pruritus; they may be excoriated and inflamed and require treatment. The best treatment for them is to cut them off absolutely flush with the skin, so that there is no stump left. It is risky to sew their cut edges together; they heal up well and safely if they are not stitched. You can understand there is danger in putting stitches in that situation, and the patients get on very well if the piles are cut off flush and dressed with ammoniated mercury ointment. There is another form of so-called external pile, the inflamed or œdematous external pile, and that is nothing else than one of the radiating folds of skin about the anus which has become inflamed. When you get a case of that sort, always examine it, to see if you can find any small excoriation or fissure, or even a small fistula, which may have led to this inflamed condition. If so it is a little more complicated, but in an ordinary uncomplicated case the patient should be kept in bed with the buttocks elevated on a pillow and some antiphlogistic measures applied, such as hot lead lotion on lint, and over that hot fomentations renewed every hour. As an anodyne I am fond of using orthoform; a small quantity of the powder blown over the part acts as an antiseptic and local anæsthetic. Another useful application is belladonna and tannic acid in the form of an ointment. Some recommend cold for these conditions, but I think heat is more comfortable and safe, and is certainly more in accord with modern views of the treatment of inflammation. When the condition does not clear up there is probably some undiscovered cause, such as fissure or fistula, and in those cases it is best to give an anæsthetic and dilate the anus, and find the cause and deal with it. Then the one or more radiating thick folds which get inflamed can be cut off with scissors and again dressed with ammoniated mercury ointment.

There is another form of external pile, and that is the sentinel pile, which is of considerable interest because it is so frequently associated with anal fissure, which causes such great suffering. The symptom of anal fissure is pain on defæcation lasting several hours. When you examine such a case you find a small pile, a solid-looking fleshy pile half the size of a pea, and the anus tightly contracted. If you open the anus gently with your fingers, at the same time asking the patient to strain down, you can see the fissure leading up the bowel from the base of this pile; it is usually situated posteriorly. Many people teach that in all these cases where temporising treatment does not heal a fissure it is necessary to divide the sphincter and do a rather elaborate operation. In all the cases I get which I think are suitable—and they are those where there is a sentinel pile, and where the condition has not been present too long—I give the patient gas in the left lateral position and thoroughly dilate the sphincter for about two minutes, and then cut off the sentinel pile, examine for a small polypus at the upper end of the fissure; if present it is removed. Then swab the part with an antiseptic solution; a little wool wrung out of the solution is applied outside, also wool and a T-bandage. Then the greater number of cases heal completely without further trouble, provided care is taken to keep the bowels regular and the motions soft. The patients are also instructed to keep the parts washed with water twice daily and afterwards to apply ammoniated mercury ointment. A number of fissures will heal without dilatation, but where there is a sentinel pile present I think that is the treatment to adopt. There is one little detail in connection with the giving of gas in these cases which is useful. If you give a patient gas in the left lateral position, directly you attempt to touch the anus and dilate the sphincter they shoot the lower limbs out straight and nothing can be done. To avoid this, I put a short jack towel round the patient's neck and round the flexed knees; he is then trussed up like a fowl, and you can dilate the sphincter and have him entirely at your mercy. That will save you a good deal of trouble. Now with regard to internal piles. Numbers of varieties of internal piles have been described, but when one comes to sort them down there are practically only two; the others are varieties. The first is the capillary arterial hæmorrhoid, the second the ordinary venous internal piles. The first variety, when you dilate the sphincters and look at them by means of the speculum, are very red, often very friable, and easily bleed. They are slightly elevated patches on the bowel, consisting mainly of arterial capillaries. A good deal of hæmorrhage may come from those, enough to make a patient quite anæmic. The best way, I think, of treating them is to put in a speculum and just sear the whole of the

surface of the hæmorrhoid with the actual cautery, heated to a dull red, and then smear the parts over with eucalyptus ointment, and put on a pad and bandage. Another method which acts very well is to put ointment all round the anus to protect the skin, and after inserting the speculum to touch the whole of the surface of the pile with fuming nitric acid. Let it act for a couple of minutes, and then neutralise it with bicarbonate of soda solution and smear it over with eucalyptus ointment as before. These cases are thus cured with little trouble and risk. Generally one application of the cautery will cure them, but two may be necessary. Carbolic acid may be used in the same way, but I think nitric acid is better, and the actual cautery best of all. With regard to the venous internal piles, not every case of this condition requires operation. In many cases palliative treatment is all that is necessary. I believe constipation is one of the greatest causes, and that is the first thing to see to. Many people are constipated because they do not drink enough water. So I start these cases on a tumblerful of hot water when they wake, that is to say three-quarters of an hour before breakfast, another at night, and I try to get another in some time during the day, but not at meal times, say at 12, 3.30, or 6. In addition, these cases may want some laxative. You may have heard it said that aloes is not suitable to give in piles, as it tends to produce them. I have not found that so myself. A pill I often prescribe contains half a grain of extract of aloes, one grain of sulphate of iron, one quarter of a grain each of extract of belladonna and extract of nux vomica. That is valuable given one, two, or three times a day after meals. If at first a patient finds three a day necessary, it will probably soon be found that two are sufficient, and later, perhaps, one a day; sometimes they can leave it off altogether. Locally they should have some astringent and some anodyne, and a very useful combination, if you give an ointment, is to give one composed as follows: tannic acid, 2 drms.; belladonna ointment, 1 oz.; hamamelin, 16 gr. A more expensive drug to add is adrenalin chloride, which you can use in a strength of 1 in 1000 to 1 in 5000, according to the trouble there is with the hæmorrhage. In many cases you must order an ointment introducer to make it of any use, where the nozzle is passed into the anus, and pressure made on a piston, when the ointment is forced into the bowel. As a rule patients apply the ointment externally and it never touches inside. Ointments are often more useful than suppositories, because the latter tend to slip up too high. If you give suppositories it is a good plan to tell the patient to insert the suppository into the sphincter, to hold it there for a minute with the finger, and in that time some of it is melted and remains where it is required. A suppository which I use a good deal contains hamamelin 2 gr., extract of belladonna ½ gr., and 5 gr. of tannic acid. That is one of the most useful suppositories I have come across for the treatment of internal piles. By giving attention to these details, and by making the patients sit on hard chairs instead of soft cushions, and take a certain amount of exercise during the day, by not over-eating or over-drinking, and by getting the bowels to empty easily and thus to avoid straining at the water-closet, many cases of piles will be relieved or cured and cease to cause trouble. But if you are not successful, the conditions are present as follows: piles constantly prolapse during defæcation and do not go back of themselves, necessitating the patient replacing them with his own fingers. Or if the piles come down at other times, for instance, when he is walking about, or if the hæmorrhage is not controlled, or if there is a constant aching and dragging pain in the bowel which begins to affect the patient's nerves, I always look upon those conditions as indications for operation if the palliative treatment does not give the necessary relief. Then there is the question of the kind of operation for choice. I think there is no doubt that the most generally useful operation for piles is the ligature operation, and so I will go rather into detail as to the method which I always use for the ligature operation. The patient's bowels must be well cleared out, and I think the most useful drug for that is castor oil, because of its constipating effect afterwards. The aperient should be given two days before you want to operate, either towards the evening or in the morning, according to how long the aperients generally take to act in the patient, or according to their habit. So suppose you want to operate on a Wednesday morning, give the aperient on the Monday morning or evening, and then on Tuesday evening you will wash the bowel out with an enema. On Wednesday morning you will wash it out again, at least three hours before the operation.

That is not sufficient for some people. The first essential is that the bowels shall be well cleared out so that they do not act while you are operating. Beware of giving anything of the magnesium sulphate variety, because it may cause dribbling to be going on during the operation, and as an aseptic operation is what you are aiming at it is rendered practically impossible if that occurs. The position in which you put the patient may be either the lateral position or the lithotomy position. Personally, I think the lithotomy position is the more generally useful. Then the anus is very gently dilated with the first two fingers, gradually until the sphincter is felt to easily give way in all directions. If you feel any sudden tear you must of course stop using any force at once. You will, however, find that by steady gentle pressure the whole sphincter will be dilated; then four fingers may be used or the thumbs. The bowel inside is thoroughly mopped out with antiseptic solution, 1 in 1000 perchloride of mercury or cyllin solution, one drachm to the pint, or lysol of the same strength. Having thoroughly disinfected the parts in that way, each pile is caught in the Spencer Wells clip. Before you start operating at all, catch hold of each pile in the clip, or you will be sure to miss one or other of them. The general situation for these internal piles is, that there will be one in front, called the perinæal pile, and a right lateral pile, and a left lateral pile. Sometimes there will be another small one here and there, that is to say, in front of or behind the left lateral pile. Each will be seized with Spencer Wells forceps, and it is advisable to deal with the small one behind first. If you start operating towards the front, blood trickles back and obscures the area of operation. Draw the pile towards you, and with good sharp scissors or with shears invented by Mr. Salmon, who founded this hospital, a snip is made at the junction of the skin with the mucous membrane, care being taken not to injure the sphincter. As soon as you make that snip you will find the pile with the mucous membrane can be easily stripped from the external sphincter by a little pressure with the finger or with the closed scissors, or by further snips. It is important to remember that you do not want to produce any constriction of the bowel in this operation. Having separated the pile from the external sphincter, hold the pile forwards and cut through the mucous membrane obliquely inwards, towards the upper end of the pile, so that by the time you have done that the pile is pear-shaped. There is no risk in cutting inwards, because the vessels of the pile enter at the upper end in the middle of the stalk just under the mucous membrane. When the ligatures are applied after this manœuvre there is very little chance of constricting the bowel. So having this oblique cut made towards the upper end, the pear-shaped pile is loose except at its upper end or stalk; your assistant will then draw the pile away from the bowel wall, and you will put a strong ligature into the groove which you have made between the pile and the bowel wall, and the pile is pulled downwards by the assistant. You tie the ligature so that the knot is on the mucous membrane side of the stump. It should be tied very tightly with three knots. If it is a very big pile indeed, it is advisable to snip away part of the pile, not more than one third of it, to see that there is no bleeding from it. Never snip away the whole of it. I have said nothing about transfixing the pile, because it is unnecessary, you only put a ligature round the pile. If it is a very large one and you think it is not safe to put a ligature right round it, cut it in two and treat it as two piles. If it is small, cut none of it off; if too much is cut away the ligature may slip off and cause serious hæmorrhage. If there are redundant skin-tags they are trimmed up, care being taken not to remove too much for fear of causing contraction of the anus. Any small bleeding points are now looked for and ligatured with fine thread. That is all that many people do. I generally do a little more than that. I take hold of the cut edge of the mucous membrane, draw it down with forceps, and suture it by chromic gut to the skin. That may require four or five sutures. In that way the mucous membrane is approximated to the skin and there is no raw surface to be seen at all. I blow on the parts some powdered orthoform and Aristol, and put into the bowel a half inch rubber drain tube, about four and a half inches long. The tube should have a hole on each side at its upper end, and a ligature attached to it, so that it does not slip up the bowel out of reach. Some gauze is packed round the tube outside the anus. The object of the tube is to permit flatus to escape easily and let blood out if there is hæmorrhage. There will not be any concealed

hæmorrhage if you do that. Let the rubber tube touch the T-bandage which holds the whole dressing in place, and if there is hæmorrhage it shows itself. It is safer to do that, because if hæmorrhage takes place after the operation, in spite of the dilating of the sphincter it will bleed into the bowel, and the hæmorrhage may not be discovered until the patient faints perhaps, or becomes pale. In four days' time give a smart aperient of castor oil. You want to give such an aperient that the patient is obliged to go to the water-closet although he does not want to have the bowels opened because of the possible pain. But if you do what I say, you can assure the patient he will have no pain on defæcation. One man, who was not operated upon by me, said the pain on going to stool was like that of passing a hedgehog backwards, but I believe neither the olive oil enema nor sutures were used. It is evidently worth taking some trouble to avoid pain like that. After the aperient has been given, about the fourth day, when the patient begins to feel that the bowels will act, I have four to six ounces of olive oil injected into the bowel and then the tube withdrawn. The oil is held as long as possible, and the combination of sewing the parts up and applying warm olive oil makes it painless. I always have the parts irrigated twice a day with antiseptic solution. On the ninth or tenth day the ligatures come away, after which I gently insert the lubricated finger to its full extent into the bowel to see there is no contraction present; if there is, the insertion may have to be repeated several times. Always do that as soon as the operation is over, and also previously to see if there is any other disease higher up. Many cases are diagnosed as piles, but the real trouble is disease higher up in the bowel. In fourteen days allow these patients up for a short time; they walk round a little at first, and in three weeks they are generally convalescent. These patients are allowed to get out of bed to have a bath and the first passage of motions, and also subsequently if the patients wish it and are strong enough and there is no hæmorrhage or other contra-indication. If there is any hæmorrhage when the bowels act, keep the patient recumbent, or if a tendency to œdema in the surrounding skin, keep the patient in bed and keep strong lead lotion on the parts—one part plumbi sub-acetatis, three parts of water, on lint. The skin-tags which are so frequently apparent after this operation generally disappear under that treatment.

Another extremely good method of treating piles, especially where there are only two or three uncomplicated piles, is by means of the clamp and cautery. Two varieties of clamps are used. One is Smith's, and the other Gowlland's, of which here are specimens. They seize the pile in rather a different way, and many people prefer Gowlland's clamp because the upper part of the pile is the last part to be released, whereas in the other it is the first part to be let go when the operation is over. There is another advantage in Gowlland's clamps, and that is before you start operating you can put a clamp on each pile which you intend to remove before you start burning them off. Here is a photograph of a case before operation—two large piles with the Gowlland's clamps in position. The advantage of putting clamps on each pile before burning any off is that there is no fear of tearing open the cicatrices, which easily might take place if clamps had to be applied after any piles had been burnt off. Part of the pile is cut off with scissors, and then the stump is seared down as far as the clamp with Paquelin's cautery heated to a dull red heat. If you heat the cautery to a white heat hæmorrhage is very likely to take place. This is a very excellent method of treating two or three uncomplicated internal piles. If there are more than three, there is not room to apply a clamp to each before using the cautery. Convalescence is very easy after it, and secondary hæmorrhage is excessively rare. I have never known secondary hæmorrhage to take place after it, and healing is quicker than after ligature, and the patients get very little pain. I prefer the clamps with curved handles. Another very good method of operating on one or two uncomplicated internal hæmorrhoids is the crushing operation with Allingham's clamp. You see it is a very strong clamp, worked by means of a screw. The pile is pulled between the jaws of the clamp, which is screwed up lightly and is left on for a few minutes, and the operation causes practically no after-pain, there is no hæmorrhage from it, and the parts rapidly heal up. But it must be uncomplicated internal piles, and there should not be more than one or at the most two piles, or, again, hæmorrhage may be caused by the application of the clamp to

any piles after the first two. There is another method which has been used a good deal for one or two uncomplicated piles which can be prolapsed through the anus, and that is the injection with carbolic acid. The solution which is used here contains twelve grains of carbolic acid and half a drachm of water and half a drachm of glycerine. It is necessary to get the piles prolapsed: swab them with an antiseptic and pass the hypodermic needle under the base of the hæmorrhoid and inject about three minims, and one or two minims into other parts of the pile, then smear it over with an antiseptic ointment and pass it back into the anus. That method has been strongly advocated by many people, and it is used when patients object to anything more radical or when they cannot possibly lie up in bed. I have never seen any harm follow. The worst I have seen happen after it was that a patient complained of severe pain in the abdomen for a short time, but many cases have been operated upon in that way with safety, although some cases have gone wrong, and some have died. I believe those cases which have died have been operated upon by quacks, who probably do not do the operation with proper precautions. The objection to the operation is that it is not as a rule a radical cure. The piles are generally back again in two or three years' time. To tide a person over the difficulty for two or three years it is useful. The injection is practically painless and people are not necessarily confined to bed after it, and it does not require a general anæsthetic. Patients should be warned that if the piles prolapse after injection they should be immediately replaced, or they may be caught by the sphincters, swell and become strangulated. Also they must see that the bowels act easily and without straining, and it is advisable to use an antiseptic suppository, such as one containing three grains of aristol, twice a day.

There is another method of operating for piles about which you have no doubt seen the correspondence in the 'British Medical Journal' lately, and that is Whitehead's operation. Many surgeons strongly advocate this method, and there are a greater number who oppose it. I think the truth of the matter is this: the operation should be entirely confined to the hands of an expert, and should not be done by the ordinary individual. In the correspondence which has appeared in the 'British Medical Journal' several well-known surgeons have said they have never seen a case which has not been a success when treated by Whitehead's operation, or where there has been trouble following it. In the last year at this hospital I have seen at least half a dozen cases that could not be called successful. For instance, last Tuesday a woman came to this hospital who five years previously had been operated upon by a very well-known surgeon and the operation performed was Whitehead's. Ever since that time she had had a mucous discharge from the anus to such an extent that she has always had to wear a diaper held on by a T-bandage, and the mucous discharge caused pruritus and irritation round the anus. That cannot be called a success, but is rather a catastrophe. One of the patients who has been here has complete incontinence since the operation, and considers his life has been wrecked in consequence. Perhaps it is not fair to attribute that to Whitehead's method, because it might happen to anybody operating that he might tear the sphincter through. But in the last year, as I have said, I have seen six cases of trouble following Whitehead's operation, and the forms of trouble are discharge of mucus, owing to the mucous membrane being attached too far down, pruritus and stricture. I have had two cases of stricture here; one, a woman, has been attending for the last five months, but is now well; the part has been dilated with the finger—bougies did not answer so well; the other, a man, has ceased to attend. Therefore I should not recommend anyone to do Whitehead's operation who is not skilled in anal surgery. In those cases where there is a complete ring of piles all round the anus and also in the cases where there is considerable prolapse of mucous membrane, it is a useful operation. But not only does the operation want doing carefully, but the after-treatment also wants more care. Mr. Whitehead himself, in his description of the operation, says he seldom saw the patient again. I do not think that is what one would care to be the case now. These cases want antiseptic irrigation of the anus twice a day, and the application of an antiseptic ointment, such as ammoniated mercury, to the parts, because the ideal thing is to get healing by first intention. Still, to accomplish that, as you will readily understand, is most difficult. In fact it is almost unknown for the skin and mucous

membrane to heal together absolutely by first intention. If that does not take place, the stitches cut out, and a raw surface is left, and there is a tendency to stricture, and in some cases the mucous membrane appears to be anchored too far down, and that leads to this constant discharge of mucus, with following pruritus and general irritation. Another excellent plan when the piles are not too friable, is to seize the pile with clip forceps and pull it forwards. The base of the pile is then seized, in the long axis of the bowel, by a forceps with long, narrow blades and having a catch, so that they will hold on of themselves. The pile is then cut off with scissors, but a stump about a quarter of an inch long is left projecting beyond the blades. A piece of fairly stout, ordinary catgut about one foot long is now taken, and a round, straight needle threaded on each end. One needle is now passed under the mucous membrane of the bowel just above the tip of the forceps; it must go deep enough to get under the vessels supplying the pile. Half the length of the gut is now drawn through and the gut is tied in a reef knot. The vessels supplying the pile should now be controlled. One needle is then passed through the upper end of the stump from right to left and the other close to it, from left to right; the two pieces of gut are pulled taut and tied in a reef knot. This is repeated about eight times to the inch along the whole length of the stump and the clamp is then removed. Each pile is treated in the same way. There are several modifications of this method. In one of them the gut suture is placed as before, but is only threaded with one needle, and before tying is drawn through so as to leave one end long and the other end short, the needle being on the long end. The needle and its gut are now passed round and round the stump and the clamp, till the whole length of the former is included, but the suture is not pulled tightly. The clamp is now withdrawn, and by pulling on the two ends of the gut the edges of the stump are approximated without puckering. The lower end is now tied and both ends cut short, so that in this method there are only two knots—one at each end of the suture. These methods give excellent results when the piles are not situated too closely together.

November 28th, 1910.

A CLINICAL LECTURE.

Delivered at the Seamen's Hospital, Greenwich.

By H. S. PENDLEBURY, M.A., M.B., F.R.C.S.,
Surgeon to St. George's Hospital, and to the Royal Waterloo Hospital for Children and Women.

GENTLEMEN,—This afternoon I am going talk about two or three common operations, viz. circumcision, and the radical cure for hydrocele of the tunica vaginalis. At the Hospital for Children, in Waterloo Road, we are continually receiving children for treatment who have been circumcised either incompletely or with some resultant deformity. That being so I thought there were certain points that we might consider as regards operations for circumcision.

I will take the treatment of circumcision, first of all in infants and then in adults. In the case of infants you ought always to be careful to keep the child under observation for eight or ten days, more particularly if you know nothing about the child and have not seen it previously, for in more than one case at the Children's Hospital it has happened that the child has taken scarlet fever, measles, or some other infectious disease a couple of days or so after the operation has been performed. Keep the child in, therefore, for the length of time I have mentioned, and so make sure as far as possible that no fever or cough or other malady is likely to complicate the operation. What I am telling you to-day is the result of fifteen years' experience at the Children's Hospital.

There is no doubt that children who have a superfluity of prepuce ought to be circumcised if their parents are agreeable to that being done. One of the chief reasons why children should be circumcised is that in the event of their catching any venereal trouble in after life they are much more likely to have a less severe attack than a man who is unable to retract his prepuce freely and to keep the parts beneath perfectly clean. On that ground alone I think it advisable that certain children should be circumcised.

As to the actual operation, as you know, in children you have often a certain amount of adhesion between the cutaneous mucous membrane and the covering of the glans penis. All surgeons have their own method of beginning the operation.

Personally I always like to draw the prepuce forward with a pair of forceps or with the fingers and estimate more or less the amount of skin that you want to remove. Then I press the glans penis back and grasp the skin between dressing forceps making certain that the glans is sufficiently out of the way before cutting off the skin. I now take a pair of scissors and cut off the portion of skin desired in such a way as to leave an extra Λ-shaped piece below. When you have cut through the skin you will find, looking at the penis, that you have a raw surface left where the frænum has been cut, and you want to cover that piece of raw surface by suturing the skin to the fringe of the cut frænum. Nothing makes a more ugly or less æsthetic result than a thick piece of skin hanging at the lower portion of the penis. That can be avoided if you pay a little attention when cutting through the skin. When you have cut through the skin in this way cut through the mucous membrane along the dorsum and then circularly round, leaving a fringe of mucous membrane around the glans to stitch the skin to.

After doing that the next thing is to stop the bleeding, and there is very seldom any trouble in doing it. But there are probably few surgeons who have done some hundreds of circumcisions who have not had one case in which profuse bleeding has occurred. That, I think, has been almost invariably due to carelessness. Probably the surgeon has not been at sufficient trouble to see that the bleeding has stopped. Little children, especially when given an anæsthetic like chloroform, suffer from shock, and then when they recover from this condition there is a tendency for bleeding to occur. It is therefore just as well at this point of the operation to ascertain the character of the patient's pulse so as to guard against such an occurrence.

Then comes the question of sewing the skin to that little fringe of mucous membrane. I would always use fine catgut and always interrupted stitches—one at the frænum, and two others at the side towards the dorsum. You can never tell how much swelling is going to occur owing to effusion of serum, and therefore it is best to leave a good interval between the stitches. It is well, I think, even in infants to have about three of these stitches through the skin and mucous membrane. There is an advantage in using catgut for the stitches as there is no trouble in taking them out; they simply absorb or fall out. I always stitch, because certain cases have been brought into the Waterloo Hospital for re-circumcision—cases in which these two or three little stitches have not been put in where the glans has slipped back and the patients have consequently had contraction of the orifice in the prepuce.

In reference to the dressing everybody of course has his little pet fad. One must remember that these children will wet themselves, so that it is no good going in for an elaborate antiseptic dressing. But I think you will make a nice dressing if you take a little strip of plain sterile gauze three layers thick, and just a small portion, sufficient to go round, should be smeared over with a little boracic ointment or any simple ointment. Supposing here is the glans, and here are the little joined surfaces with the three stitches, and you are looking at the glans from the side, the ointment will come round in this sort of way so as to be in contact with the actual wound. You then take the two ends of the dressing and twist them round each other. That keeps them in position and makes a little pressure which will help to stop any bleeding. After that put a little wool round and then put on an ordinary napkin so as to prevent the child from interfering with the part. Keep changing the dressing as often as required, and the simplest way of doing it is when giving the child a bath.

As regards keeping children in bed, it is generally wisest to keep children of five or six years of age in bed for about a week until the parts have settled down. It saves a good deal of swelling and other little complications.

Now as to the question of adults, the operation is of course a much more serious one than in the case of children, and I should never attempt it unless you have had an opportunity of preparing the patient for it. An adult should come into the hospital the day before. If he is to be operated upon at home see that you get a nurse for him, or a couple of nurses if he is a particularly nervous patient. He should have such diet as will keep his bowels open. He should avoid smoking. These patients take an anæsthetic better if they have limited their smoking for a week or two. Then you should give him some bromide the day before the operation to produce a little depressant effect.

The operation is practically the same as the other we have just described. You take exactly the same precautions and again interrupt your stitches. You want to get perfect apposition. But do not put the stitches too near together, so that if there is any exudation of serum it may escape between the prepuce and the mucous membrane of of the skin. You can use catgut for the stitches. As regards the dressing, not a bad one is just the same as that which you use for a child. Some people like to use a little collodion with the gauze, but if a man has been circumcised and the wound has not properly healed, the act of taking off the dressing usually causes him much pain. I personally always avoid collodion and simply put an ordinary gauze dressing on, twisting it as in the case of a child.

The after-treatment should consist, I think, in keeping the patient in bed. A great many patients like to get up almost as soon as the dressing is applied, but I invariably insist on my patients going to bed for seven or ten days. By that means you will get much better healing and less thickening of the skin and mucous membrane, and, as a rule, good firm union. If you allow the patient to go about too soon the dressing is apt to get out of position, and considerable trouble may result.

The chief trouble of after-treatment, as you know, is to prevent an erection. For that purpose the man should have a cradle, so that the bedclothes may not come in contact with the penis and make it very warm, and let him have an ice-bag by his side to apply to the part at the first sign of discomfort. Supposing a man had been operated upon at 9.30 in the morning, you may give him a dose of sodium, ammonium, and potassium bromide, ten grains of each, about 5 p.m., and at 9 o'clock at night another dose of the same sort. Remember that the bigger the man the bigger the dose. If he gets the least feeling of discomfort from an erection, or a feeling of aching or throbbing, the whole thing settles down under the treatment I have named, and the presence of the ice-bag gives the patient confidence and also ensures that he will sleep better. If you attend to these little details you will add a great deal to the comfort of your patient, because remember after circumcision healing may be delayed if, for instance, a man gets an erection at night when asleep. Circumcision is an operation that every surgeon has to do, and if it is not performed with efficiency it may easily injure his reputation. It is well worth while, therefore, to pay attention to all these details when you are doing your operation.

The next operation I want to consider is the radical cure of hydrocele of the tunica vaginalis. This affection may be a part of tuberculous disease or syphilitic disease, or ordinary traumatic orchitis. In each of these three maladies there is a tendency to get a little effusion into the tunica vaginalis, and their treatment is to treat the cause. The treatment of an ordinary acquired hydrocele may be put under the following three heads: (1) Excision of as much of the sac as possible; (2) tapping and injection; (3) tapping only. There is only one plan of sound treatment in the case of a healthy man in the prime of life, and that is excision of the sac. In one method of treatment the sac is opened and simply turned inside out. I myself do not believe in that, for you have a large surface left for secreting fluid, and the contents of the scrotum remain more bulky. There are cases on record where even the small amount of epithelium covering the testicle has kept up its secreting power, and there has been a recurrence. As to the treatment by injection, it is a most uncertain thing. I have not had much experience of this, because I do not employ it. It used to be taught me that only 25 per cent. of the cases are cured by this treatment. With reference to tapping, the percentage of cures is very small indeed.

As regards the actual operation itself, here again attention to details produces excellent results.

Grasp the testicle with the left hand, and make tense the skin over the swelling. Make your incision almost from top to bottom of the swelling. You then incise carefully on to the distended tunica vaginalis. If you want to do this operation easily do not open the sac until you have the whole thing separated out as far as possible. There are a few cases in which you can get the whole thing out without having to cut round the testicle at all. Go gently down until you can see the thin shining sac, then gradually separate it off from the surrounding tissues with your director until it is only adherent to the testicle. If you want to do this operation with ease you will find it of great importance to separate the sac carefully from the cord and right away to the edge of the testicle before

you cut it. The freer you are able to get it the less difficulty you will have with hæmorrhage. Then open the sac and the contents will flow out. Cut the sac away, leaving a small fringe round the testicle. You will nearly always find a fair number of vessels, and these sacs are sometimes very thick, so that unless you leave a fringe round the testicle you will find it hard to grasp these vessels. You must be sure in all these cases to stop the hæmorrhage before you close the wound, otherwise you may perhaps find the patient's scrotum full of blood a few hours later. After you have done that sew up the wound. I invariably drain these wounds, because it makes things more comfortable for the patient. As a result of the large amount of raw surface that you get by doing this operation, there must necessarily be a good deal of oozing, and I am perfectly sure that in these cases it prevents swelling and distension of the scrotum. You should drain for about twenty-four hours and then remove the tube and re-apply the dressing.

With regard to the dressing, personally I like ordinary sterile gauze, just a little gauze arranged in such a way that it holds up and takes any pressure off the cord itself. The man of course must go to bed. Many surgeons dress with collodion, but it prevents the natural oozing which nearly always occurs in these cases. You must keep the patient in bed for ten days at least. I would not undertake a case of this kind unless he promised to stay in bed for that time, because otherwise I think the results are not so good. The man should wear a suspender for some weeks after the operation.

Concerning the preparation of the skin before this operation I should like to give my experience of the value of the iodine method. Make up a 2 per cent. solution of iodine in rectified spirit. Do not take methylated spirits, because the fumes from this mixture will probably make your eyes water and irritate the skin of the patient. Supposing you do the operation at 9 o'clock in the morning, you will see that the man has had the parts shaved and an ordinary bath the previous evening. Then at 7 o'clock next morning the scrotum and neighbouring parts should be swabbed over with the iodine solution and wrapped in *dry* sterile gauze. Now there is no good applying a solution of iodine to skin soaked in fluid of any sort. If, for instance, you wash the skin with soap and water and put on the iodine immediately, the skin soaks up the water and its cells are then incapable of taking up a proper amount of the iodine solution. Immediately before the operation you paint over the part again.

You ought to have two knives for the operation, one to cut through the skin and the other to deal with the rest of the operation. This method of skin preparation has one or two disadvantages. One is that it makes the skin tough and blunts the knife; another that it increases oozing of blood from the skin; and another that you may get some desquamation.

But I have used this method for more than nine months, and have found it as reliable as it is simple and inexpensive.

November 28th, 1910.

Nasal Reflex Neuroses.—Killian states that nasal reflex neuroses are becoming more and more frequent in city dwellers, and that the whole subject needs revision, as treatment is successful only when applied strictly according to indications in the individual cases. These indications are determined by the sensitiveness of the nasal mucosa, and this he tests by the reaction to the contact of a piece of fine thread (No. 100), held in a slit cut in the end of a fine sound, so that the thread projects about 7 mm. from the tip of the sound. Drawing this thread along the forehead, cheek, outside or inside of the nose for about 3 mm. causes a tickling sensation. With normal sensibility tickling does not induce any reflex action, but with an ultra-sensitive mucosa the tickling is followed by sneezing, lacrimation, etc. Certain points in the nose are more sensitive than others, especially the processes on the septum and points in front just above the anterior end of the inferior turbinated bones. The reflex action occurs more easily, as the mucosa grows more sensitive under the prolonged influence of irritating dust, etc. By the thread test it is possible to localise the areas of ultra-sensitive mucosa and put an end to the neurosis emanating therefrom by cauterising the nerve terminals or slicing off the mucosa. It is important to determine the special nerve involved in the morbid area. E. Jonge has been successful in curing the anterior type of nasal neurosis by resecting the internal branch of the nasal nerve.—*Journ. A. M. A.*, vol. lv, No. 20.

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** Specially reported for the Clinical Journal. Revised by the Author.*

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TWO CLINICAL LECTURES ON TYPHOID FEVER.

Delivered at Guy's Hospital.

By W. HALE WHITE,
Senior Physician and Lecturer on Medicine to the Hospital.

LECTURE II.

THE DIAGNOSIS OF TYPHOID FEVER.

GENTLEMEN,—Last time, you will remember, we went over some of the points in the treatment of typhoid fever, and on thinking of a subject on which to talk to you to-day, it came to my mind that it would be useful to go over the diagnosis of this disease. We have several cases of the condition in the ward which make us think about it at present, cases which are mild in degree, and it is the mild cases which, generally speaking, are difficult of diagnosis. It is the insidiousness of typhoid fever which makes this difficulty. Often for a fortnight before you are able to make a certain diagnosis the patient feels ill, and nothing more definite can be said about him. For many years the difficulty has been great, but from different causes at different periods. Up to the time when Sir William Jenner wrote his paper in which he clearly differentiated typhoid fever from typhus, the two diseases were confused, and as you probably know, the Germans have no word for typhoid fever even up to the present day; they call it "abdominal typhus." But that difficulty does not present itself often nowadays, for the simple reason that there is very little typhus fever. I think now that the distinction between the two has been pointed out; in nearly every case everyone is able to notice the differences. But a few years ago there were three young adults in the wards, all under three different physicians, and they had all come from the same house. As a matter of fact they all three

had typhoid fever, but two of them who were under separate members of the staff were said to have typhus, while on the other hand the third case was said to have typhoid. It soon became known that they were all from the same house, and so the various members of the staff looked at each other's cases, and there was no doubt that they were all three cases of typhoid fever, with such a very profuse rash and the children so ill that it was pardonable to confuse them with typhus.

There is a child now in No. 19 bed in Miriam Ward in whom it is very difficult to say what has been the matter with her. For the first week we observed her we thought that probably she had typhoid, but further observation has shown that she was not suffering from either typhoid or paratyphoid fever, and all we can say is that she has had some febrile illness. So even now and for a very long time there has been confusion between typhoid fever in its early stages and any other case of illness in which there is a rise of temperature. If you had been students here thirty years ago you would have got up a stock question as to the distinction between tuberculous meningitis and typhoid fever in children. And the reason that was a stock question was that many patients with typhoid fever have nerve symptoms, and many children with typhoid fever have not any spots. The difficulty of diagnosis was one which was very constantly recurring and still does recur. Only, thanks to the agglutination test, we get the diagnosis right sooner or later in most cases. And that reminds me to say that when we talk about the difficulties of diagnosis in typhoid fever, you must not think that there are many cases which go down to the post-mortem room undiagnosed. Dr. Hilton Fagge, looking up the hospital cases when he wrote his article thirty years ago, remarked that the diagnosis was usually correctly made before the patient was well or before the patient died, as the case might be, for the difficulty is in the early stages. One difficulty that there is at the present day is, that since we have cleared away chiefly by means of the agglutination test, some of these difficulties which I have mentioned, the very agglutination test itself has brought forward another difficulty, for it is now found that there is another organism, the para-typhoid bacillus, closely allied to the typhoid bacillus, but which is certainly not the same bacillus, which will cause a fever which is clinically, as I shall show you before the lecture is over, indistinguishable from typhoid fever. So that this very advantage to our knowledge, while it has cleared away some difficulties, has, as is often found to be the case, added a further difficulty in the diagnosis. The chief difficulty which comes before you is, as I have said, in the early stages of the disease. You see a patient who is feverish and has pain all over him, and for the first few days it is impossible to say from which of the specific fevers he is suffering. Occasionally the pains locate themselves very much about the joints, and you find these cases described as those of mild rheumatic fever, and that is a difficulty common to all specific fevers—that in the early stages the pain may lead you to think that the patient is suffering from rheumatic fever. You know that one of the earliest symptoms of typhoid fever is the headache. It is present in about 75 to 80 per cent. of the cases. And it is the persistence of the headache that often leads you to think that the case is really one of some cerebral disease. The headache of typhoid fever may be anywhere in the head. As a rule it is not of the excruciating variety, which makes the patient shriek with the pain as the child with meningitis in John Ward shrieks with pain at the present moment. In a bad case of typhoidal headache it is an intense headache, but of a dull character, making you feel you must hold your head and that you cannot go about with a headache of that character and intensity. Sometimes when the temperature is not much raised and the headache is a predominant feature many mistakes may be made. As I told you last time, I have had typhoid fever, and I went to a man whose opinion I respected, and still respect, very much, but he did not recognise that my headache was due to typhoid fever. I was in residence in a hospital (not this) at the time, and as we were having a heavy time at the hospital he thought that probably I was run down and that I wanted a week's holiday. As a matter of fact I wanted a good deal more than that, because it came to three months' holiday before I was well of the typhoid fever, of which this intense headache was the earliest symptom. This headache usually persists about a week; it invariably goes sooner or later. The longest time I have ever known it to last was seventeen days, but that is quite an exceptional length of time. It is nearly always gone by the tenth day. That is a point which often makes the

differentiation between typhoid fever and some cerebral condition fairly certain. Usually as the cerebral condition gets worse the headache increases in severity, whereas the headache of typhoid fever invariably goes. So it works out that if the patient is going to be delirious, as many patients with typhoid fever are, he will have a headache, say, for ten days or a fortnight, and then by that time, the typhoid having reached its acme, the headache will be followed by delirium. But, on the other hand, if he is suffering from meningitis, for instance, the headache will, at least in some cases, co-exist with the delirium, though in the case of typhoid fever it very rarely does. I mentioned, when we talked of treatment of typhoid fever, that probably the best drug with which to treat the headache of typhoid fever is bromide of potassium. When you are trying early in the case to tell whether a headache is due to a cerebral condition or to typhoid fever you will, of course, look at the optic discs. But unfortunately optic neuritis may be, even if it is going to be present, in the meningitis a late symptom. But if you will look systematically at the optic discs of all the patients with typhoid fever, you will often find some haziness at the edge of the disc due to the febrile condition. Therefore you see you might get a disc in which you would find it difficult to say whether it was due to commencing optic neuritis indicative of meningitis, or whether it was just a blurring of the disc which might be associated with typhoid fever. But that difficulty would not last more than a day or two, because the slight blurring of the edge of the disc which you occasionally see in association with typhoid fever does not increase. On the other hand, if due to meningitis it soon passes into a severe degree of optic neuritis. Then carefully look also at the abdomen. We have had a striking instance in John Ward lately showing the importance of looking at the abdomen in cases of meningitis. You know that in typhoid fever the abdomen, if it is altered, is distended. In meningitis, if it is altered, and it nearly always is altered, it is retracted. So that that also would help you very much in diagnosis. Another point is this: I mentioned that the headache of typhoid was a dull, heavy headache. Of course there is no rule without an exception, and I have known patients scream with typhoid fever headache on account of the acute pain. But as a rule it is a dull, heavy headache, while on the other hand many patients with meningitis scream as the boy in John Ward screams now with his meningitis headache. Then, too, patients with typhoid fever sometimes pass into a stage, as I mentioned just now, of delirium, sometimes wild delirium, sometimes of the low, muttering type, constituting the "typhoid state," which means a condition of low, muttering delirium, which typhus fever patients commonly get into, and which patients suffering from other specific fevers rarely get. "Typhoid" applied to this state is an adjective, from the word "typhus." As a matter of fact the "typhoid state" is not very common in typhoid fever, and it is very common in typhus. But it does occur in typhoid fever. By the time the later stages have been reached the diagnosis of typhoid fever will almost certainly have been made by the spots or by the agglutination test, so you will at this stage have no difficulty in deciding whether it is typhoid fever or meningitis. The difficulty of telling typhoid fever from cerebral conditions occurs chiefly in children, and in order that you might not think that I personally exaggerate the difficulty, I thought I would tell you what Sir Samuel Wilks said in his lectures. He talks of tubercular meningitis, and he says: "In the first place the child may be fretful, listless, have stomach disturbance and other troubles which bear no especial character. Afterwards feverish symptoms marked by a hot skin, quick pulse, furred tongue, and headache. At this period the case is generally—I might say always—styled one of typhoid or gastric fever. It is not the exception but it is the rule to make a mistake." So bear in mind that in children it may be very difficult in the early stages to distinguish between typhoid fever and meningitis, and the difficulty is enhanced by the fact that spots are often absent in children when they have typhoid fever. Later on, for instance, Sir Samuel Wilks repeats himself and says: "At the commencement of tubercular meningitis the head symptoms are not the more marked than those of typhoid, and a positive diagnosis cannot be made." That enables me to mention a sign which we have paid attention to here, because the late Mr. Stocker, who had remarkable acumen for telling offhand at first glance what was the matter with people, laid much stress on it. He pointed out that in the case of the meningitis patient, if you went to turn down the bedclothes to examine him he resented it very much. Go to the bedside of a

child with meningitis to examine him, and he will curl himself up and pull the bedclothes over himself. The typhoid fever patient, on the other hand, does not usually mind the examination at all. Again, very rarely, you may have great difficulty in distinguishing typhoid fever from hysteria. There is no better collection of cases of typhoid than that given in the 'Johns Hopkins Hospital Bulletin,' cases recorded by the physicians there, mostly Professor Osler's cases. Under "Error in Diagnosis" he says: In two very interesting cases the mental condition at the onset of the disease led to a mistake. We saw a young girl, æt. 13 years, who had been treated at the outside for ten days for hysteria, and certainly, according to the mother's account, the symptoms which she presented were quite typical of that disease, and amongst others there had been anomalous acts and laughing and crying. When she was admitted into the hospital it was found she had typhoid fever. The second case was a case in which Professor Osler was in error, a young woman, æt. 28 years. When seen in the admitting room she was completely off her head, so much so that Professor Osler did not think the case suitable for admission. He thought it was one of mental disease. But her temperature was found to be slightly elevated, so the patient was taken in. She turned out to be a case of typhoid fever. So even early there may be functional mental troubles which will lead you astray. Thus you have two kinds of difficulties in distinguishing nervous disease from typhoid, organic disease, chiefly meningitis, and occasionally functional disease.

Another difficulty is the pyrexia. Of course pyrexia may be due to almost any specific fever, and I do not really know, in the early stages, any way of being certain that you are right in saying that pyrexia is due to typhoid fever. The headache and other signs may give you a hint, but in some cases it is not possible to be certain. The agglutination test has made matters still more difficult, because it is clearly shown, I think, that many cases of slight fever, which would have been called, for want of a better name, febricula before the agglutination test became known, are really typhoid fever. You can only wait until the spots appear, or you get the definite agglutination test positive, before you come to a diagnosis.

The tongue we have often talked about in the wards. There are few things more characteristic than the tongue which is free of fur at the edge and somewhat free in the middle, and has a parallel line of fur down the middle of each side of the tongue. It is one of the commonest symptoms of typhoid fever, and it is of great help in the diagnosis. But you sometimes meet with cases in which you cannot say that the tongue is characteristic of the disease, and yet the development of the spots or the agglutination test, being positive, shows that the patient has typhoid fever. Still, it is a symptom on which I want you to place a good deal of reliance in coming to a diagnosis.

Another early symptom of typhoid fever is, sometimes, epistaxis. For my own part, I fancy the books exaggerate the importance of it; they say that one patient in four has it, but I should rather say, upon my own experience, that it is more like one in eight. It is often an early symptom, and the patient with fever who also has headache and has nose-bleeding has probably typhoid fever. Some books tell you that the pulse may be slow throughout in enteric fever, and certainly I have sometimes seen it so, but very rarely; a slow pulse is presumptive evidence that you are not dealing with enteric fever, but that you are dealing with a case of meningitis or possibly with one of influenza.

Now to come to another difficulty. You know that bronchitis is a common complication of enteric fever. Sometimes it is an early complication, and several times in one's experience here people have been sent in with the diagnosis of bronchitis, when in reality their bronchitis was a complication of typhoid fever, the latter disease having been overlooked. I particularly want you to remember that difficulty, for it is a bad mistake to make, because, generally speaking, especially amongst these hospital patients who have to work for their living, they are ill disposed to lie up if they think all that is the matter with them is bronchitis. Therefore they do not lie up for the first week of their typhoid fever, and there are few things which render the outlook of typhoid fever worse than the patient being up and about while the fever is on him. Consequently be very careful to make sure that the bronchitis which you see in a patient is not a complication of typhoid fever. Here is a case in this Johns Hopkins series in which it was pleurisy and not bronchitis which put the physicians off their

diagnosis. It was the case of a women, æt. 19 years, who was admitted complaining of severe pain in the left side. For nearly a month she had not felt well. She would shiver, but get up and go about. She had some headache, had had a cough, and she had the morning before admission a very severe pain in the left side, which was made much worse by taking a deep breath. Her temperature was raised, and her cough was increased. When she was admitted into the hospital she had great pain in the left axilla, and there were the physical signs of pleurisy there, including a loud friction murmur. The abdomen was not distended, the spleen was not enlarged, it was not thought she had typhoid fever. The next day and the day after she was still regarded as a case of pleurisy. On the fourth day it was noticed that she looked more ill than would be expected in an ordinary case of pleurisy, and that caused the physicians there to look for typhoid spots, but they did not find any, and it was not until the fifth day after admission that they found some spots, and the diagnosis in that way became clear. So that both bronchitis and pleurisy may lead you astray.

I have already often mentioned the agglutination test. The test is that the serum of a patient suffering from typhoid fever will cause clumping of the typhoid bacilli. But many sera will do that, and it has been found by practical experience that there must be a time-limit during which the clumping must occur in order to make the diagnosis clear. You know that usually the patient's serum is diluted to 1 in 50, and the serum is mixed with the typhoid bacilli, and they are watched under the microscope, and if clumping occurs in half an hour it is considered that that is proof that the patient has typhoid fever. The very fact that it depends on the degree of dilution and on the time-limit prepares you for the statement that the test is not absolute. Because it is clear that the degree to which the blood-serum of typhoid fever patients will clump bacilli must vary, and if you dilute it down you may sometimes dilute it too much for that particular case. As a matter of fact it is wonderful that the test is as good as it is. It certainly does not hold universally. There are some cases in which clumping in half an hour does not take place, and yet post mortem the patient is found to have typhoid fever, and typhoid bacilli may be recovered from the spleen. But those cases are few. On the other hand, there are other diseases the serum of which will clump these bacilli if the serum is diluted to 1 in 50 and half an hour elapses. And that is not surprising, because if you did not dilute the serum at all the serum of almost any disease would clump typhoid bacilli. The best case in which the serum from a patient with another fever clumped typhoid bacilli that I have ever read occurred here. There was a woman admitted into Miriam Ward under me; it was doubtful what was the matter with her. The agglutination test was carried out. Her serum did not clump the typhoid bacilli. We watched her for a few days, and then it was clear she was suffering from malignant endocarditis. Again we carried out the test, and then her serum diluted 1 in 50 did clump the typhoid bacilli in half an hour. At the post-mortem examination no typhoid bacilli could be found in the blood or the spleen. So here was a case of malignant endocarditis giving a complete Widal reaction, and yet the patient had no enteric fever. The power of the serum of the patient with typhoid fever to clump typhoid bacilli persists in the serum for a long time after the typhoid has ceased; it often persists three or four years, and a case has been recorded in which it persisted as long as twenty-six years. So that if a patient came into the hospital, say, this evening with marked endocarditis, and I found her blood-serum agglutinated typhoid bacilli, I could not be quite certain that that was not due to the fact that she had had typhoid fever previously in her life. But in the case which I have quoted to you you will remember that on admission a Widal test was done and no agglutination occurred, so that previous typhoid fever could not be the explanation of the agglutination which finally occurred. To sum up, you may say the agglutination test will help you in about 90 per cent. of the cases, but there is remaining 10 per cent. of cases in which the typhoid patient fails to agglutinate, or in which some other disease will agglutinate the typhoid bacilli in the right dilution and the right time. But the point which greatly detracts from the practical value of the test is that it often cannot be demonstrated till the tenth or even fourteenth day, even when typhoid fever is present.

Now we come to another important point, and that is the spots. The presence of spots is certainly more definite in helping your diagnosis

than the presence of the Widal reaction. You know the typhoid fever spot when you see it, or I hope you do. It is usually of a pale pink colour, in size between half and a quarter the size of a split-pea, it is slightly raised, circular, you can press the blood out of it, and these spots are best seen on the abdomen and chest. And yet, with all that description, I have not mentioned the characteristic of it. The characteristic feature of these spots is that they come and go in crops. There is hardly any other exanthem, unless you mention syphilis, which does that. The rash in scarlet fever and measles does not come and go in crops, and it is because the typhoid fever rash comes and goes in crops that you put rings round the spots or else you would not know where they had been. Spots in typhoid fever may come out as early as the fifth day, but usually they make their appearance towards the end of the second week, unfortunately, because otherwise they would enable us to make the diagnosis much earlier. They may go on coming out in crops until the sixth week of the disease, long after the temperature has regained the normal level, but I admit that such an occurrence is exceptional. They usually come out in crops for something like ten days. Usually each spot persists three or four days at the outside. As I have mentioned, these spots are commonest on the chest and abdomen, but they may occur almost anywhere, and be very profuse over the body. I have even seen them on the hands and arms. And it is these profuse cases which render the diagnosis from typhus occasionally difficult. These spots may even be hæmorrhagic. Here are notes of a case of hæmorrhage spots which is in point. "There is a severe typhoid rash, copious on the abdomen and back, and just below the right clavicle are several ecchymotic spots. There is an area the size of a silver dollar into which superficial hæmorrhage has taken place. In this region nine or ten old ecchymotic spots are seen." But remember such a hæmorrhagic typhoid rash is very rare. But it is interesting in diagnosis, and it is interesting as showing the link between typhus fever rash and typhoid fever rash. Before I leave that point let me repeat that the essential feature of the rash is that it comes and goes in crops. It occurs in about 80 per cent. of the cases. Unfortunately, it is rarer in children than in adults, and it is children who present most of the difficulties in diagnosis. You have therefore 20 per cent. of the patients mostly children in whom we have to make a diagnosis without the spots. But you may ask, Do these typhoid fever spots occur in other diseases? Fortunately, practically, no. There is a well-known case which has been often quoted which occurred in Guy's, in which the spots were present in asssociation with acute miliary tuberculosis. That is, however, highly exceptional. It is one of the most remarkable things in medicine that if you see a rose spot on the abdomen, which any untrained observer would pay no attention to, you can say there will be ulcers in the intestine. And if outsiders are inclined to jeer at medicine, ask them if they can show anything to compare with that. It is one of the triumphs of observation in medicine that the presence of these little round spots on the abdomen should indicate that there is ulceration in the intestine. You see that inasmuch as the Widal reaction by the end of the first fortnight is present in about 90 per cent. of the cases, and the spots are present in 80 per cent., it will not be often that you will get a case in which neither the Widal reaction nor the spots are present. But still such do occur.

The enlargement of the spleen is a matter which many place a good deal of reliance upon, but it is rather fallacious. It is true that the spleen is enlarged in two thirds of the cases, but the difficulty is that, in the first place, in children especially, the spleen may be a little enlarged and yet be within almost normal limits of size. In the next place, it may be difficult to feel that a spleen is enlarged, it may be difficult to percuss it out. So that although it is an important point in diagnosis, considering that it is sometimes not greatly enlarged, and that in something like a quarter of the cases it is not enlarged at all, that when it is not greatly enlarged it may be difficult to detect that it is enlarged; the size of that organ is of less practical importance than you would think. And you have to remember also that in many other febrile conditions, *e. g.* pyæmia, the spleen may be enlarged, and it is sometimes difficult to distinguish enteric fever from various septic conditions.

Now we come to another interesting point, and that is rigors. They sometimes cause very great difficulty. In the first place the fact that a patient has had a rigor as an initial symptom is very strong

evidence indeed that the disease is not typhoid fever. But unfortunately, in a few exceptional cases there is no doubt whatever that typhoid fever may be ushered in by rigors. But still, as I say, those cases are not numerous. Out of seventy-nine cases at Baltimore there were thirteen in which rigors occurred, but in many of them it was a slight chill and not a severe rigor. But on the other hand, as showing how difficult the diagnosis may be, in some the rigors were numerous and severe. Even the patient may have a rigor at the oncoming of a relapse when there was no rigor in the original attack. For example, here is a case of a girl, æt. 22 years, who went through an ordinary attack of typhoid fever. She had no rigors at all, and she had an interval of twenty-three days without fever. On December 20th her relapse began and on the twenty-first she had her first rigor, and on the same day as the rigor her temperature was 106° F. She had a second rigor, and the day after that another rigor, and she had one more after that. She got quite well after going through an ordinary relapse. Although in rare cases these rigors may be numerous in the disease, and severe too, yet the presence of a definite rigor at the onset of the feverish illness is strongly in favour of the disease not being typhoid fever. In old days antipyretic drugs were given for typhoid fever and they sometimes caused rigors, but that is not of importance now. The only other rigors which are of importance in typhoid fever are those which indicate complications, and nearly always if in the course of the disease there are rigors, you may be certain there is some complication present. In my own experience the most frequent complication to cause a rigor is thrombosis in a vein, and the most frequent vein to be so affected is the saphena vein. But all the following conditions have been known to cause rigors in typhoid fever: pneumonia, pleurisy, acute otitis media, suppuration of mesenteric veins, which sometimes occurs after ulceration of the intestine, acute periostitis, and I have mentioned thrombosis of the veins. So whenever there is a rigor in the course of the disease, look out for and search carefully for some complications. It is the presence of rigors which sometimes renders the difficulty of diagnosis between typhoid fever and malignant endocarditis great.

The difficulty has occurred, chiefly in children, of mistaking tubercular peritonitis for typhoid. In both those conditions there is fever, in both the abdomen is tumid, there may be an imperfect history from the patient's mother, and for the first week or so there may be doubt. But sooner or later if it is enteric fever you will be able to see the spots and find the agglutination test positive. Before the introduction of the Widal reaction we were in great difficulty with those cases of typhoid fever which had no spots, that is to say, the 20 per cent. which I have already alluded to, and which went on for a long time. We could not tell whether they were cases of generalised tuberculosis without physical signs, nor whether they were cases of malignant endocarditis, because the murmurs may have been hæmic murmurs associated with enteric fever. It is not likely you will get a case of difficulty with no spots and no Widal reaction. But if such does occur, the old difficulty of distinguishing between generalised tuberculosis, malignant endocarditis, and typhoid will re-assert itself. In tropical countries malaria of various kinds caused great difficulty until the malarial organism was found in the blood. All the world over it was the commonest difficulty in diagnosis from typhoid fever. But with the Widal reaction on the one hand, and the examination of the blood for the malarial parasite on the other, those difficulties do not occur now so often as they did.

I need hardly mention that you must look carefully for obscure pyæmia. Has the patient some salpingitis? Has she pus in the nose, pus in the antrum, pus in the ear which is keeping up this temperature, showing that the case is not typhoid fever?

Having run over those various difficulties to you quickly, I have now only to talk to you a little about para-typhoid fever. The difficulty is that para-typhoid fever may be, clinically, quite impossible to tell from typhoid fever. A patient suffering from an infection by the para-typhoid bacillus has an irregular temperature, often has rose spots on the abdomen, and has the characteristic tongue, the characteristic headache, in fact, every symptom of typhoid, and, post-mortem, you may find the lymphatic structures of the ileum affected, including Peyer's patches, and it has been shown that just as typhoid is most frequently conveyed by drinking water, so is para-typhoid conveyed by it. The only clinical and pathological points to direct your attention to in diagnosis—and none of them are

of great importance—is that para-typhoid fever is shorter in duration, the spots come out earlier, and the patient is not so ill as he is if he has typhoid fever. But of course that is of no use in diagnosing between a mild case of typhoid fever and an ordinary case of para-typhoid. Very few cases of para-typhoid fever die, because it is a less severe disease. When seen in the post-mortem room the Peyer's patches have not usually proceeded to the degree of ulceration which you find in ordinary typhoid, but that, again, does not help you, because the case may be one of early typhoid fever. The only help in differentiation is the bacteriological one, and that, again, is not conclusive, unfortunately. You know the serum diluted 1 in 50 from a patient suffering from typhoid fever will not clump the para-typhoid bacilli in half an hour. On the other hand, it will clump the typhoid bacilli. If the patient, on the other hand, has para-typhoid, the serum will clump para-typhoid bacilli but will not clump typhoid. All that seems plain and straightforward sailing. When you have done that you have done all you can. But there is no doubt that there are a few cases in which the patients have been suffering from para-typhoid fever, as has been shown by the recovery of the para-typhoid organism from the blood and from the spleen, and yet, nevertheless, that serum has clumped typhoid bacilli, and *vice versâ*. Happily such cases are rare, but in the rare cases in which the agglutination test does not help you the only way to settle the matter is to cultivate the para-typhoid or typhoid organism from the patient's blood or from the patient's fæces. A practical point which comes out from this is, that when you are dealing with a case in which you are not quite sure that it is typhoid fever, and you very rarely can be sure, you ought, as a matter of habit, to ask the bacteriologist to do an agglutination reaction, not only for typhoid, but for para-typhoid fever.

December 5th, 1910.

We are asked to announce that in consequence of the general election the Le Touquet Golfing Tournament for the medical and other professions has been postponed.

A CLINICAL LECTURE

ON

ATELEIOSIS (HASTINGS GILFORD'S FORM OF ESSENTIAL INFANTILISM).

Delivered at University College Hospital.

By H. BATTY SHAW, M.D., F.R.C.P.,
Physician to the Hospital, and Assistant Physician, Hospital for Consumption and Diseases of the Chest, Brompton.

In giving a lecture on this subject some preliminary remark must be made, and it is this—that though nothing can be done for its cure, it is necessary that the condition should be known, so that when this form of infantilism is met with it may be recognised and dealt with as far as is possible. The term "ateleiosis" was introduced by Mr. Hastings Gilford to indicate a condition of imperfection of development and of growth, the literal meaning of the word being "not arriving at perfection." The information to be brought before you in this lecture is entirely owing to his close studies of the subject, and the interesting example of a typical case, as well as many most entertaining pictures of this and allied conditions, you owe to the same source, and it is my duty and pleasure to thank him for his generous help in the matter.

Dwarfism.—There is no hard and fast limit of height which may be used to separate normal individuals from dwarfs on the one hand and giants on the other. It is a common experience to meet with individuals who, though normal in other particulars, are below the average height of their ages, and occasionally, though more rarely, those are met with who are very much below the average stature. They may be described as dwarfs or dwarfish people; sometimes whole races are met with, such as the "pigmy" races of Africa, New Guinea, and other parts of the world. Such individuals may be normal in every other way—physically, mentally, and sexually; they are mannikins or diminutive men and women, or better, so it seems to me, they are dwarfs or dwarfish people; there is no evidence that they are pathological results.

Infantilism.—There are, however, other conditions, in which an individual may be undersized and undeveloped owing to some abnormal influence.

Unusual features may be present, any one of which stamps the individual as quite different from a normal one of ordinary physical, mental, or sexual development. Such individual may be of small stature, but in addition these abnormal features are present, which are shown in some irregularity, as in manner, facies, etc. Or the particular individual may be of normal height and yet be possessed of the same unusual characters, which show that there is something amiss in development. The generic name of infantilism has been given to such individuals, and though the classification of Mr. Hastings Gilford may not agree with that of other observers, more especially continental ones, it is so practical and based on such a large amount of careful investigation that it may be recommended to you.

In order to give you clear ideas of the types of infantilism and their relation to one another, I describe to you, thanks to a recent private communication received from Mr. Hastings Gilford, a grouping which he has drawn up. Infantilism may be divided into two groups of cases.

(1) *Symptomatic infantilism.*—This includes individuals whose condition is secondary to, or part of, some definite pathological abnormality, which is familiar to all students of medicine. It is an acquired condition and is never transmitted to offspring. The features of this condition are very varied, as well they may be, seeing that they depend upon entirely different causes.

(a) *Achondroplasia dystrophia, or micromelia.*—It is not uncommon to meet in the London streets men and women whose "point of view" is, by reason of their height, placed at a much lower level than that of an ordinary adult; they may be cases of achondroplasia. The reason is that, though the head and body development is much like that of other adults of the same age, their legs are much shorter, even only half the proper length, and the change is largely due to shortness of the femora; even the arms are short, from defective development of the humerus in main part. Though rickets may be present in infancy, it is not the cause of this form of infantilism. You will see the early stages of this malady in the photograph shown of an infant suffering from it, who was recently a patient in the children's ward, and attracted much attention by the great growth of hair upon the head. So characteristic are the defects in the limbs that achondroplasia should not be confused with ateleiosis, which is a form of essential infantilism.

(b) *Rickets.*—This condition, unhappily so common amongst the London poor, may cause considerable shortness of stature owing to the curvature and deformity of all the long bones, but especially those of the lower extremities. There is not any want of development mentally or sexually, and there are the characteristic signs of deformity of the chest and head and a history of defective alimentation in infancy.

(c) *Mongol or Kalmuc children.*—This is another condition in which there is a certain amount of infantilism. As the examples to be shown to you will exhibit, the facies is so characteristic, recalling as it does the Mongolian type of features, viz. the downward slope of the palpebral fissures towards the nose and the encanthus; there is often a flattened bridge to the nose, which is *retroussé*, protruding tongue, often nystagmus, and a smallish spherical head. There are frequently deformed ears, deformed little fingers, and the sexual organs may be malformed or ill-developed. Mentally these children are imbecile, but notwithstanding this they are very vivacious, good-tempered, and affectionate. They rarely reach adult life.

(d) *Cretins.*—When a child is born wanting in thyroid gland, the features of cretinism develop after weaning, and though the child may reach adult life it is obvious, from the case shown, that full stature is not reached; cretins are permanently stunted people. They never reach the mentality of an ordinary adult, however carefully they have been treated with thyroid gland, and they retain some of the features which are characteristic of the untreated condition; they may be dull, and the mouth is often coarse and the lips held open; the limbs are short and stout and the hands broad. They are not at all vivacious nor affectionate, in contra-distinction to the Mongolian child.

(e) *Syphilis (congenital).*—It is well known that a child the subject of congenital or inherited syphilis may be of small stature. The diagnosis depends on the history and on the presence of syphilitic stigmata, such as scars at the angles of the mouth, old interstitial keratitis, the presence of nodes, and of the Hutchinsonian type of teeth. Those affected with syphilitic infantilism may be

mentally defective or may present no abnormality in this direction, and sexually they may be normal.

(f) *Congenital heart disease.*—This brings us to a group of infantilism in which bodily growth is defective and dependent upon primary disorders other than those so far mentioned. It is common for a case of congenital heart disease to be defective in development, and the diagnosis in many cases, though not in all, is shown by the cyanosis and polycythæmia, by clubbing of the fingers, and by the presence of a cardiac lesion.

(g) Mr. Hastings Gilford speaks of an infantilism which is one of *correlation*, and quotes as examples that abnormality of development which is occasionally met with in kyphosis, microcephaly and hypertrophic or biliary cirrhosis of the liver. Still another condition in which infantilism may be correlated with another condition is splenomegaly. Only quite recently was the knowledge gained by a perusal of Mr. Hastings Gilford's writings of use to me in a case of considerable rarity, viz. splenomegaly combined with jaundice (acholic) in whom there were periodic attacks of fever. This young man, of about thirty years of age, was small of stature, though he belonged to a family of well-developed individuals. He was an able business man, and but for his defective stature was fully developed. The awkward question was raised whether, in view of his life-long jaundice and splenomegaly, he should be allowed to marry. Was it likely he would transmit his imperfections to his offspring? The answer given was, that so far as the fear was concerned of transmitting his defective growth to offspring there need be little hesitation, and he might marry, but with regard to his jaundice and splenomegaly, medical knowledge was so scanty as to its transmissibility that a clear answer could not be given.

Another form of infantilism of the symptomatic variety mentioned by Mr. Hastings Gilford is that associated with intoxication dependent upon the use of wine or tobacco or upon the poison of rheumatic, scarlet, or typhoid fever, or upon lead poisoning, and he also refers to the so-called "pancreatic" infantilism and to infantilism dependent upon poisoning from intestinal decomposition—Herter's "intestinal" infantilism. One case which will now be shown to you may well be an example of this latter condition.

Charlotte H—, æt. 18 years, height 4 ft. 4½ in. (average, 5 ft. 1·95 in.), weight 4 st. 1½ lb. (average, 8·2 st.), belongs to a family of normal height and weight. Skiagrams show that the bony union of the pubis and ischium with the ilium, like a normal individual's, is not complete; but that the union of the great trochanter with the femur, unlike a normal person's at this age, is also incomplete. The patient is clever at needlework, which she carries out at home, earning four shillings weekly. She refuses to "go to business" with her sisters, preferring to work at home. The delay of growth was first noticed at the third year of life. The expression of face and the manner of the patient are those of her years, but not of her stature. She has never menstruated, and there is no trace of sexual development such as occurs at puberty. The mother had one miscarriage after the birth of this child. From the age of three years the patient suffered for some years with a swollen abdomen and very offensive motions.

In the absence of any other explanation, it seems tempting to advance the hypothesis that her deficiencies may be attributed to intestinal disorder in infancy. Some years ago there was another case in the hospital which recalls this possibility in which the process was in actual development. An undersized child was admitted to the Children's Ward suffering from Hirschsprung's disease—that is, from dilated colon; she also had tetany, which is met with in various disorders of the intestinal tract.

Another case will be shown to you to-day which points also to the possibility of intestinal disturbance being responsible for the defective growth; in this case such disturbance was expressed by a life-long constipation.

Miriam D—, æt. 27 years, height 4 ft. 6 in. (average, 5 ft. 2 in.), also belongs to a family the members of which are of normal height and weight. Skiagrams show that union of the epiphyses of the femur and tibia is not yet complete; at her age they should be. She helps at home, but cannot do needlework. She made no progress at school, but has, since her school years, taught herself to read and write at home; she is unable to do responsible work, and has a bad memory for recent events. The teeth are defective and the palate arched. She was a "six-months" child, and at the age of one year was smaller than her newly born sister. The mother has had no mis-

carriages. As in the last case, her facial expression and manner generally are more those of a normal individual of her age than of a child of her height. She is not without a fair amount of intelligence, and enters freely into any discussion at home. She has always been very constipated, and up to ten years of age slept eighteen hours out of the twenty-four daily; even now she sleeps in the daytime a good deal. She began to menstruate at the age of eighteen, but is not very regular. Though the breasts are not much developed, there is the usual development of hair met with at puberty.

(2) We are in the position now to consider Mr. Hastings Gilford's second form of infantilism, *i.e.* the essential or symptomatic infantilism which is distinguished from the first variety "by its pronounced degree, by its seemingly spontaneous appearance, and by the occurrence," in some cases at least, "of transmission to offspring."

This group is divided into two subdivisions—(*a*) progeria, and (*b*) ateleiosis.

The first subdivision must be dismissed briefly, mainly because I am unable to show you what it means by a living example, though the pictures presented in Mr. Hastings Gilford's paper will give a most graphic representation of the condition. In progeria the infantilism is mingled with premature senility (senilism). The stature is small, there is baldness, emaciation, arterio-sclerosis, and general decrepitude.

(b) *Ateleiosis.*—At last we reach the subject of this lecture. It is a form of essential or cryptogenetic or idiopathic infantilism, and most deservedly should be also designated as "Hastings Gilford's form of essential infantilism." "It may begin at any age of progressive development, and its characters are for the most part those normal to the age of its first appearance. It usually begins in infancy or early childhood, and the size, proportions, and physiognomy of this time of life are perpetuated. It is prone to be associated with cryptorchism or with some corresponding ill-development of the ovaries causing divergence into two varieties — sexual and asexual. In asexual ateleiosis all the physical characters of infantile life are stereotyped, but in sexual ateleiosis, though the physiognomy and proportions remain infantile or childish, the onset of puberty (often greatly delayed) brings with it some accession of growth and the addition of the primary and secondary sex characters of the adult." (Private communication from Mr. Hastings Gilford.)

I was prompted to give this lecture because I believed that I had discovered amongst my out-patients, two years ago, an example of this form of ateleiosis, but to my dismay I found that the little patient had died from tuberculosis at St. Bartholomew's Hospital, Rochester, under the care of Dr. Henry Smith, in February of this year. Owing to the generosity of Mr. Hastings Gilford this catastrophe, so far as this lecture is concerned, is remedied by his courtesy in sending one of his cases for you to see, as well as lending quite a little gallery of pictures of other cases and allied conditions.

Mr. T. L. S— (now æt. 36 years) is an example of the asexual variety of ateleiotic infantilism, a form of the condition the separation of which is entirely due to researches of our generous friend, Mr. Hastings Gilford, from whose description I take the following facts: The rest of the family are normal. He was of average size when born and cut his teeth at the usual age. At the age of two years, before he had had any sort of illness, he was noticed not to be growing. He left school at sixteen, having reached the sixth standard. In 1902 he was 3 ft. $7\frac{1}{8}$ in. high, and his weight was 4 st. 1 lb. (now, November 9th, 1910, he is 3 ft. $8\frac{3}{4}$ in. high and weighs 4 st. 11 lb. 1 oz.) There are no signs of syphilis. He is a child in many respects of about six or seven years of age, the head being large compared with the body, the muscles of the limbs and body being hidden by fat. The face is broad and the cheeks full, and the bridge of the nose is somewhat sunken. There is no sexual hair on face or body. His voice is more treble than that of an adult of the same age. The laryngeal cartilages are small, like that of a child. Despite the signs of immaturity, there are marks of age on his face and an adult assurance of manner. His mental development and tastes are somewhat childish, but he does not care to associate with children. He is fond of reading, and earns three shillings a week as an under-gardener. (He is at present, December, 1910, employed as a compositor earning nine shillings a week, and has been engaged in this work for the last five and a half years.) The external genitalia are of the size and appearance of a child of three years of age; the testicles are un-

descended; the breasts are not developed. A radiogram taken four years ago showed ossification of a boy of about the age of ten years, and one taken at the present date shows, as you will see, that the epiphyses of the hand and wrist are not united to the corresponding diaphyses.

One cannot close this lecture without a short reference to small people who were of such interest to our parents and grandparents. I refer to the interesting individuals known as Tom Thumb (Charles Stratton) and his wife Lavinia Warren, who had a child of normal development, which, however, died early of meningitis. Tom Thumb died, æt. 41 years. Lavinia's sister, Minnie, married the celebrated "Commodore" George Washington Nutt, who also was one of this type, and who died, æt. 33 years. Mr. Hastings Gilford groups these cases amongst the class of sexual ateleiotics, and he has sent us a photograph of a sexual ateleiotic who begat a family of children who, except one who was an asexual ateleiotic, were normal.

Reverting to my opening remarks, I trust you will be convinced that you will in future, like myself, have a much keener interest in people who are of small stature, and the knowledge gained will be of practical use, if not altogether from the therapeutical point of view, at any rate from the point of view of diagnosis. So far as my reading and judgment go, I think I may close my lecture with the justifiable remark that Mr. Hastings Gilford's enthusiasm and clinical acuity have fixed for all time a form of infantilism which is as remarkable as it is difficult to explain.

December 5th, 1910.

Lordotic Albuminuria.—Pechowitsch shows that this clinical variety of orthostatic albuminuria, which becomes less in extent when the patient creeps on the floor, is due entirely to lordosis in the lumbar region, which furnishes a mechanical obstacle to the passage of blood through the renal veins. The creeping exercises, as developed by Klapp, are not only of diagnostic but therapeutic value. The children who suffer from this condition are of a type predisposed to tuberculosis and are asthenics in general.—*Medical Record*, vol. lxxviii, No. 21.

THE X-RAY EXAMINATION OF THE DIGESTIVE SYSTEM.*

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GENTLEMEN,—In this lecture I propose to indicate to you in a general way how the intelligent use of the Röntgen rays can give very valuable assistance in the diagnosis of disorders of the digestive system.

The first difficulty that confronts us is the fact that all those organs that make up this system are translucent in health equally with the rest of the soft structures adjacent. This makes it necessary to do something to enable us to distinguish them from the others.

The method first employed was to inflate them with air or gas and so make the stomach and intestines more transparent than the surrounding parts. This was only partially successful and failed in several important respects. It was uncomfortable and even dangerous in many cases, besides distorting the usual form and size of the part under examination.

The usual practice at the present time is to give the patient some inert, or at least innocuous substance in food that has the property of making the latter opaque to the X-rays. For this purpose nothing answers so well as bismuth. The subnitrate, however, must not be used, since poisoning has followed its use in the large doses that are necessary for our purpose. Theoretically the oxychloride is preferable and was first suggested by Hertz, but the carbonate seems to answer equally well. They should be pure so as to avoid any risk of poisoning.

The bismuth meal generally employed consists of ten ounces of porridge or bread and milk, in which two ounces of the bismuth salt are thoroughly mixed. This may be sweetened or flavoured in any suitable way.

Under some circumstances the bismuth is better

* A lecture delivered at the West London Post-Graduate College.

given in other media, and these will be indicated when dealing with special disorders.

The examination with the fluorescent screen is the most important method; radiographs are not necessary except to record some very special conditions. Usually it is sufficient to make a tracing on paper of the shadows observed, and these are kept for future reference.

To do this work satisfactorily some form of screening stand is quite essential, and it is also necessary that the apparatus be sufficiently powerful to give a good illumination of the screen with a tube that is not too hard.

The patient should always be examined in the upright position, and adequate precautions must be taken to protect the patient as well as the observer from a too free exposure to the X rays.

I have found it very useful to control the primary current with a small switch on the end of a long, flexible cord, which is held in the hand. By turning on the current for only a few seconds at a time we can see all that is required, the tube is saved from overheating, and both patient and operator subjected to a minimum exposure to the rays.

It is advisable to be in the dark for at least ten minutes before beginning the examination, and to make the latter as brief as possible.

Œsophagus.

It is in examining this, the first part of the digestive tube, that we have to make some modification in the consistency of the bismuth mass.

The patient is turned to an oblique position so that the clear space between the spine and the heart is easily seen. The best position is when the tube is in front and to the right.

In a case of suspected stricture we begin by giving from ¼ to ½ ounce of bismuth suspended in mucilage. Such an amount is easily seen in an average individual, and as those suffering from œsophageal stricture are usually more or less emaciated there is usually not much difficulty.

We may here consider what takes place under normal conditions, but it is well to remember that there is no constant rule and appearances vary in normal individuals.

If we give a solid bismuth mass, such as stiff bismuth jelly, it is seen to lodge for one or two seconds at about the level of the arch of the aorta, then passing rapidly into the stomach. It practically never stops at the cardiac opening, except when there is some narrowing at this point. With a semi-liquid mass a large mouthful drops quickly down to the lower third, and then passes slowly into the stomach, sometimes taking from five to seven seconds. During the period of waiting the mass can sometimes be seen to pulsate in synchronism with the heart, and if there is any stenosis there will be more or less active peristalsis of the œsophagus.

Now if we give the mouthful of bismuth mucilage and see it pass rapidly to the lower end of the œsophagus, we can be tolerably sure that there is no stricture present, or if there is, it can only be a wide one. A large mouthful of the bismuth meal may now be given and its progress carefully watched. Even this may pass a wide stricture so rapidly as to escape detection, but a medium stricture will in all probability bring about a check in its downward career, and as it leaves this point it will be seen to be reduced in diameter, and if the narrowing of the tube is considerable, drops of the bismuth will be seen to detach themselves from time to time and pass rapidly on to the stomach.

In the case of a narrow or nearly complete stricture even the mucilage will be arrested more or less completely according to the state of the constriction, and the œsophagus above fills and shows as an oval well-defined shadow, the lower end of which indicates the position of the stricture. But supposing that the fluid and semi-solid mixtures show no evidence of constriction we must not jump to the conclusion that there is no narrowing of the tube. The next procedure is to administer a stiff gelatine suppository containing from 25 per cent. to 50 per cent. of bismuth. Theoretically these should be of the same diameter as the œsophagus, but in practice those made in an ordinary rectal suppository mould of large size answer every purpose. This keeps its shape and size for some time and will be arrested at the seat of any and every stricture that narrows the tube sufficiently to give rise to any symptoms of obstruction.

Of course, œsophageal obstruction may arise from other causes than that of stricture. It may be caused by pressure from the mediastinum, enlarged glands, malignant disease, abscess, pericardial effusion, aneurysm, and so on; and by one or other of the above methods the presence and the position of the obstruction can be made out.

The appearances in a case of narrow or almost complete stricture at the cardiac end are very striking. If two or three mouthfuls of bismuth mixture are given peristalsis of the œsophagus above can be seen; this is, of course, the effort to force the food through to the stomach. As the contraction passes off the shadow of the food rises up in the œsophagus, and if enough has been given it may go as high as the sterno-clavicular articulation. When trying this experiment it is as well to have a basin handy for the use of the patient in case of emergency.

Thurstan Holland has shown how a foreign body such as a vulcanite tooth-plate, which is translucent to the rays, may be shown. When bismuth mucilage is given some of it sticks to the foreign body and its position is thus demonstrated.

It will be seen that the X-ray appearances can afford a large amount of valuable information, and it also indicates the best consistency of food that can be given in any particular case. We, of course, want to give the thickest mixture that can pass through readily and without causing irritation.

Let me conclude this part of my lecture by offering you what is, in my opinion, a piece of sound practical advice.

If a patient comes to you with a complaint of dysphagia, never treat it lightly or pass it off as neurotic. It may be the beginning of a most serious condition, and its non-recognition at the early stage will not do any person any good. In the X-ray method of examination you have a most valuable help at your hand, and one that gives the least amount of discomfort to the patient.

Stomach.

In examining the stomach the patient always stands, or, at least, the trunk is in the upright position. A coin or similar metallic object is secured to the umbilicus with rubber plaster, and it is as well to indicate the situation of the xiphoid cartilage in the same way.

The patient presses the abdomen firmly against the back of the screen, by way of making the shadows more clear. The bismuth meal is then administered. It is usual to watch the progress of the first few mouthfuls into the stomach as far as it will go in a brief space of time. Occasionally very valuable information is gained by doing this.

The rest of the meal is given rapidly, the shape and size of the stomach noticed, and a tracing of it made upon paper.

Normal appearances.—This was one of the most difficult matters to ascertain. The difficulty arises from the fact that there are few normal adult stomachs, that is, perfectly normal in the most literal sense, and also from the fact that there is no particular size and shape of stomach that can be said to be normal more than several others. This is a matter upon which it is impossible to do more than generalise, but when the general design of the healthy stomach began to be recognised after a great number of observations, many of the difficulties attending the X-ray diagnosis of gastric diseases began to disappear.

The most usual form of normal stomach has been compared to the shape of the letter J by Barclay, and to that of a bull's horn by Holtznecht. Barclay's description is, perhaps, the more accurate, but I am not sure that there is much to choose between them. The horn-shape would seem to be the most normal one, and the pylorus at the lowest point. As we have seen, the really normal stomach is rare, and as the lowest point of the stomach is frequently from one to one and a half inches from the pylorus, the J-shape is probably more frequently observed than any other.

As regards shape, we may describe the upper third as pyriform, and this part is usually filled with gas or air; the lower two thirds are tubular in form, and the diameter varies with the amount of food present.

As to position, the upper two thirds are almost vertical, while the lower third is nearly horizontal, and except for the pylorus the whole organ is always entirely to the left of the middle line; it extends from the left dome of the diaphragm to about the level of the umbilicus.

The pylorus *should* be on the level of the lower pole, and it may be to the right of the middle line. In the average individual the stomach extends below the umbilicus, and the lower pole is below the pylorus.

As has been stated, the truly normal stomach is rare, but experience shows that mild degrees of gastroptosis give rise to no symptoms so long as the motility remains good.

The shape of the normal, that is, healthy stomach, is such as anyone of a mechanical turn of mind would design for such a purpose, that is, a reservoir

and digesting organ; the outlet—pylorus—is placed at the lowest point, and it is here that we find the strongest muscle and the greatest number of secreting glands.

One of the most perfect stomachs I ever saw was in a child æt. 2 years, who had swallowed a foreign body. On the screen it appeared to contain nothing but air or gas, but not enough to cause distension. It showed as a brightly illuminated area of the horn-shape, and the pylorus at the lowest part. It is said that the stomach of the child is more globular than the adult, more uniform in every way, and that it empties more quickly; in fact, the infant stomach appears to empty itself almost at once.

With this method of examination we can see a great deal more than the mere size, shape, and position of this organ. Being a hollow muscle, peristaltic waves can be seen passing along the greater curvature at intervals of about twenty or thirty seconds. In the quiescent state these waves are small ripples of the saw-tooth variety, and at comparatively long intervals. After a liquid meal they become more undulatory and of greater volume, while a solid food gives rise to waves of great volume, which are often single. In the erect position these waves are increased in strength and volume. The "churning" movement, about which we were told in former days, has been shown to be a complete fallacy. Foods do not tend to mix, but tend more to arrange themselves according to their specific gravity.

When we come to think of it, it is not quite apparent what useful purpose churning and mixing of the food in the stomach would serve, and the idea probably arose from some error in observation. It is possible at all times for fluids to pass around the food-mass to the pylorus.

We may now pass on to the consideration of some of the abnormal conditions of the stomach, and the one we are most frequently called upon to investigate is that of gastroptosis.

The chief support of the viscera is the anterior abdominal wall, and if this becomes weakened from any cause, such as pregnancy, ascites, removal of a cyst, or even general weakness, gastroptosis will follow.

The first thing one notices in a case of the kind is that the lower border of the stomach shadow is well below the umbilicus, and between this portion and the fundus is a narrow constricted part, joining the two, stretched out like a piece of rubber tubing would be. The bismuth shadow may, in severe cases, be found below the iliac crest, and even in the pelvis. The number of forms that this shadow may take is very great, but they conform, as a rule, to the general plan I have laid down.

It is found that the lower pole can be made to rise from one to four or five inches by contraction of the anterior abdominal wall; it can also be made to rise by the pressure of the hand properly applied; further, it may be maintained in this position by a suitably applied bandage. This is best done in the recumbent position; a subsequent X-ray examination will show if it has been rightly put on.

One writer, whose name I cannot recall, has noticed the frequent association of gastroptosis and phthisis, and he accounted for it by the fall of the abdominal organs causing a partial vacuum in the cavity, and hence a flattening of the diaphragm. This in turn causes a flattening of the chest, with restricted respiratory movement. Such a condition would favour the development of tuberculosis, and the writer emphasised the necessity for keeping the abdominal wall well supported, as also exercises to help the muscles to regain their tone. The idea, as well as the explanation, are both ingenious, but I have not had an opportunity of testing them for myself. Some of the worst cases of gastroptosis have been observed in women who have been in the habit of lacing tightly.

Another condition that calls for this examination is that of atonic dyspepsia, and it is one in which it is easy to gain very positive information. The chief trouble here is that the food remains in the stomach too long a time. In an aggravated case no peristaltic waves can be seen when the patient is in the erect position. The atonic muscle seems to be unable to deal with the weight of the meal, and this shows the advisability of resting in the recumbent or semi-recumbent position after eating. This condition is frequently associated with some degree of gastroptosis, and a properly applied bandage is often of great help.

It has been observed that massage of the abdominal wall stimulates peristalsis—a positive demonstration of the value of a time-hounoured practice.

Carcinoma of the stomach.—It is unfortunate that so far we have not yet discovered any appearances

that can be said to be distinctive of this serious condition. In examining a suspected case it is important to give single mouthfuls at a time and watch their progress from the upper to the lower pole; a mass of firm bismuth gelatine is very useful for this purpose. If it is seen to take an irregular course it is suspicious, and may be due to its curling around an obstruction; it may be completely arrested for a time if a constriction is present, but we must be careful not to be led astray by the presence of an hour-glass contraction.

Goldman has shown how the gastric borders can be made more clear by inflating the colon with air, and if a small amount of bismuth emulsion is given some of it sticks to the stomach-wall and enables it to be made out very clearly in many cases. An irregularity in the outline, an indentation, for instance, is very suggestive of carcinoma.

If the pyloric end is involved the stomach is seen to be in violent peristalsis, as if trying to force the food through the narrowed opening. To state the case briefly, we may say that there is reason to suspect gastric carcinoma when the volume is decreased, the food takes an irregular course, peristalsis is impaired, and the outline of the organ distorted. It may also become fixed and even displaced. We must not attempt to diagnose gastric carcinoma by the X-ray method alone; it is only an aid to diagnosis, but an extremely valuable one.

Very much more might be said regarding this part of my subject, but it is impossible to do more than give an outline in the time at our disposal. I think, however, that enough has been said to show how much help can be obtained by this method of investigation. The stomach specialist may be able to find out all he requires to know by other methods, but all cannot be stomach specialists, and after all there is something to be said for a method of examination that gives visible evidence of departures from the normal state of things.

Intestines.

This part of the subject need not detain us very long on this occasion. Except for ascertaining the results of operations upon the stomach and intestines, such as an enterostomy, the matter resolves itself, for all practical purposes, into an investigation of those cases of constipation where the cause or site of the obstruction is obscure.

The procedure is to give the ordinary bismuth meal as for examining the stomach and to make examinations at certain intervals to ascertain the progress of the bismuth.

In such a case the usual time of making the first examination is four hours after ingestion. By this time the first part of the meal should be at the cæcum. The rest of it may be seen scattered more or less generally over the abdominal cavity. It is recognised by the short vermicular shadows, which represent very fairly casts of the small intestine. This part of the digestive tube is seldom at fault, and when it is, it is due to some external influence, such as an adhesion of one of the loops to some more fixed part, and not to inherent defects.

By the time the bulk of the mass has passed into the cæcum and ascending colon its position is well recognised as a rule.

In a normal case the bismuth should reach the middle of the transverse colon after nine hours, but it does not reach the sigmoid flexure until twenty-eight hours after ingestion.

With these times to guide us it is comparatively easy to determine the cause and situation of the delay in the normal passage of the digestive tube. Of course many abnormal conditions are recognised in the same way. For instance, ptosis of the transverse colon gives a very characteristic appearance of the letter **M**. If the ptosis also involves the ascending colon the two form a long sweeping curve from the cæcum to the splenic flexure. Numerous other arrangements are to be seen, in fact it may be said that no two cases give exactly the same picture, though normal cases conform to a general arrangement that is readily recognised—this will be seen in the radiographs that are before you.

The images given by the digestive tube after special operations conform to no fixed rule, and are at times more or less bewildering, but very often it happens that the appearances are sufficiently clear and definite to give the required information.

In conclusion, I wish to emphasise the fact that it is impossible to give more than an outline of this subject in a single lecture, but I think I have given sufficient to indicate the value of the method in assisting in the diagnosis of some diseases of the digestive system, and if I have succeeded in this it is all I have aimed at.

It is my pleasant duty to pay homage to the excellent work that has been done by Thurstan Holland, Barclay, Hertz, Groedel, and many others.

(The lecture was illustrated with numerous radiographs and diagrams.)

December 5th, 1910.

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A LECTURE ON ABORTION.

Delivered at the Polyclinic.

By G. E. HERMAN, M.B., F.R.C.P., F.R.C.S., Consulting Obstetric Physician, London Hospital.

LADIES AND GENTLEMEN,—The subject about which I have been asked to lecture to you is abortion. The frequency of abortions to labours of full term is about one in five. Many people have taken the trouble to make estimates of the frequency of abortion, and the agreement of their conclusions is as close as one could expect it to be, namely, that on the average one pregnancy in five terminates in abortion. That proportion is important, because when you are told that this or that condition produces abortion, it ought to be shown that when that condition is present, abortions are more frequent than one in five.

Abortion is important from three points of view: (1) The prevention of abortion; (2) the dangers of abortion; and (3) the treatment of abortion.

First, the causes and prevention of abortion. The only causes which are of practical importance are those which we can prevent. Those which we do not know how to remove have no particular importance whatever the condition of the patient may be. Dr. Rentoul, whose book on abortion is a useful one, gives a lists of poisons in the mother's blood which may produce abortion. First, any fever which goes over 105° F., such as enteric fever, scarlet fever, smallpox, relapsing fever, typhus fever. In all these conditions—if the temperature rises to 105° F., the danger of abortion is great. But this has little obstetric importance, because the thing we have to do is to treat the disease and prevent abortion only by appropriate treatment of the main disease. You will find in most museums specimens of decidua thickened with polypoid

growths hanging from them. That is a disease about which we know nothing which enables us to identify the cause or prevent it, nor do we know its significance. After measles hæmorrhage into the uterus has been found. The endometrium has been found congested, reddened, and softened, and placenta prævia and accidental hæmorrhage have been shown to occur very often in patients who have become pregnant shortly after measles, and so has abortion.

Laceration of cervix in former labours I believe to be an important cause of abortion, and I shall speak of it further. Anteflexion, sometimes said to be a cause of abortion, is the natural shape of most virgin uteri. Retroflexion and retroversion are said to cause abortion. I collected figures on the subject many years ago,* and I have found that in the early years of the child-bearing period miscarriage did not occur oftener with retroflexion and retroversion than in patients with the uterus in the proper position. But in the later years there was a larger proportion of abortions associated with the change in position of the uterus. And my explanation of those facts is that it is the failing reproductive energy of the patient which at once causes her to perceive discomfort from the altered position of the uterus and causes pregnancy to end in a miscarriage. Adhesions in the pelvis when pregnancy takes place become more vascular, and when softened are often absorbed. But if they are not absorbed they may lead to abortion. The cancerous cachexia often produces the intra-uterine death of the child, and thus abortion. Fibroids are often credited with being a cause of abortion, but that is certainly not the case, because every collection of cases of fibroids with pregnancy that I know of which has been examined with the view of proving the influence of fibroids on abortion shows that in pregnancy with fibroids abortion does not take place as often as one case in five. Cases have been described as placenta prævia in early pregnancy ending in abortion. I know not how that can be, because the placenta does not begin to take form until after the third month, whereas the greatest number of abortions take place in the first three months.

Now I come to consider the preventable causes. The effect of alcohol in pregnancy has been shown to be very marked indeed. Alcoholism in the mother leads to the production of a dead child, or to a child who dies in the first year or two. So in that way the effect of alcoholic excess tends to prevent the handing on of such a tendency to posterity. Those facts have been shown: the children of definitely alcoholic mothers are either stillborn or die early of convulsions. This fact would lead one to think that abortions in such cases must be numerous. But there is no statistical evidence on the subject that I know of; it may be because the gentlemen who compiled these tables have not inquired into the frequency of abortion. I think it extremely probable that an alcoholic mother would abort. One of the commonest causes of abortion is age. It is common for the child-bearing activity of a woman to end in an abortion. It has been said that in certain manufacturing districts in the North women will take lead to produce abortion. But I have not seen much of that. Syphilis requires a little consideration, because the point of view of the text-book writer and the general practitioner are not the same in this matter. We are told of a wonderful new remedy which is going to stop syphilis. But not every patient has got that yet, so we are still likely to find abortions which are due to syphilis. The test for syphilis, as most people know, is the "Wassermann reaction." This may be very accurate, but it is not one which the general practitioner can himself use in his daily work. The history must be necessarily incomplete, because you cannot run the risk of sowing discord between husband and wife by suggestive questions as to the ante-nuptial history of the other partner. So if you get a married woman who has had two or three abortions, it is best to assume the previous occurrence of syphilis and not worry the patient about the history. Leaving out of consideration for the moment the new remedy which I have mentioned, the treatment of the syphilis is by means of mercury. Small doses of mercury are enough for these patients; it does them no harm, but rather acts as a tonic. I have had repeated experience of patients who have come with the history of having had a few miscarriages, and wished not to have any more. I have assumed the occurrence of syphilis and have prescribed hyd. cum cretâ gr. j with pulv. ipecac. co. gr. j, and have used as a vehicle an American remedy, ext. viburni prunifolii gr. x. I tell the patient these are tonic pills,

* 'Trans. Obstet. Soc. Lond.,' vol. xxxiii.

and that they are to take them throughout the pregnancy. I have had letters from these patients subsequently thanking me for the result that they had been delivered of a healthy living child. That is one great preventable cause of abortion, namely, syphilis.

There is an antagonism between the cervix and the body of the uterus which has been called the polarity of the uterus. The body of the uterus tries to expel its contents, and the cervix keeps it in. If the cervix has been deeply torn, then when any external stimulus provokes the contractility of the uterine body, and the uterine body tends to expel the ovum, there is little power in the cervix to oppose that. I think this is a sound argument, and I have tested it by experience in women with lacerated cervices who have had miscarriages, and were anxious not to have any more. I have repaired the rent in the cervix, and the patients have in the next pregnancy gone to full term. So I have no doubt that lacerations of the cervix sometimes cause abortion. The extent to which this is liable to occur is in proportion to the depth of the tear. With regard to criminal abortion, one believes this is more common than it should be. I am not a lawyer, and I do not know how long the law of criminal abortion has existed, but it is one which it is impossible to carry out. I will read you the law on the subject so that you may see what it is: "Every woman being with child who, with intent to procure her own miscarriage shall unlawfully administer to herself any poison or other noxious thing, or unlawfully use any instrument or any means whatsoever, and with like intent to procure miscarriage, any woman, whether she be or be not with child, shall be guilty of felony." That is, you are guilty of felony if you intend to procure abortion in the case of a woman who is not pregnant. This law is comparatively seldom put in force. It seems to me the induction of abortion should be regarded as an offence against morals rather than against the criminal law. It has been repeatedly used as an instrument of blackmail. Having abortion procured is not, from the point of view of the patient, a very agreeable thing. I have had patients in whom I have advised the induction of abortion, and with the co-operation of the family doctor, have performed it; but when the patient was pregnant again she has not desired an abortion again—she wanted to go to full term. And I think that is the case with most. To empty a pregnant uterus, when done under favourable circumstances and with proper attention to antiseptic principles, is a simple and harmless procedure. The reason death sometimes follows criminal abortion is that the procedure has to be done in concealment, and it is generally done by ignorant people who use dirty instruments, and death comes from the ignorance of the operator. I think early abortion is much commoner than is believed. There are cases of membranous dysmenorrhœa which are nothing but monthly abortions. The late Dr. Cory recorded a case in the 'Transactions of the Obstetrical Society of London'* of a woman who passed a membrane every month with pain. But when she went away from home she did not pass any membrane, and had no pain; and if her husband was away from home, then so long as he remained away she did not pass any membrane and had no pain. But when he came back the membranes appeared again. I saw a case not long ago in which a married couple were anxious for a child, and the husband was a histologist. He saw a minute solid body in his wife's discharges, put it under the microscope and found it was an early abortion.

Abortion is said to be commonest in the third month or before it. Whether it is commoner in the second, or the third, or the first month, I do not know, because the earlier the abortion occurs the smaller it is, and the more easily it is overlooked. It begins to be less common in the fourth month. In retroversion of a gravid uterus abortion is comparatively rare. I suppose that the adhesion of the placenta to the uterus is firmer when that organ is more developed. The doctor is sent for in a case of abortion usually on account of hæmorrhage. Abortion is accompanied by pain, not severe—but it is the hæmorrhage which alarms the patient and causes her to seek medical advice. There is sometimes severe pain in these cases, but not often

The importance of abortion is that it is possibly a cause of hæmorrhage and sepsis. If the abortion is complete and quick the hæmorrhage is not great. But the cases which occupy us in the hospitals, which we see so much of, are cases of incomplete abortion. What happens is that the

* Vol. xx.

amnion bursts, the embryo and the amniotic fluid come away, and the membranes are retained. So long as the chorion is in the uterus hæmorrhage is more or less continuous. Secondly, it must be remembered that the lower part of the vagina always contains millions of micro-organisms, most of them saprophytes. As a rule there are no organisms in the body of the uterus. But if the organisms get up to the body of the uterus, as they easily can, they will lead to decomposition of the retained chorion, to sapræmia, and, possibly, septicæmia. Incomplete abortion has been spoken of as missed abortion, but every full-term pregnancy represents a missed abortion. Incomplete abortion is the proper term. When the ovum dies and the amnion is unruptured it comes away quickly as a rule, and the hæmorrhage after it is expelled is not very great. The ovum may die, abortion may not occur, and the dead ovum may be retained *in utero* for an indefinite time without any symptoms. But incomplete abortion always requires interference. A case of incomplete abortion generally presents itself to you as a patient who has gone two or three months without seeing anything, and then she has been losing for days or weeks. I will consider first how you should distinguish an inevitable abortion from one which can be avoided. The main indication is, in the first place, expansion of the cervix. If you can get your finger into the cervix and feel the ovum abortion is inevitable. After abortion the usual thing is for the cervix to contract up again. If the hæmorrhage is so copious as to blanch the patient you must dilate the uterine cavity and remove the stuff which is causing the hæmorrhage. You must do that for two reasons: first, to stop hæmorrhage, which is exhausting the patient and may kill her; and secondly, to remove from her danger of sepsis. The possibility of sepsis may seem to be a small one; but I remember, when I began practice, being sent for to a lady who had an incomplete abortion, and I removed the retained membranes with the assistance of the family doctor, under an anæsthetic. Years before the lady had been a patient under the care of the late Dr. Oldham, who was physician to Guy's Hospital a long time ago, long before the introduction of antiseptics; and when she had got about again she went to see Dr. Oldham and related what had happened to her. Oldham told her that it was unnecessary to empty that uterus; he said the abortion would have come away of itself, and it would have been much better not to have interfered with it. And, from his point of view, he was right; it was, at the time he was in practice, safer to leave it than to put a dirty finger or dirty instruments into the uterus. If you use clean instruments and if you employ proper skill there is no particular danger. You are told sometimes that the incomplete abortion should be removed with the finger. This is a statement copied from book to book. You cannot remove anything with one finger. You can detach it, but how can you catch hold of it with one finger? You must use an instrument to remove it. Besides, a bit of chorion may be adherent. In such a case the proper course is to dilate the cervix. This may be done with tents or with Hegar's dilators. As a rule I prefer a piece of laminaria, followed by Hegar's dilators. You can dilate, in a pregnant uterus, the cervix more easily than that of an unimpregnated uterus. With Hegar's dilators you get the cervix open until it will admit the finger. When you insert the finger you can feel there is something inside the uterus, and you can put in a forceps, grasp it, and bring it away. There are forceps sold in instrument-makers' shops under the name of "ovum forceps." They are very old instruments, and seem to have been constructed with the idea that the ovum is a precious thing, and that although you remove it from the uterus and it will be of no use afterwards, yet nevertheless it must not be injured. Therefore, these instruments are constructed so that they will not firmly grasp anything. If you are going to remove an ovum from the uterus there is an end of that ovum; it has no particular value, and therefore the forceps you use for the purpose should be capable of holding. Here* is a forceps I use. The serrations fit into one another, and therefore these forceps, you will easily see, will hold. If you have the cervix wide enough to admit your finger, pull down the cervix with a volsella, and push the forceps up, open and then close them again. If there is anything in the uterine cavity you are bound to seize it. By putting the forceps up time after time and exploring the cavity in various directions you will soon find you do not get anything more, and you may safely conclude that the uterus is empty. But there is still the decidua.

* Picture in Down Bros.' Catalogue, p. 728, No. 3255.

Abortion may have been caused by disease of the decidua. And if there is diseased decidua bleeding will continue until it has come away. I think it is best to hasten the separation of the decidua by scraping it away with the curette. If you do that you will find that the hæmorrhage soon ceases. If the endometrium should be thickened and you do not scrape it away the hæmorrhage will last a few days, but the hæmorrhage will not last longer than a day if you scrape it away. There is another thing which should be borne in mind, viz. that there is a disease which is known as chorion-epithelioma, a form of malignant disease of great virulence, which attacks the uterus after abortion or delivery, most often after abortion, and especially when there has been so-called vesicular mole. This is present in only a small number of cases, but bearing in mind its extreme malignancy every substance which is removed from the uterus should be examined, because if it should be chorion-epithelioma it becomes of vital importance that the patient should be relieved of her uterus as early as possible. After scraping out the uterus it is well to wash out the body of the uterus with an antiseptic douche.

Those are the chief points of importance in connection with abortion. First, because the patient will go on bleeding as long as fragments of chorion remain in the uterus, and also because if the bit of chorion is not cleared out it may putrefy and cause sapræmia. For those two reasons the prompt emptying of the uterus in incomplete abortion is not only the best treatment but it is the necessary treatment. The principal means whereby abortions may be prevented which are known to me are the treatment of syphilis and the repair of the cervix. There are other causes of abortion, but they are causes which cannot be altered. You cannot, for instance, alter a patient's age, and make an old woman into a young one.

December 12th, 1910.

A NEW comedo extractor has been brought out by Dr. T. P. Beddoes. The instrument is made by Allen and Hanburys, and is so constructed that graduated pressure can be applied whilst the "blackhead" remains under observation, and injury to the skin is prevented.

TUBERCULIN DISPENSARIES AND SANATORIA.

By A. MEARNS FRASER, M.D.,*
Medical Officer of Health, Portsmouth.

PULMONARY tuberculosis has affected the people of this country for years, but it is only in comparatively recent times that the nature of the disease has been completely understood. Owing to the researches of Koch, the results of which were published in 1882, it was demonstrated beyond doubt that tubercular disease was caused solely and entirely by a small micro-organism, invisible except with a high-power microscope, termed the tubercle bacillus. This tubercle bacillus gains entrance to the body in food, or, more usually, is inhaled in the act of breathing. It may attack the lungs, the brain, the liver, the intestines, joints, gland, or, indeed, any part of the human body; the organs, however, most commonly affected are the lungs, where it gives rise to the wide-spread disease known variously as pulmonary tuberculosis, tuberculosis of the lungs, phthisis, or consumption; it is with consumption that this report is principally concerned.

Consumption is an infectious disease and spreads from person to person, and from one member of a family to other members. The principal mode of infection is by inhalation of the tubercle bacillus; these are discharged into the air in large numbers by consumptives in the act of coughing, or spitting, or sneezing, and may subsequently be inhaled directly, or after having become dried and powdery. Although the tubercle bacilli, which are coughed up or expectorated by a consumptive patient, will live for some time in dark insanitary rooms, they are rapidly destroyed on exposure to sunlight. As is therefore only to be expected, consumption spreads more frequently in dirty, close, and badly ventilated dwellings or buildings.

It is estimated that tubercular diseases are responsible for 50,000 deaths in this country each year; in Portsmouth alone for the past ten years they have caused nearly 300 deaths each year.

* Being his Special Report to the Council of the Borough of Portsmouth on the provision of sanatorium treatment and tuberculin dispensaries for the treatment of inhabitants of the Borough suffering from consumption.

Stated in another way, this means that out of every nine persons one will die of tubercular disease; or again, the population of Portsmouth at the present time is 217,986; if no successful measures are undertaken to diminish the present consumptive death-rate, 24,220 of these will ultimately die of consumption. This, however, in all probability does not represent the whole truth, for probably a number of deaths that are attributed to bronchitis, pleurisy, influenza and other causes are due, directly or indirectly, to the tubercle bacillus, and there is this further appalling feature in connection with tubercular disease, it is not a disease that attacks after the best part of life is run, like cancer and some other diseases, but it attacks man in his prime, the most fatal period being from twenty to forty years of age, the very period when he should enjoy life, when he is of most value to the State, and when in many cases he is the main support of a family, the members of which are dependent upon him for their daily bread.

As a matter of fact consumption is even more widespread than the above figures indicate, and it will probably come as a surprise to many to learn that quite half of the people who reach adult life suffer from consumption to a greater or less degree. The proof for this statement is found in the post-mortem room, where examinations have shown that a very large proportion of persons, who have died of other diseases than consumption, have at some time or other in their lives also suffered from consumption, from which, however, they have quite recovered at the time of death. Nägeli, of Zurich, in the course of 500 post-mortem examinations found that no less than 90 per cent. had suffered and recovered from consumption; Lubarsh, of Posen, stated that out of 800 post-mortems 88 per cent. had suffered from consumption; and in this country Dr. Harris stated that there were signs that *extensive consumption* had been cured in 39 per cent. of the post-mortems at the Manchester Royal Infirmary.* Prof. Brouardel, of Paris, speaking on the same subject, says that the scars left by tubercle showed that there had not been simply a mild attack, but that, judging from the size of the scars, the disease had in many instances reached an advanced stage.†

Prof. Sir T. Clifford Allbutt, in his opening address on sanatorium treatment at the British Congress on Tuberculosis, 1901, said, "I am guilty of no extravagance when I suggest that one third of you who hear me, wittingly or unwittingly are, or have been, infected with tubercle."

The foregoing figures, startling though they may be, have their bright side, for they show, what is of the utmost importance, that wide-spread though consumption may be, it is also curable, that, in fact, the majority of people attacked must eventually recover and become quite well again. Brouardel states that "Phthisis, therefore, is curable even in its most advanced stages," and Grancher expresses the opinion that "Tubercle is the most easily cured of chronic diseases."*

In view, therefore, of the awful ravages of the disease as seen in our workhouses, hospitals, and in the death returns, coupled with the evidence that exists of its curability, there can surely be no subject which should appeal more to sanitary authorities than that of the prevention and cure of consumption.

So widespread have been the misery, poverty, and death caused by consumption that public opinion all over the country is unanimous as to the urgency of the need for taking some action towards the prevention and cure of the disease. The public, prompted by the medical profession in this and in other countries, seems to have committed itself to the principle that the best means of curing consumption lie in the provision of sanatoria, and for preventing it in the improvement of the conditions generally under which the poor live. The latter is being energetically carried out in this borough, and by active administration of the Housing of the Working Classes Acts, by giving advice to persons affected with consumption, by providing sputum bottles, by disinfecting infected houses, and by providing the means for free bacteriological diagnosis a large amount of preventive work is being done. The advantage to the borough from these preventive measures will be disputed by no one; they must have considerably increasing effect in preventing the prevalence of the disease. The only question now under consideration is whether we should not go a step further, and, following the example of certain other authorities, provide a sana-

* Report of Medical Officer to Local Government Board on Sanatoria, etc., 1905-6, pp. 105. 106.

† *Op. cit.*, p. 105.

* *Op. cit.*, p. 105.

torium for the treatment of the consumptives in Portsmouth.

It is not astonishing, considering the terrible ravages of the disease, that one should grasp at any means of lessening the evil, and the provision of sanatorium treatment being the fashion, it is only natural that action in this direction should be urged upon the Council as the one means of salvation. But, however anxious to see an attempt made by the Municipality to deal on a comprehensive scale with the prevention and cure of consumption, I cannot, as your responsible medical adviser, commit myself to advocating the institution of sanatorium treatment until convinced that this is the best method to adopt, and that the results to be obtained from it will be commensurable with the expenditure it will necessitate. I would ask you therefore to consider very carefully the points I shall put before you, always bearing in mind that it is consumption amongst the poor and amongst the working classes that forms the bulk of the problem with which we have to deal.

Cost of Sanatoria.—I have gone very carefully into the cost of the provision of sanatoria, which is admirably given in the Report of the Medical Officer of the Local Government Board, 1905–06, already referred to, but I do not intend, for reasons that will appear later, to discuss the subject very fully here. It is sufficient to say that the cost varies enormously, and while it may be anything from £150 to £1000 per bed, I do not think any kind of permanent sanatorium in or near Portsmouth could possibly be erected at less than £200 per bed, and the cost of maintenance with administrative charges will in no case be less than £2 per patient per week. The cost of a borough sanatorium and of its upkeep can roughly be calculated upon this basis, *e.g.* a sanatorium of fifty beds would cost £10,000 for erection, and if it were fully occupied it would cost £5000 per annum. And although it could not cost less, it might easily cost double.

In the event of sanatorium treatment being decided upon in this borough, it will be necessary to ascertain how much accommodation would be required. The number of deaths from consumption in Portsmouth has, on the average, been over 280 per annum during the past ten years. But, in addition to those 280 persons who die in one year, there must also be a large number ill with consumption who will die during the next two or three years, and taking the average duration of the disease at two and three quarter years (a moderate estimate), there must be at any one time nearly 800 persons suffering from the disease in the town. It is out of the question for all these persons to be treated in a sanatorium; to treat 800 persons, allowing each six months' treatment, would entail a sanatorium of 400 beds—that is to say, an outlay of £80,000 and an annual cost of £40,000 a year.

A little consideration will show that the amount of sanatorium accommodation that can reasonably be provided can only benefit a very small proportion of the persons suffering from consumption in the borough, and a further point is, as I hope to show later, that not only is the cost of sanatorium treatment excessive, but that a proportionately far greater amount of good can be effected in other directions by the money that would need to be expended for the provision of quite a small sanatorium.

Sanatorium Treatment.—The objects of sanatorium treatment are three-fold in character:

(1) The cure of persons suffering from consumption.

(2) The prevention of the spread of infection by means of the isolation of consumptives.

(3) The education of consumptives in the mode of living and conduct most favourable to themselves and least dangerous to those associated with them.

Sanatoria as Curative Agencies.—When sanatoria, on the lines of open-air treatment as now understood, were first started in this country some few years ago, the most sanguine expectations were indulged in as to the success that would be achieved in the cure of consumption. Unfortunately these expectations have not been realised, and there have been, to my knowledge, no results published which warrant the assumption that, by means of sanatoria alone, they ever will be realised. It cannot be denied that the immediate results of sanatorium treatment are encouraging, and patients almost invariably improve under it. Nor is this to be wondered at when we remember that many of the patients on entering the sanatorium exchange a hard existence, where they are often badly fed and live under insanitary conditions, for a life of rest, good food, and careful medical supervision.

Unfortunately a large proportion of the cases which have improved in the sanatoria suffer relapses on leaving these institutions, and are soon as ill as they were before; probably many cases have come within the knowledge of members of the council of persons who, although they have returned from sanatoria improved in health, have again relapsed after a few months' interval. This experience is not limited to this country; sanatorium treatment has been far more widely practised in Germany than here, and in Germany the after-history of cases that have been treated in sanatoria has been more thoroughly inquired into. It is found in Germany that of the cases discharged from sanatoria 7·6 per cent. are unfit to work or are dead in 1–1½ years; 19 per cent. in 2–2½ years; 33·3 per cent. in 3–3½ years; and 55·6 per cent. in 4 years. These investigations were carried out by Weicker, who, by the way, began them as an enthusiastic advocate of sanatorium methods.* It is also important to remember that these statistics refer only to cases which were admitted in the earlier stages of consumption, *i. e.* those cases in which, above all others, sanatorium treatment is expected to effect a cure.

In the report of the Local Government Board already referred to, Dr. Bulstrode, discussing the lasting effects of sanatorium treatment, comments upon the omission of sanatoria to publish the after-results of their cases. Whilst agreeing that this omission may be due, in considerable degree, to the difficulty of keeping in touch with patients after they have left the sanatoria, he goes on to say (p. 184) that it is to be feared that this omission "is also due likewise, in a considerable degree, to the disparity between the actual results obtained and the optimistic prophecies made by some persons at the inauguration of the sanatorium movement in this country."

Further evidence of the failure of sanatoria to cure consumptives so that they are able to return to work and earn their living (and it is not right otherwise to call a case a "cure") is afforded by extracts from annual reports of sanatoria quoted in the same report (pp. 191–193). From these I may quote the following:

From the annual report of the Daneswood Sanatorium:

"With regard to the cases which leave the Sanatorium as 'arrested' and 'much improved,' we desire to emphasise the importance of the after-care of such cases. If they are allowed to return to the same unsatisfactory and unhygienic conditions under which they originally developed the disease, there is no doubt that, even if arrested at the Sanatorium, the disease will soon break out again."

In the third Annual Report of the Leeds Sanatorium it is stated that:

"The Committee feel it must be recognised that the best permanent results of sanatorium treatment cannot be obtained where the majority of patients must return to unhealthy and poor homes. The problem of finding work for discharged patients has been occupying the attention of the Committee, but its solution is an exceptionally difficult one."

The report of the Kelling Sanatorium states:

"Stress of circumstances and the necessity of accepting any work which may be obtainable, often in most unsuitable surroundings, must have a harmful effect, and lead to much disappointment to those who have helped patients to meet the expense of sanatorium treatment. This seems to emphasise the experience that the healing effect of sanatorium life, unless supplemented by subsequent care and help, is often thrown away."

From the Devon and Cornwall Sanatorium Report:

"The figures of after-results are by no means good. The chief cause is that patients delay coming until the disease is far advanced, so that even if they can be brought to the stage of relative cure by sanatorium treatment, the strain of returning to their old surroundings is too much for them and they break down again."

The Report of the Bowden and Crossley Sanatorium contains the following:

"Whilst the good results of the treatment of patients both at Bowden and Delamere are quite equal to those met with, not only in sanatoria in this country but also on the continent, the Medical Board regret to record that after a short interval they see the patients too frequently relapse into their former condition. They are confident that this occurrence cannot to any great extent be due to re-infection, since in Manchester, more particularly, it is customary to disinfect the homes on the removal of patients to the sanatorium."

Many more extracts could be given, but these are sufficient to show that patients, especially those from the working classes, who form the problem with which we have to deal, are not capable, after being discharged from sanatoria, of returning to everyday work and earning their living. In other words, the disease, in the majority of patients discharged from sanatoria, is only temporarily arrested and not cured.

* 'Treatment of Consumption,' W. Camac Wilkinson, p. 136.

A frequent subject of complaint by superintendents of sanatoria is that the patients who seek the treatment do not come in an early enough stage of the disease, that they only come when the disease is too far advanced for them to benefit permanently by sanatorium treatment. Although this explanation may quite correctly account for a number of the failures under sanatorium treatment, it must surely at the same time be a powerful argument against relying on the provision of sanatoria alone as a solution to the consumption problem. For if it is, as is now generally admitted, an essential to the success of sanatorium treatment that the cases must be admitted in the earliest stage of the disease, it must also be admitted that, as regards the working classes, this essential will commonly be wanting, and therefore sanatorium treatment can never prove successful in regard to this class.

Consumption usually commences in an insidious manner, and the working man in employment is not going to give up his job or even consult a doctor on account of the initial symptoms, which often appear to him trifling. He has not enough money to afford to go to a doctor for slight ailments, consequently he continues working until the disease has got such a hold on him that he is physically incapable of continuing. The result is that before he sees a doctor, and is advised to seek sanatorium treatment, the disease has advanced beyond the stage in which sanatorium treatment is likely to have permanent beneficial effect.

Sanatoria for Purposes of Isolation.—But although sanatoria may not cure the majority of their patients, there are those who say that they may be used as a means of isolating patients in an advanced stage of the disease so as to prevent their being a source of infection to others. It is admitted that sanatoria may have a certain value in this respect; on the other hand, there is a strong disinclination on the part of the patient to spend his last few months of life away from his home, and in this disinclination he is often energetically supported by his friends and relatives. No doubt it would tend to diminish the prevalence of consumption if every patient in an infectious condition were isolated, but that such a procedure could ever be established in this country is out of the question. The only cases of advanced and hopeless consumptives who would be likely to consent to go in a sanatorium are those which even at the present time do go to the Poor Law Infirmary. As they are by this means looked after and prevented from becoming a danger to other inmates of their homes, there is nothing to be gained by providing for them extra sanatorium treatment. The argument, therefore, that a sanatorium should be erected in the interest of the general public with a view to isolating consumptive patients and so preventing infection may be dismissed as unsound.

Sanatoria as Educational Agencies.—The question of the educational value of sanatoria must also be considered. When the sanatorium movement was started their advocates—and I was amongst their number—looked forward to it as likely to effect cures in a large number of cases. To-day I regard sanatoria from a far less sanguine standpoint, and that I am not alone in this opinion is obvious from the fact that at present the principal argument advanced in favour of their erection is based not so much on the cures that may be effected, as on the ground that patients there treated can be educated in the value of open air, suitable diet, and in the control of their sputum, so that when they return home they will have been taught how to live under better conditions, and how not to be a dangerous source of infection to other persons in the same house. Doubtless there is also a considerable amount of truth in this contention, although some writers on the subject point out, that for the effects of the sanatorium education to be lasting, it is advisable that the cases should be followed up in their houses by trained health visitors, who should advise the discharged patients how to adapt what they have learnt to the special home conditions existing in each individual case.

But, granting everything that is claimed for the value of the educational effect of sanatoria, it appears to me that the cost of the education is excessive. To pay for the education of one patient at the rate of £2 per week for the two, three, or four months he may be in a sanatorium is a very heavy charge on the rates, and far better value for the money could be obtained by the employment of trained nurses, who would periodically visit consumptives and explain the best methods for them to adopt for their own health and for the protection of those with whom they live. At the most not

more than six patients per bed could be efficiently educated in a sanatorium during the year, the minimum cost of which would be £100. For the same money a trained health visitor could be employed for a year, in the course of which time not six but a hundred patients could be instructed. This health visitor would have the further value that, in visiting the house of the consumptive, she would recognise other members of the family affected with consumption whilst yet in the early stage, in which it is apt to be ignored, to whom there might thus be brought the opportunity of securing early and successful treatment.

CONCLUSION AS REGARDS SANATORIA.—Summarising the views that I have expressed so far, it is my opinion that the provision of a municipal sanatorium by the local authority of this borough is not advisable. I am not opposed to sanatorium treatment on the ground that it does no good; in many cases undoubtedly good, and much good, is effected. What I maintain, however, is that the results so far obtained in this country, and also in Germany, do not warrant the amount of money that is expended upon them.

I am, of course, regarding sanatoria from the point of view of its effect upon patients from the working classes, for it is the members of the working classes that provide the problem with which we have to deal; the well-to-do and leisured classes can go from one sanatorium to another, and from one climate to another, wanting nothing that money can provide to give them a chance of health; and further, the well-to-do have no need to return to every-day hard work when they leave the sanatorium. If, however, sanatorium treatment is to be of value to the working man he must be able, when he leaves the sanatorium, to take up his ordinary work and earn a livelihood for himself and his family. It is of little use to say that he must take up some light job in the open air, returning periodically to the sanatorium to be toned up again to go on a little longer. Light open-air work is not easy to be obtained, and the common result is that the man and his family are soon destitute, and the man himself is of no working value to the State and community.

The views I have given expression to on the value of sanatorium treatment will possibly surprise many who have come to regard it as a specific cure for consumption, and they may be inclined to doubt the correctness of my deduction. I would ask these to investigate the statistical results of sanatorium treatment that have been published, and I venture to affirm that in no case will they find that it can be shown that 50 per cent. of undoubted cases of consumption amongst the working classes, treated in a sanatorium, have been found to be engaged in ordinary every-day work five years after being discharged as "cured" and "disease arrested." Further, in appreciating the cures from sanatorium treatment, do not lose sight of the evidence of the post-mortem room to which I have already referred, that a large proportion of cases of consumption cure themselves without any sanatorium treatment, and it is only fair to assume, in view of this evidence, that a certain proportion of the cases cured in sanatoria would also have become cured at home, either with or without medical attention.

If the local authority had plenty of money I would say nothing against the provision of a sanatorium; it would not do harm, it could only do good. But when it comes to the question of deciding how to secure the best possible results in the cure and prevention of consumption by means of a limited expenditure, then I say unhesitatingly that it is not by the provision of sanatorium treatment as at present carried out in this country. At the best, owing to financial considerations, any sanatorium which we could afford could deal with but a small percentage of the consumptives in the town, and of those treated there is no evidence in existence warranting the assumption that the majority would be cured or permanently benefited.

In stating thus my views on the non-advisability of the erection of a sanatorium in this borough, I should probably have been loth to decide so emphatically against it if I had not some other course of action, more certain in its results and infinitely less costly to recommend. If I had thought that sanatorium treatment, although it would be too costly to deal with the majority of cases in the town, was yet the treatment that held out the greatest hope of success, then I should undoubtedly advise the council to provide a sanatorium, and to deal with as many cases as the funds which could be devoted to this object would permit. The evils of consumption are so great that the fact of our not being able to treat every case

would be no excuse for omitting to treat as many as we could, even although these might represent a very small proportion of the whole. As I have stated above, however, I believe there is a better method available, and it is to this alternative course that I shall devote the latter part of this report.

TUBERCULIN DISPENSARIES.—The course which I am about to recommend the council to adopt for the treatment of consumptives in the borough is the provision of tuberculin dispensaries. Two dispensaries would probably be sufficient to treat all the consumptives in the town. As, however, tuberculin dispensaries are practically a new departure in this country, the council, if they adopt my suggestion, would possibly prefer to start with one, and watch the success of this before opening a second.

My reasons for advocating a tuberculin dispensary in lieu of a sanatorium is that I believe the results of treatment from the former are infinitely more satisfactory than from the latter. Incidentally, the cost is nothing compared to that of a sanatorium. The sum of £600 per annum would pay for a dispensary and everything connected with it, including medical attendance, a nurse, a caretaker, drugs, apparatus, etc., and allowing the dispensary to be open five hours a day and six days a week, sixty patients could be treated each week.

I mention the cost first to show that from a financial point of view, at all events, dispensary treatment of consumptives is well within the means of the local authority, and therefore, if it holds out the hope of success that I believe it does, is most assuredly well worth a trial in this town.

TUBERCULIN TREATMENT.—I will endeavour to point out as clearly as possible what is meant by tuberculin treatment. Tuberculin itself was introduced by Koch, and is a preparation made from tubercle bacilli which are grown artificially in a laboratory. It is harmless and has no effect when small doses are injected into a person who is not suffering from tubercular disease; if, however, the person is suffering from tuberculosis, in however slight a degree, there results what is known as the "tuberculin reaction." This reaction consists of a rise in temperature and a little local inflammation and hardness at the site of injection—the skin of the forearm is usually used for this purpose. Tuberculin has been very largely employed in this manner for purposes of diagnosis in doubtful cases, and, if it has no other use, it would for this reason alone be a most valuable preparation; by its means the physician can detect at once, in the earliest stages of the disease, whether or not a person has consumption, and inasmuch as the success of every treatment for consumption depends upon its application at the earliest possible stage of the disease, the value of tuberculin will be readily appreciated. Moreover, there is no other test of the same value, and, if it is not used, a positive diagnosis of consumption is often left until the bacteriologist has been able to detect tubercle bacilli in the sputum. The finding of tubercle bacilli of course absolutely settles the matter, but the fact of tubercle bacilli being found also indicates that the disease has made considerable progress; the value of this method is not, therefore, to be compared to that of tuberculin, which detects consumption before the bacilli appear in the sputum. This use of tuberculin has for some years been recognised and practised in Germany, and its employment in one form and another is now being increasingly taken advantage of in this country.

The employment of tuberculin for purposes of diagnosis is not, however, its sole or principal value, for it is probably, in addition, the most successful curative agent for consumption at present known, and the method of employment in this respect is as follows:

To a patient in whom consumption is suspected a small dose of tuberculin is given. If there is a rise of temperature it shows that he is tubercular. The treatment then consists in giving small doses of tuberculin at intervals, gradually increasing the dose until it is found that no reaction, *i. e.* no rise of temperature, follows. The fact of a sufficiently large dose being followed by no reaction indicates that there are no longer any active tubercle bacilli in the patient's system, that, in fact, the disease is either cured or arrested. Possibly the patient may be permanently cured; on the other hand, after this, as after every other known treatment, the disease may again break out at some future period; it is therefore generally advisable for a further "test" dose of tuberculin to be administered in about a year's time, or at some later date should the disease then show signs of recrudescence. In the event of the "test" dose of tuberculin showing that the disease has again become active, a further course of treatment is indicated.

The foregoing is practically the whole method of administration of what is known as the tuberculin treatment. It is very simple in character, and by a medical man who understands it can readily be carried out, as I have seen it, in quite a small dispensary, without any great outlay on scientific apparatus, instruments, or drugs. The most expensive item is the tuberculin itself, but it is estimated that the average cost of tuberculin, necessary for the complete course of treatment of one patient is only about twenty shillings.

RESULTS OF TUBERCULIN TREATMENT.—The great exponent of the use of tuberculin in this country is a distinguished London graduate, Dr. W. Camac Wilkinson,* and it is by his kindness that I have been enabled to see the tuberculin treatment as now being carried on at the Tuberculin Dispensary in Kennington Road, London. Dr. Wilkinson has been practising the tuberculin treatment since 1892, principally in Sydney, but later in London, where he has recently been instrumental in establishing a tuberculin dispensary. As to the results he has obtained I cannot do better than quote from his work 'Treatment of Consumption,' published in 1908. Dr. Wilkinson, after giving an account of each case (the following are all cases in the Stage I of the disease) sums up as follows:

"Out of the 110 cases treated fully from 1902–1908 I know of the death of three cases: one of acute malaria, one of splenic anæmia, and one the day after laparotomy for symptoms suggesting intestinal obstruction or acute pancreatitis. The last case died in New Zealand. I have done my best to keep in touch with all my cases, and I have not yet heard of a single case dying of pulmonary tuberculosis. In short, I know of two deaths among twenty-three early cases that refused tuberculin treatment, and I know of no deaths among the 110 that have had treatment. I know that ninety out of these are well and strong, and I would lay a wager about the others, giving odds. *At the worst I can claim success for tuberculin treatment in* 80 *per cent. of cases treated from* 1902–1908."

From the above statement it will be seen that, as the three patients who died succumbed to diseases which had nothing whatever to do with tuberculosis, and as, in spite of all efforts to keep in touch with the others, no deaths have been heard of, Dr. Wilkinson is justified in assuming that he has, so far as it has been possible to ascertain, secured 100 per cent. of cures amongst those patients in Stage I of the disease who had gone through a complete course of treatment. No such percentage results have been approached in the figures of any report on sanatorium treatment that I have yet heard of.

It is not, however, solely in the cases treated in the first stage of the disease that the use of tuberculin has proved successful; it is found also to be proportionably more efficacious than sanatoria in those cases in which the disease has reached Stages II and III. In support of this I quote the after-records of all the patients treated by Dr. Wilkinson between the years 1902–1905. The cases are classed according to the Stages I, II, or III, which the disease had reached when the patient first presented himself for treatment.

"AFTER-RECORDS.*

"*Summary of results observed*, 1907–1908.

"Cases treated in 1902:

"Stage I, 7 cases: 1 man went to New Guinea and died of malaria; no deaths known; 5 cases known to be well.

"Stage II, 4 cases: all alive; 2 had relapse after five years, but are now improving again.

"Stage II–III, 3 cases: 1 quite well, at school; 1 lost sight of; 1 very late, died in 1907.

"Stage III, 5 cases: 3 marvellously well; 2 died within a year, hopeless from the start.

"Treated in 1903:

"Stage I, 14 cases: 1 died of abdominal operation, 1907; 1 partially treated, lost sight of; 11 known to be well in 1907; 1 well in 1906, not heard of since.

"Stage II, 3 cases: 1 case gave up treatment after a few doses; he died within a year, this is a 'control' case; another went away after six months, lost sight of; 1 known to be quite well in 1908.

"Stage II–III, 3 cases: 2 remarkably well in 1908; 1 improved immensely and then gave way to drink, yet alive in 1907.

"Stage III, 1 case: improved immensely under treatment, but through foolish act brought on hæmorrhage and died.

"*Control cases*:—In 1903 had 2 'control cases.' One was tested by me in 1901; was then in Stage I; she refused tuberculin treatment, and in 1903 was in Stage III, hopeless. Another had sanatorium treatment for a year and was dying.

"Treated in 1904:

"Stage I, 13 cases: all alive and well.

"Stage II, 10 cases: 2 left off treatment, could not afford time; 1 partially treated, great improvement, left off coming; remaining 7 well.

"*Control case*:—1 'control case.' This patient decided at first to have tuberculin treatment. Then persuaded to

* 'Treatment of Consumption,' W. Camac Wilkinson, pp. 244, 245.

go to sanatorium. *Was exhibited in* 1905 *as successful case of arrested disease*, and was dead in two years and a half.

"Stage II-III, 5 cases: 2 left off treatment; other 3 surprisingly well in 1908.

"Stage III, 14 cases: 9 hopeless cases died within a year; 2 cases improved extraordinarily; 2 other cases were alive in 1908; 1 case improved greatly, but lost sight of since 1906.

"Treated in 1905:

"Stage I, 27 cases: 22 had full treatment, and all are known to be well in 1908, except one who died of splenic anæmia in 1906; 5 had partial treatment.

"*Control case*: M—, æt. 30 years, Stage I; refused treatment by me because she saw another doctor who did not find tubercle in sputum. In 1907 in Stage III.

"Stage II, 12 cases: 3 had only few doses; 8 had full treatment, and were well in 1907.

"*Control case*: 1 'control case,' put off treatment because he was too busy; he died within a year.

"Stage II–III, 9 cases: 8 improved immensely, and were well in 1908.

"*Control case*: 1 gave up treatment because he had not time; he died within eighteen months; a further 'control case.'

"Stage III, 20 cases: 9 hopeless cases, could not take treatment, died within eighteen months; 7 cases are alive, marvellously better; 4 cases temporarily improved."

What are very striking in the results given above are the numerous successes claimed for tuberculin in respect of patients in the later stages of the disease, *i.e.* in those cases in which the result of sanatorium treatment is admittedly unsatisfactory, and, as regards the very advanced stages, hopeless: "When the after-results (of sanatorium treatment) have been separately stated for early, moderate, and advanced cases, it is seen that the prospects of continued working capacity for the advanced cases are very far from encouraging."*

The employment of tuberculin as a curative agency has not been practised to any great extent in this country; there is, however, the Kennington Tuberculin Dispensary, to which I have already referred, and tuberculin dispensaries have also recently been established at Aldershot, Street (Somerset), and Irvine (Ayrshire).

Tuberculin treatment has also, partly in conjunction with hospital treatment, been recently carried out by Colonel Treherne, R.A.M.C., at the Cambridge Hospital, Aldershot. I have had an opportunity of seeing some of the cases of consumption under Colonel Treherne's treatment, and he has also very kindly forwarded me particulars of some of the results he has up to date obtained.

Out of four patients in Stage I of the disease, three are apparently cured and are now at work; in the fourth the treatment is not yet completed, but the patient is much improved and is also at work. Out of sixteen patients in Stage II, one is apparently cured and is in full work, the remainder are still under treatment; one is "nearly well," five show "remarkable improvement," eight show distinct improvement, and one has so far only had the treatment for ten days. Most of these patients are doing light work. Out of four patients in Stage III, two show considerable improvement, one being at work as a gardener; the other two are cases of "mixed infection," and are still under vaccine treatment.

The value of these particulars is that they show that in almost every case marked improvement, or cure, has resulted from tuberculin treatment, even in those cases in the II and III stages.

The word "apparently" that is used to qualify the "cured" cases must not be taken to mean that there is any doubt about the cures, but only that a sufficient length of time has not elapsed since the conclusion of the treatment to use the word "cured" unqualified. A patient ought not to be said to be cured until two or three years have elapsed since the conclusion of his treatment, during which time he has been fit and well, and able to follow his ordinary work; and not unless a "test" dose of tuberculin fails to give any reaction. (The terms "cured" and "disease arrested" so often seen in sanatorium statistics are very misleading, for they often refer only to the apparent condition of the patient on his discharge, and take no account of the fact that in many cases serious relapses occur a short time after discharge.)

At Street (Somerset) a tuberculin dispensary has been started by Dr. Hilda Clark only a few months ago, so no complete results are yet obtainable. Dr. Clark informs me, however, "of 54 patients, whose treatment was begun between July and September, 19 were in the first stage, and 35 more advanced or with complications. Of those in the first stage, 13 have shown much improvement and 5 some improvement. Of those more advanced, 17 have shown much improvement and 12 slight improvement. In no case has a patient got worse or suffered from the treatment.

* 'Report of Local Government Board on Sanatoria,' etc., p. 185.

In every case there is evidence of more or less improvement in the physical signs in the lungs. The majority of the patients are at full work. All are still under treatment, though several appear to be practically cured."

Tuberculin treatment has been more extensively employed in Germany than in this country, and it has been there largely used in conjunction with sanatorium treatment. As regards the results that have been obtained at one of the German Sanatoriums I may quote the following:

"Prof. Moeller, of the Belzig Sanatorium, near Berlin, and a recognised authority on tuberculosis, eight years ago an utter sceptic upon tuberculin treatment, is now one of its most earnest advocates. He tells us that in cases where sanatorium methods have utterly failed, tuberculin has been proved to have undoubted curative properties, and that while sanatorium never cures in three months, permanent cures are often obtained with tuberculin.

"In his first record Moeller showed that—

"Sanatorium methods alone cured 10·9 per cent. of the cases.

"Sanatorium methods and tuberculin combined cured 36·3 per cent.

"Arranged according to stages the results are—

"Stage I (this is the early stage of the disease)—

"Sanatorium methods alone cured . 31·8 per cent.

"Sanatorium methods and tuberculin cured . . . . . 75·0 "

"Stage II (a more advanced stage)—

"Sanatorium methods alone cured . 1·9 "

"Sanatorium methods and tuberculin cured . . . . . 20·0 "

"Stage III (very advanced stage)—

"No cures possible except as curiosities.

"According to Moeller's later experiences, *sanatorium methods alone* may cure 25·30 per cent. of the cases of pulmonary tuberculosis in the first stage (German Board of Health classification), while *tuberculin* in his hands has secured the same result in 84·6 per cent. of the cases."*

Here, again, is strikingly shown the marked superiority of tuberculin over sanatorium methods in those patients in whom the disease has advanced to the second stage, and in view of the other results obtained from tuberculin, not in conjunction with sanatorium treatment, it is only logical to assume that it is the tuberculin in Prof. Moeller's cases, and not the sanatorium, that has been the predominant factor in the results he has obtained. At the same time no one would wish to deny the beneficial effect of sanatorium treatment, and especially to that class of consumptives suffering from what is known as "mixed infection," greater benefit will accrue from tuberculin treatment if the patients can at the same time receive the advantage of careful medical supervision in a sanatorium. The latter has the undoubted effect of building up the body tissues and strengthening the resistance of the individual to those other pathogenic organisms that are often found in connection with the tubercle bacillus, whereas tuberculin acts specifically against the tubercle bacillus itself.

The conclusion to be drawn from ascertainable facts appears to be that sanatorium treatment alone will cure a certain proportion of patients in the first stage of the disease and a much smaller proportion of cases in Stage II. Tuberculin treatment alone will cure almost all the patients in Stage I of the disease, and a very much greater proportion than sanatoria of those in Stage II. That for a certain class of case the greatest success will be obtained from a combination of sanatorium with tuberculin treatment, but if only one treatment is available, that of tuberculin is infinitely to be preferred.

There is one factor of the utmost importance in considering the respective merits of tuberculin and sanatorium treatment as regards the working classes. I have referred before to the natural reluctance of a working man to giving up his means of a livelihood to go into a sanatorium for from three to six months for treatment. The working man, as a rule, has not been able to make provision for the maintenance of his family for such a period, and therefore, almost of necessity, he goes on working, possibly without consulting a medical man, relying on cough mixtures and tonics, until the disease has reached an advanced stage. Even if he has consulted a doctor in the early stages and has been advised to try a change of climate or a sanatorium, of what use is it? Only in an infinitesimal proportion of cases can he act on the advice tendered. A treatment which necessitates his giving up work for months and leaving those dependent upon him unprovided for, not to speak of the cost of maintenance in a sanatorium, is for him out of the question. With tuberculin treatment, however, the case is quite different. There is no necessity for him to give up work; all the time that is required is, say, half an hour once or twice a week to attend at the dispensary for treatment. He does not lose his work,

* 'Treatment of Consumption,' W. Camac Wilkinson, p. 152.

and he is still capable of maintaining himself and family. He will therefore be able to avail himself of the benefit of tuberculin treatment without any disability, and if he submits himself to treatment in the early stage of the disease there is almost a certainty that the disease will be cured or arrested and will never cause him to become incapacitated from earning his living.

If tuberculin treatment can be provided for the "early consumptive" working man, and if he can be cured before the disease incapacitates him for work, the gain to the country, even from a financial aspect alone, would be enormous. It means that hundreds of thousands of persons who become chargeable to the rates through consumption would remain self-supporting. Dr. Newsholme, Medical Officer to the Local Government Board, writes:

"Assuming that it (consumption) causes one fifth of the total disablement at ages 15–65, we can calculate what this meant for the 819,716 members of the Manchester Unity of Oddfellows during the years 1893–97. According to Dr. Watson's tables, these men experienced in these years 4,707,680 weeks of sickness, of which 941,575 must be attributed to consumption. The expense to the Oddfellows of this amount of sick relief, and of the deaths associated with it, must have been at least half a million sterling, and the loss of wages to the men themselves at least double this amount."

(5) "In publications of the National Association for the 'Prevention of Tuberculosis' it is estimated that one-eleventh of the total cost incurred in the relief of pauperism in England and Wales is caused by consumption. The total expenditure in poor-law administration in the year ending Lady Day, 1907, was £14,035,888, so that on this basis considerably over a million sterling is annually spent on paupers who were made such by consumption."*

If, then, tuberculin treatment only secures one tenth the success that is claimed for it, it would be difficult, even from a financial aspect alone, to over-estimate the great benefit that may result to the Borough from its adoption.

As a matter of fact experience shows that patients do come for tuberculin treatment in the early stage of the disease, whilst they are in work. I saw at the Kennington Dispensary a number of men in employment who were undergoing treatment and who were attending the Dispensary once or twice a week—men who in all probability would not have been able to secure sanatorium treatment, and they were strong in the belief that they were daily getting stronger from the treatment.

Here I may mention that I questioned a number of patients at the dispensary, and several, by the way, had already had sanatorium treatment, but had relapsed or been discharged as unsuitable cases, and in each case the patient himself or herself had no doubt whatever that he or she felt stronger and better in health for the treatment. Before I had had an opportunity of observing the employment of tuberculin, I had been favourably impressed by the accounts I had read of it; when, however, I saw the patients being treated and heard their opinion of the result of the treatment, these impressions as to its value were enormously strengthened. I may perhaps mention one case—not that one case of itself proves anything, but it is of interest in showing the occasional effect of tuberculin even when the disease has become advanced. The patient was a man of about 38 or 40, a pianoforte tuner by trade. He told me that when he first came to the dispensary some three months ago he could only manage to crawl there by the aid of a couple of sticks; he was not, he said, able to lace his boots up himself. Before that he had been an inmate of a consumptive hospital for some months, but was discharged as no more could be done for him. He was now to all appearance strong and well set up. He could easily walk three or four miles without becoming tired, and naturally he was rather enthusiastic as regards the effect of the treatment he had undergone. This account reads, I am afraid, almost like an advertisement of a patent medicine; they are, however, the facts as I ascertained them, though I must confess, I should have found them difficult to credit unless I had seen the patient myself. It must not from this be for one minute assumed that tuberculin will usually, or even often, effect such a cure in an advanced case; for tuberculin treatment to prove generally beneficial, one of the principles of success is that the patients must be treated in the early stages of the disease, before there is much wasting and breaking down of lung tissue.

It is important to have a nurse or health visitor attached to the dispensary, whose duty it should be to visit the patients in their homes; by this means frequently some other member of the patient's family is found to be suffering from the initial symptoms of consumption, and he or she can be induced to have the tuberculin treatment, and so the disease may be arrested in its early stage; not

* 'The Prevention of Tuberculosis,' Arthur Newsholme, pp. 16–18.

only is consumption more easily cured in its early stage, but the advantages of so doing are obvious. The work of the nurse attached to the Kennington Dispensary has been found most valuable in this respect, and has resulted in many cases coming for treatment in the early stages who would otherwise have delayed seeking advice until the disease was advanced. Dr. Newsholme (referring to tuberculosis dispensaries), writes:

"A well-organised tuberculosis dispensary becomes a valuable aid in securing more general notification of cases of tuberculosis, and its visitors can not only secure that domestic precautions are taken, but also that the patients are brought into touch with the different forms of domestic aid, or with the sanatorium or hospital treatment that the needs of the individual case indicate."*

It is this method of securing patients for treatment in the early stage, before they have become too advanced to render a cure doubtful or impossible, before, too, they have reached the infectious stage and have become a danger to others, that I believe promises to be the most successful for stamping out consumption.

When a person has reached an advanced stage neither tuberculin nor any other treatment can be relied upon to effect a cure. Tuberculin is, however, even at this stage, probably better than any other treatment; certainly improvement is often seen and the patients often feel better, and, what is a great advantage, tuberculin appears to diminish the cough and expectoration, so that the patient is not such a source of danger to others living with him.

The best method of putting tuberculin treatment at the disposal of inhabitants of the borough is by the provision of a tuberculin dispensary. The power to provide such an institution would probably be found in Sections 131 and 133 of the Public Health Act, 1875, which deals with the provision by a local authority of "hospitals or temporary places for the reception of the sick," and of a "temporary supply of medicine and medical assistance for the poorer inhabitants of their district." A tuberculin dispensary could be provided at little cost; all that would be needed in the way of accommodation would be three large rooms; possibly by way of a trial a fair-sized house in a suitable part of the town could be rented and adapted to the purpose. Little furniture and no expensive appliances would be needed. A medical man, a health visitor, and a caretaker would be necessary, and probably the whole could be managed at a cost of about £600 per annum. If considered desirable, in order to relieve persons attending from any taint of pauperism, a small charge could be made for the treatment, and the local authority would have power under the Act to recover the cost of treatment from those attending. At the same time, as in the case of the Infectious Diseases Hospital, it would not be desirable to make charges in all cases. Even if no attempt is made to make patients defray the cost of treatment, the saving that would be effected by preventing persons becoming chargeable to the rates on account of consumption would, if the tuberculin treatment prove as successful as I anticipate, make a financial return alone equal to that of any trading concern that the borough has yet engaged upon. The benefits to the health of individual inhabitants are so obvious as to need no comment.

* 'Memorandum by the Medical Officer of the Local Government Board on Administrative Measures against Tuberculosis,' 1909, p. 10.

Conclusions.

In conclusion, therefore, I think it inadvisable at present to provide a sanatorium for the treatment of consumptives in this borough. While in no way opposed to sanatorium treatment, my objection is that any scheme dealing comprehensively with the consumptives of the town by means of a sanatorium would be so costly as to be out of the question, and to deal only with a small percentage of the cases by means of a smaller sanatorium scheme would necessitate the outlay of a sum of money that, expended in another direction, would deal, and probably in a more successful manner, with at least twenty times as many patients.

The course I recommend the Council to adopt is to establish a tuberculin dispensary and give it a fair trial for a year. In view of the immense problem with which we have to deal, the sum of £600, which would thus be incurred, would be well worth risking even if our calculations were based on more slender grounds than the successful results of tuberculin treatment published by Dr. Wilkinson.

I do not wish to appear too sanguine, for I do not feel that tuberculin treatment has had a sufficiently long trial for any one to dogmatise upon the subject; and to adopt an over-confident attitude in regard to any new treatment in medicine too often results in disappointment to others and discredit to one's self. All that I feel justified in stating at present is, that, so far as I have had an opportunity of observing, the results obtained from tuberculin treatment appear to be so much superior to those from sanatorium or any other treatment, that it is well worth a trial in this borough. Should it prove a failure it can certainly do no harm, and will have involved the town in practically no expense; moreover, in the want of any better treatment being evolved, there will still be the provision of sanatoria to fall back upon. Should it, however, as I trust, prove a success, it will be a success the magnitude of which will be difficult to estimate.

December 12th, 1910.

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SOME POINTS IN RELATION TO TUBERCULOSIS AND ITS PREVENTION.*

By J. EDWARD SQUIRE, C.B., M.D., F.R.C.P.

TWENTY-TWO years ago (January, 1889) I had the privilege of reading before the Epidemiological Society a paper on the "Prevention of Consumption." At that time few people thought of the disease as preventable; now hardly anyone doubts that it can be prevented. In these twenty-two years much has been added to what we may term the intimate knowledge of tuberculosis, chiefly pathological details; in the essential characteristics of the disease little has been added, nor have we had to modify our views of causation to any important extent. The paper I brought before this Society over twenty years ago would require important additions to bring it up to date, but what is there written would require little modification if it were to be republished to-day.

When we look to what has been accomplished towards the prevention of tuberculosis in the quarter of a century during which we have possessed the knowledge that this scourge of civilised man is a preventable disease, and have been aware of the facts on which prevention must be based, there must be a sense of disappointment, though there is much which justifies hope for the future. We see a diminished death-rate, and, what is important, a *progressive* diminution in the mortality from tuberculosis; but this decline was evident for several decades before the discovery of the *Bacillus tuberculosis* showed us the causation of the disease. This fact indicates that the diminution in the mortality from tuberculosis may be attributed to the general improvement in the sanitary and hygienic surroundings and habits of the people rather than to any special measures directed against

* Delivered before the Royal Society of Medicine.

this disease in particular. Nor has there been any deviation from the steady progressive decline of the tuberculosis death-rate to indicate that our more exact knowledge of causation has resulted in any marked effect on the mortality from the disease. It is time that we inquired seriously into the reasons for this apparent failure of preventive medicine to produce any marked effect on the prevalence of a disease which destroys so many in the prime of life, and which is admittedly preventable.

The question here forces itself upon us whether there has, in fact, been any notable diminution in the *prevalence* of tuberculosis, or whether the lessened death-rate may not be largely attributed to a diminished case mortality; whether it is due to prevention of the disease or to more successful treatment? It is probable that both these factors are concerned in the result. I do not pretend to assess the relative influence of preventive and of clinical medicine in this matter. It is, however, a point which it might be interesting to follow out.

The mere statement of the query, however, suggests at once a matter of great importance, though admittedly of some difficulty. We have the figures of the mortality in relation to the population, but we have no complete knowledge of the case-mortality of tuberculosis. Without complete notification of cases we must remain uncertain whether or no the prevalence of the disease has diminished. True, we have evidence of a saving of life, whether this is due to prevention or to more successful treatment; but no one who knows anything of tuberculosis—its mode of spread and its effects, physical and economic, on the affected individual—will contend that it matters little which of these causes has mainly contributed to the result. Every case is a possible starting-point of other cases, and the cured consumptive has probably been the source of incalculable harm to the community. When also we consider the long duration of the disease, the prolonged incapacity of the invalid, and the economic waste caused by the disease, even when recovery ultimately ensues, no one can allow that a life saved by treatment is equal to one saved by being protected from becoming affected.

Without universal notification throughout the kingdom we cannot know either the incidence or the case-mortality of the disease. The many objections to compulsory notification of tuberculosis cannot be discussed here; they are in great part difficulties in connection with administrative action which should follow notification, rather than objection to notification *per se*. A solution of the problem might be found temporarily in the suggestion I put forward at the annual meeting of the British Medical Association at Sheffield in 1908, that notification of cases of tuberculosis should be made compulsory throughout the kingdom—the results being furnished to the Registrar-General's Department for statistical purposes—but that it should be left to each local authority to decide what, if any, action should be taken in the way of administrative control of the tuberculous. This would allow the various local health authorities the same freedom as at present to formulate regulations for the control of tuberculosis in accordance with the needs and the resources of the locality, whilst insuring a general return of all cases of tuberculosis for statistical investigation.

Next in importance to obtaining all possible information about the disease comes the need to disseminate information amongst the people, without whose intelligent co-operation no scheme of prophylaxis can be efficiently carried out. Much educational work in this direction has been done by the different health authorities throughout the country and by the National Association for the Prevention of Consumption and other forms of tuberculosis. It was perhaps inevitable that the educational propaganda should at first have resulted in exaggerated alarm as to the infectiveness of tuberculosis; possibly it is necessary to frighten some people before they can be induced to take precautions. This alarm, however, tends to be prejudicial to those affected with tuberculosis, who may be looked on by the half-instructed as dangers to society. The "big poster" campaign which has just been inaugurated by the Special Appeal Committee of the National Association may insure a wider appreciation of the nation's losses from tuberculosis, but it will undoubtedly increase the difficulty—already sufficiently serious—which the "cured" consumptive finds in obtaining employment, and may in consequence tend to prevent patients seeking early treatment for fear of becoming branded as consumptives. It is, perhaps, time that we insisted less on the infectiveness of the consumptive and more on the danger of the conditions which produce susceptibility — that we

talked less about spitting and more about the hygiene of the home.

Beyond the educational work, health authorities and preventive associations appear to have focussed their activities on the establishment of sanatoria for the curative treatment of early cases. There is, undoubtedly, value in sanatoria. These institutions may be utilised as schools to teach consumptives how best to regulate their lives so that they may obtain the fullest advantage from treatment at home, and may perhaps then continue at work with the least risk of increasing the activity of the disease or of being dangerous to others. When so utilised, a constant succession of patients enters for a short stay, and results are not to be measured by the immediate effect on the patient's condition.

The usual aim of the sanatorium is, however, the cure of consumptives, so that they cease to be centres of possible infection, and are rendered fit to follow their employment with advantage to themselves and without danger to others. Towards this objective—which is an important, and perhaps essential, feature in an efficient scheme of prophylaxis—such sanatoria achieve little unless cases are admitted in the early stage of the disease and can be retained sufficiently long to allow of arrest being effected. There has been lately a considerable amount of criticism as to the efficacy of the sanatorium treatment of consumptives, founded on the statistics furnished in the reports of such institutions. The comparative failure of treatment which these statistics in some measure disclose is due partly to the large proportion of cases admitted with comparatively advanced disease, and partly to the difficulty experienced in keeping the patients sufficiently long under treatment to effect a lasting cure. Both these matters deserve comment. It is undoubtedly a fact that a majority of cases are not admitted for treatment sufficiently early: the reports of most sanatoria make complaint of this.

The chief reasons for this are—(1) The disease is not recognised in its early stage; (2) when it is recognised, the patient does not at once commence systematic treatment.

The medical practitioner cannot be held responsible when the non-recognition of early tuberculosis is due to the patient failing to seek advice; but in the case of a disease of such importance to the public health, preventive medicine cannot be content to claim a like freedom from responsibility. Realising, as every medical man should now do, how closely the success of treatment depends on the stage of the disease at which this is commenced, it surely becomes a duty to endeavour to find out the early cases and to persuade these to commence treatment without delay. Some inquiries by the National Association for the Prevention of Consumption, with which I have been associated, have shown how little is done in London to search out early cases.

During the last five years that I held the appointment of physician to the St. Marylebone General Dispensary (1902–7), the resident medical officer, Dr. Hulbert, made for me close inquiries as to all members of the families and fellow-workers of all the tuberculous patients, and persuaded a large proportion of these to come to the dispensary to be examined by me. A short report of this work may be found in the 'Transactions of the International Congress on Tuberculosis in Washington,' 1908 (vol. iii). Such examination of "contacts" is almost an essential if we aim at getting early cases amongst the poor for treatment. It is carried out by the tuberculosis dispensaries—such as have been established in Edinburgh, in Paddington, and quite recently in Marylebone. The systematic examination of the children in the elementary and secondary schools should also be useful in detecting early tuberculosis, but it would be well to have some expert to whom suspicious cases could be referred for examination. In a few of the Metropolitan boroughs a partial examination of contacts is carried out, only those who complain of illness or appear to be ill being examined. This is, in my experience, of slight value in detecting *early* cases.

Clearly one urgent requirement for the prevention of tuberculosis is the visitation of the homes of the consumptive with the view of obtaining an examination of contacts; such examination should be carried out by experts in chest examination, for the experience of all medical officers of sanatoria shows that the evidences of early tuberculosis in the lungs are easily overlooked. Of the delay in commencing treatment, which is due to failure to detect the disease when the patient first seeks advice, I shall say little. The wider one's experience of the clinical examination of the chest in health and disease, the less is one

disposed to be surprised that the evidences of early tuberculosis are so often unrecognised. One is, however, I think entitled to regret that the assistance of the clinical expert is now so often replaced by some form of tuberculin test, or by examining the sputum for bacilli which are not likely to be present until softening has commenced.

The delay in entering the sanatorium when the disease has been recognised may be due—(1) To financial considerations—the loss of earnings when the patient has others dependent on him, or, in some cases, the expense of paying the sanatorium fees; (2) the large numbers seeking admission to special hospitals and free sanatoria and consequent long waiting for vacancies; (3) the fear of being labelled as consumptive and consequent difficulty of obtaining employment after discharge. These matters must be considered and provided against if we wish to grapple seriously with the tuberculosis problem. But the treatment of early cases, though a most important factor in prevention, does not constitute the whole requirement. We need homes of refuge for the advanced consumptive of restricted means; a place where he can be separated from close and continued association with susceptible individuals, whose susceptibility is progressively increased by the increasing poverty produced by maintaining an invalid; a place where his wants can be adequately attended to, where his remaining days are made less dreary, where his relatives and friends may visit him, and, above all, a home where he may rest without being branded as a pauper living on charity. There should be no more disgrace attaching to the acceptance of a home and attention when disabled by illness than there is in accepting a pension when disabled by age. Such homes for advanced consumptives would set free many beds in the special hospitals, which would then be available for early cases, and their cost would probably be covered by what they would save the poor-rates. Labour colonies for chronic cases fit for restricted employment are needed, and might be made self-supporting. We need also provision for the support of those dependent on the consumptive whilst he is in the sanatorium—for want of which many early consumptives cannot give up work to undergo proper treatment, and many that enter the sanatorium cannot afford to remain long enough for cure. We need also some employment bureau for obtaining work for the "cured" consumptive on his discharge from the sanatorium, for many discharged patients break down under the hardships entailed by the unsuccessful search for employment.

Obviously the complete scheme for dealing with the tuberculosis problem is almost beyond the scope and ability of individual charities, and should be under State organisation and control. Such a body as the National Association for the Prevention of Tuberculosis might do much to convince the Government that a scheme can be organised and worked by co-ordinating existing agencies throughout the country. A suggestion of this nature has been laid before the Council of the Association, which may, I hope, bear fruit. I would urge on all health authorities, hospitals, charitable and philanthropic agencies which concern themselves with the comsumptive, to do all they can to further any scheme which seems to promise success in co-ordinating their efforts towards the prevention of this disease. But I must not detain you longer on general considerations of prophylactic policy. I am not now attempting to work out the details of a comprehensive scheme for stamping out tuberculosis; this is not a task which any wise man would attempt single-handed, for there are too many interests involved. Let me rather direct your attention to a few special points on which my experience has led me to certain conclusions. There is a development of modern medicine which seems to me a rather unfortunate phase—namely, the undue reliance on so-called "tests" for specific diseases. I have elsewhere* expressed my doubts as to the reliability of the tuberculin tests in the human subject. Inoculation with tuberculin may in some cases be a useful addition to clinical examination, but surely few would agree that it can usefully replace the usual clinical methods.

One result of this reliance on the tuberculin test has been to produce a very exaggerated estimate of the proportion of the population which is affected with tuberculosis. It may be true that some 80 per cent. of the persons tested will react to the tuberculin test, but no one will agree that this proportion of the population is *suffering* from tuberculosis. Many who react never present any indications of the disease, or suffer any inconvenience or disability attributable to the implantation of tuberculosis. Such an attack of tuberculosis

* 'Lancet,' August 6th, 1910, p. 373.

may well be disregarded, unless it can be shown that the affected individual is a danger to others.

This brings us to a matter which must be considered in connection with the prevention of tuberculosis which has not as yet received much attention, and is perhaps not yet sufficiently realised by the majority of workers in public health matters. We are now fully cognizant of the existence of so-called "typhoid carriers"—individuals who, whilst apparently in good health, yet harbour in their bodies the causative micro-organisms of enteric fever, which they throw off in the excretions, and thus may become the unsuspected cause of the disease in others. We are fully aware that during an epidemic of diphtheria there will be many persons who apparently escape the disease themselves who yet have the bacilli in the secretion of the mouth and fauces, which, expelled by cough, may infect others. Every hospital physician will have noticed the many cases of follicular tonsillitis which come to the out-patient room when there is an outbreak of diphtheria in the neighbourhood, and most of them will have found the micro-organisms of diphtheria in swabs taken from the throats of these apparently harmless cases. I suggest to your notice that there are also tuberculosis carriers—individuals who, although they may not be in robust health, are yet not suspected of being tuberculous, but who yet are excreting tubercle bacilli and possibly infecting others. There may even be individuals who remain in good health who may nevertheless be carriers and disseminators of tubercle bacilli.

The "carrier" is evidently a person whose specific protective processes are not sufficiently active to immediately destroy the particular infective micro-organisms which may enter the body, but who is, nevertheless, so far immune as to escape illness from the infection. In this sense there may be—and I think there are—tuberculosis carriers, but there are also many cases where active tuberculosis is present but unsuspected.

To what extent the insusceptible members of a consumptive's household or of the staff of a hospital or sanatorium for consumptives may harbour tubercle bacilli and distribute them I am not prepared to say, but I think the question is one which is worth further investigation.

Kayserling, of Berlin, in referring to the subject of tuberculosis carriers,* suggests that they give off no bacilli, but harbour them in the body for months or years, until some accident or illness renders the soil fertile and they become actively tuberculous and thus centres of infection. On the other hand, tubercle bacilli have been found in the mucus from the mouths of those in constant association with consumptives, and it is conceivable that these might be transferred—by kissing, for example—to others who might be susceptible to infection.

In connection with the question of "carriers," it is interesting to bear in mind the various observations which have led to the belief in latent tuberculosis, the main fact being that infective (tuberculous) material may be present in the body without any pathological changes, either macroscopical or microscopical, being discoverable. The infectiveness of the material is established by the inoculation of animals. Amongst others, Prof. Francis Harbitz,† for example, in the records of the Pathological Anatomy Institute of Christiania, pointed out that a large proportion of the children examined were in this sense the subjects of latent tuberculosis. In most of the cases it was the lymphatic glands which were affected, but a similar condition may occur in other organs and tissues, even in the lungs. It may be, as has been suggested, that the bacilli in these centres of latent tuberculosis may have their virulence diminished; it may also be that, though virulent, their power for harm is counterbalanced either by the activity of protective agencies in the individual or because the bacilli are isolated and cut off from communication with the rest of the body. In any case, if this infective material were to gain entrance into some other individual whose tissues furnished a fertile soil, disease might result. We do not know how long tubercle bacilli may remain "latent"; Prof. Harbitz suggests that it is only a few months, or at most a couple of years, but it seems long enough to offer sufficient possibilities of danger to others. Apart, however, from this conjecture, there is no doubt that many cases of tuberculosis of the lungs remain undetected and become sources of danger because their infectiveness is unsuspected.

* 'Tuberculosis,' June, 1906.

† 'Untersuchungen über die Häufigkeit, Lokalisation und Ausbreitungswege der Tuberculose,' von Francis Harbitz. Christiania: A. W. Broggers, 1905.

I have several times drawn attention* to the cases of chronic bronchitis in elderly persons in whom tuberculosis has supervened on the bronchitis, and, causing no new symptoms, but merely an exacerbation of existing discomforts, remains undetected, though examination of the expectoration would demonstrate the presence of tubercle bacilli.

It is also important to recognise the fact, insufficiently appreciated, that many cases of "cured" or arrested tuberculosis of the lungs continue for months, or even years, to cough up—not continuously, but from time to time—phlegm containing bacilli. These persons having been pronounced free from active disease, and being free from all feeling of ill-health, have relinquished the preventive precautions which they exercised during their illness, and become therefore unwitting dangers to others.

The foregoing considerations suggest the primary importance from the point of view of prevention of directing our attention to combating susceptibility, for possible sources of infection are innumerable, and can hardly be avoided since they may be unsuspected. Most of us are daily in contact with such sources of infection and probably receive bacilli into our bodies, but these produce no ill-effects whilst our protective agencies are vigorous. The healthy person has nothing to fear from infection except from the rare accident of an overwhelming dose. Thus, if we can retain the condition of resistance which appears to be natural to the healthy human animal, we need not shun the consumptive. The line of defence against tuberculosis which seems to promise the greatest measure of success is that which aims at increasing the resisting power of the individual who is exposed to infection. We are all agreed that the conditions which tend to break down the resisting power of the individual are to be found most prevalent in the homes of the poor. In these homes also we find a large proportion of the dangerous consumptives—the sources of infection. It is, then, in the homes of the poor that the war against tuberculosis must be fought out.

There is a natural tendency amongst officials of all kinds to require some numerical expression of the extent of a need before they can agree to consider the possibility of meeting it by regulations or enactments. Thus we require statistics showing the extent of the infectivity of tuberculosis, the proportion of contacts who become tuberculous, and the effect of inherited susceptibility as shown by a relatively larger proportion of infected individuals amongst the contacts belonging to the patient's family as compared with those not related to the consumptive. Reliable statistics on these subjects are, however, difficult to obtain, and we are still largely dependent on general experience unsupported by exact figures.

In a recent communication to the 'Lancet'* the Right Hon. R. Farquharson, M.D., expresses grave doubt "as to the really dangerous infection of consumption." Of course much depends upon what is considered to be "dangerous infection." Since Dr. Farquharson proposes, as necessary prophylactic measures, the prevention of spitting, the disinfection of sputa, and the thorough cooking of meat and boiling of milk, it seems that he does believe in the communicability of the disease through inhalation and ingestion. It is true that over-insistence on the infectious nature of tuberculosis has caused the partially instructed to compare it with the well-known infectious diseases, such as measles and scarlet fever, whereas there is a very wide difference in the infectiveness of tuberculosis and that of the exanthemata. It would, perhaps, be more correct to say that there is a great difference in the susceptibility of the human subject to suffer from the infection of tuberculosis on the one hand and of measles and other exanthems on the other. Yet every one who has had any large experience of tuberculosis knows that many consumptives can trace the commencement of their illness to close association with an advanced case of consumption. Cases frequently present themselves where it is impossible to doubt that infection has spread from patient to nurse, from mother to daughter or *vice versâ*, from husband to wife, or from a consumptive clerk or workman to another working in the same room. I could give examples of all of these from my own experience if it were necessary to insist on what is the common experience of all who are intimately engaged in the treatment of consumptives.

In a record of the resident staff of the Mount Vernon Consumption Hospital, which I recently brought forward before the Medical Section of

* 'International Clinics,' iv, 16th ser., 1906, p. 90.

* 'Lancet,' July 23rd, 1910, p. 244.

this Society, I showed that during the past fifteen years about 2½ per cent. of the nurses and servants had become affected with tuberculosis whilst in residence at the hospital. These nurses and maids were unmistakably "contacts," but they were selected individuals living under conditions specially devised to prevent infection, and it is impossible to affirm that all became infected from the patients. Yet this report undoubtedly suggests that there is some risk of infection—small though it be—from continuous and prolonged association with consumptives even under favourable conditions. The influence of family predisposition or susceptibility showed itself in the fact that the proportion of cases of tuberculosis amongst the nurses and maids who gave a history of other cases of the disease in their families was just twice as great as amongst those with no family history of tuberculosis. The numbers were too small to be of much value, but we may find other evidence that some families are more prone to tuberculosis than others. For example, I have examined during the past year 900 death-claims of the Pearl Life Assurance Company (of which I am the consulting medical officer), and here also I find that the deaths from tuberculosis are more numerous amongst those whose family histories record other cases of the disease than amongst those with no such record. The figures are as follows: Total number of claims examined, 901; total deaths from tuberculosis, 140, or 15·53 per cent. Of these 901 claims, 53 gave a family history of tuberculosis—5·88 per cent.; of 848 with no family history of tuberculosis, 124 died of tuberculosis—14·62 per cent.; of 53 with other cases of tuberculosis in the family, 16 died of tuberculosis—30·18 per cent., again showing an incidence twice as great amongst those with a family history of tuberculosis.

We are still without a numerical expression of the risk of infection under the conditions obtaining in the homes of the poor consumptive; but we shall, I think, not be far from the probabilities if we first double the percentage incidence of the hospital staff to represent the greater susceptibility of the relatives of consumptives, and then double this again to represent the extent of the increased risk due to absence of proper preventive precautions. This will give us 10 per cent. as representing the risk of infection; or in other words, we should expect to find ten persons affected with demonstrable tuberculosis in every 100 contacts examined. This is a sufficiently large proportion to justify the examination of all who are in close association with consumptives in the poorer homes. I find, however, in the report of the Paddington Dispensary for the Prevention of Consumption, that definite signs of pulmonary tuberculosis were found in over 20 per cent. of the contacts examined.

If we agree that the fight must be at close quarters with the disease in the places where it is largely bred and spread—*i. e.* in the home of the poor consumptive—we must first consider how we are to get into these homes. The poor are sadly over-visited and get accustomed to the intrusion of uninvited strangers, but they often resent the visit of officials who may claim admission as a right. For this reason there may be an advantage in making use of voluntary agencies to supplement the official sanitary inspectors or health visitors. The more complete is our knowledge of the number and location of the consumptives in any district, the better are we able to combat the spread of the disease; and information may be obtained by voluntary agents—such, for example, as district nurses, district visitors and church workers, local health societies, etc.—from sources other than those appearing in the official returns of the medical officer of health. When the home is visited advice should be given as to ventilation, cleanliness, the disposal of sputa and the prevention of infection generally. All persons in close and constant communication with the consumptive should be examined occasionally to insure that they remain free from the disease, or that if they become infected this may be recognised and treatment commenced before the disease has become too far advanced. My experience in the examination of such contacts whilst engaged in this work under the Marylebone General Dispensary, as well as a large experience in the examination of children in the elementary schools, has convinced me that it is insufficient to examine only those contacts who give evidence of ill-health, as is done by the few metropolitan boroughs which take any steps towards the examination of tuberculosis contacts. I may also again insist that to test contacts with tuberculin alone is unreliable, and to wait for the detection of tubercle bacilli in the sputum is to

miss most of the early cases and some of the advanced ones.

The organisation for home visitation should also be in a position to offer any aid which may be necessary, such as extra nourishment, extra coverings, etc., or admission orders to a hospital or sanatorium or a convalescent home. It should, therefore, be in touch with the Charity Organisation Society and all the various philanthropic agencies of the district. The machinery of the medical officer of health's department is hardly sufficiently complete for this, so he seeks the assistance of the local health society or association of public welfare where such exists. It has been suggested that hospitals could do this visiting and furnish the help that is required for the patients attending the institution; and in a few instances this has been attempted. There are obvious difficulties and disadvantages in this, at all events in London; for the patients of any hospital are drawn from too extended an area to be covered by any one agency, and patients keep moving from one dwelling to another and change from one hospital to another.

The organisation for home-visiting must be founded on the basis of locality, and to be efficient as a prophylactic agency it should be in touch with all the tuberculous cases in its district. In London the division into metropolitan boroughs provides a convenient basis for such organisation, which would be in close association with the medical officer of health of the borough, and should aim at getting to know of every case of tuberculosis in the borough, and the condition of the home, etc., thus being able to offer help, besides supervising the hygienic conditions of the dwelling.

Whatever organisation undertakes the visiting of the homes, whether it be a hospital or a special anti-tuberculosis dispensary or the local health authority, its value as a preventive agent depends largely upon its visiting, supervising, and to some extent controlling as large a proportion as possible of the infected homes within the district. The object of the visiting authority, whatever its nature, should be to visit the homes of *all* consumptives living within the area covered by its activities. If prevention of spread of the disease is the objective, this agency cannot afford to have any infected home within its district which is not under its inspection. It is obviously not essential that the visiting authority should itself undertake the medical examination of contacts, nor the actual treatment of the consumptive individual, but it must be in close association and communication with the agencies and individuals who examine and treat. It should also be closely associated with the medical officer of health, to whom reports should be furnished of all cases. The medical officer of health (or rather the health authority which he represents) should have the right of entry for its officials, or persons delegated by it for this duty, to all dwelling and working rooms occupied or used by consumptives. He should have the power to remove a dangerous consumptive, and to retain in any institution any consumptive who, in consequence of his habits or of the conditions to which he proposed to return, was considered likely to be dangerous if discharged.

I am inclined to think that for London at least, and probably for all large towns, special anti-tuberculosis dispensaries, each responsible for a definite district, would at present best serve the purpose of home visitation. Such an institution would also co-ordinate all the agencies in the district for providing aid to the consumptive, and getting admission to suitable institutions for the patient and contacts who seemed in danger of becoming affected. It would also provide for the examination of contacts and the treatment as out-patients of the affected. It is, however, impossible, even if it is desirable, to oblige all consumptives within the area to attend the dispensary as patients, since the consumptive, like any other sick person, has the right to choose where and by whom he will be treated. Nevertheless, the dispensary should be responsible for the supervision of all the centres of infection within its district; otherwise it is only partially protective, and is defective as a preventive agency for the district. It seems to me, therefore, essential that a local anti-tuberculosis dispensary should make provision for the visitation of the homes and workplaces, not only of its own patients, but also those of all consumptives living and working within the area of its control, no matter by whom the patient is being treated, unless such visitation is specifically undertaken by the doctor or institution under whose treatment the patient may be for the time being.

Many private practitioners working amongst the

poor, and probably all hospital physicians, would gladly avail themselves of the dispensary organisation for the hygienic control of the consumptive's dwelling, so long as they knew that there would be no attempt made to take the patient out of their hands. No doubt, also, practitioners or institutions might be glad to hand over certain of their consumptive patients to the dispensary for treatment as well as for general supervision.

I believe that this view of the duties of an anti-tuberculosis dispensary is not fully in accordance with the practice of such dispensaries as are at present in existence, but I feel sure it will commend itself to those who are interested in the broad question of prevention.

At present we must look to private enterprise to found and maintain such preventive dispensaries, but there is much to be said in favour of the local health authorities eventually undertaking the control of such institutions, and providing, maintaining, and working the anti-tuberculosis dispensary as part of the sanitary organisation of the district for the health of which these local authorities are responsible.

If the attempt of individual hospitals to deal with the problem is unsatisfactory because they can only visit patients attending that institution, and because their patients are so widely scattered, anti-tuberculosis dispensaries will, in my opinion, fall short of the requirements for efficient preventive organisations if they only visit the homes of a proportion of the poor consumptives in their neighbourhood.

The medical inspection of children in our elementary and secondary schools offers great possibilities in the direction of improving the hygiene of the homes of the poorer members of the community, and in this way helping materially in the fight against tuberculosis. It is not in the personal examination of the children alone that medical inspection fulfils its greatest good. Even a superficial examination gives abundant evidence that much of the ill-health which prevents children from obtaining full benefit from instruction in school is due to home conditions. From the point of view both of treatment of these children and of prevention of such hindrances to education, it is essential that attention should be paid to the improvement of the causative conditions. The school nurses are instructed to visit the homes of children who are reported to be verminous, and in certain cases of ill-health, in order to impress on the parents the necessity for treatment. This home visitation might, with advantage, be greatly extended so as to include visits to instruct the parents in essential health requirements in their homes. Where the faulty condition of the child's home is found to be due to neglect on the part of the parent, there should be power to compel obedience to directions or to enforce penalties for disobedience. If the State undertakes the education of the child, it has a right to insist on the parents doing their part, and, so far as in them lies, keeping the child fit to receive the instruction given.

At the same time the school ought to give instruction to the child in the essentials of hygiene; if each child has a right to be taught sufficient to enable it to earn its living in after-life, it surely should also be taught how to safeguard its health, without which remunerative work is almost impossible. To the argument that there is no room for this subject in the already crowded time-table of our schools, I would suggest that, after all, hygiene is of more practical utility than botany (for example), and that State-provided education should in its scheme of instruction bear in mind the ultimate good of the nation, and consider the welfare of the individual mainly in relation to the community of which he is a member. It is not enough to teach the child how to earn money for himself; education should also aim at making him a good citizen, and after all, a healthy population is one of the valuable assets of a nation.

The inspection of school children is, of course useful in making possible the detection of unsuspected cases of consumption, and thus in some cases insuring early treatment. It is also valuable in bringing under notice those children who, though not tuberculous, are yet by reason of general debility specially susceptible to infection. It is, however, not sufficient to recognise predisposition; measures should be taken to counteract or remove this predisposition. Open-air schools, country and seaside schools for town children, and convalescent homes are valuable in this connection. It seems to me that a good deal more might be done in the direction of boarding-out town children in country villages where they could attend the local school, and thus have a few weeks or months in the country without interruption to their education.

We must, however, bear in mind that the hygienic condition of the cottage is not always satisfactory, and a knowledge of the essentials of healthy living should be taught to the villager as well as to the town dweller.

We must always be careful about sending consumptives to the villages, for, once introduced, the disease seems difficult to get rid of. I have recently stayed at a small village of 330 inhabitants,

in a most healthy part of England, where one would hardly expect to find any cases of consumption. I am informed, however, that out of twenty-one deaths which have occurred in the last four years, seven have been from consumption. The schoolmaster, who has lived in the village for thirty years, says that he believes that there is not a single house in the village that has not had at least one death from consumption in it since he came to the place. In one house there have been six such deaths. It is difficult to explain how the disease became so persistently prevalent in this small community in so favourable a locality. Can it be that the cottages are all infected, or can we accept the suggestion that infection comes from the droppings of cows, which are never removed from the village street, and which may be derived from tuberculous cattle? I cannot express an opinion on the possibility of this latter theory. I believe that bacilli have been found in the fæces of tuberculous cows, and it is easy to conceive that, if present, they might be disseminated from dried excreta, as from dried sputa; on the other hand, we do not generally associate tuberculosis of the lungs with infection from bovine sources. The matter may, however, merit further attention and investigation, as also does the agency of flies, and possibly fleas, in the transmission of infection.

The possibility that the source of infection is to be found in the cottages suggests the necessity of efficient disinfection of rooms occupied by consumptives who are expectorating sputum containing bacilli. This is, I believe, generally carried out in London when a room is vacated by an advanced consumptive; in one metropolitan borough disinfection is carried out periodically whilst the room is occupied by the patient. This latter practice seems desirable, and the clothing and bedding should also be disinfected. Disinfection, to be effective, should be carried out or supervised by a competent individual. I was told some little time ago of a consumptive in a country district who for some time before his death was unable to go upstairs to his bedroom, and died in the sitting-room downstairs, where he had spent the last few weeks of his life. Disinfection was carried out by the local "road-man," who burned a sulphur candle in the old man's bedroom, which he had not occupied. I cannot vouch for the accuracy of this tale, which, however, is at least *ben trovato*.

I am afraid that I have occupied a great deal of your time without having added anything to your knowledge of the subject of tuberculosis and its prevention. I have attempted to draw your attention to certain aspects of the problem rather than to dogmatise, or even to explain the points I have raised. The expression of one's thoughts challenges thought in others, and may thus lead to valuable results.

December 19th, 1910.

A CLINICAL LECTURE

ON

LYMPHADENITIS.

Delivered at the Hospital for Sick Children, Great Ormond Street.

By H. A. T. FAIRBANK, M.S., F.R.C.S.,
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GENTLEMEN,—This afternoon I propose to talk about the lymphatic glandular enlargements in children and infants. The classification I have selected is the following:

(1) *Lymphadenitis: Acute* (simple and suppurative).
Glandular fever.
Chronic: (*a*) Simple.
(*b*) Tubercular.
(*c*) Syphilitic.

(2) *General enlargements* probably of infective origin.
Lymphadenoma, Hodgkin's disease.
Lymphatism.
Still's disease.
Tuberculosis.
Rickets.
Leukæmia.

(3) *New growths:* Lympho-sarcoma (primary).
Secondary sarcoma.

We naturally begin with the ordinary inflammation of lymphatic glands, lymphadenitis, which usually affects one particular set of glands and one only. In the neck, however, we commonly meet with bilateral infections, the same set of glands being affected on either side of the neck, because the infection so frequently arises from the throat, where inflammations are almost necessarily bilateral. This localisation of the glandular involvement is opposed to the more general diseases, in which there is a tendency, if not at first, at any rate in the later stages, for all the glands throughout the body to become enlarged. In Hodgkin's disease, for instance, you know that the lymph-glands all over the body, not only in the neck, but also in the chest and abdomen, become enlarged. There are various other conditions in which general glandular enlargement is the rule.

Lymphadenitis is usually divided into *acute* and *subacute*, and *chronic*. The acute inflammation of glands may be *simple*, or *suppurative*, and it is usually the case that a gland which is going on to suppuration gives all the appearances of simple enlargement to begin with, and only shows a tendency to abscess formation later on. We can say that the tendency of "acute glands" is to end in one of three ways: in resolution, in suppuration, or in the glands becoming chronic, and remaining enlarged. The sites for enlarged glands which we have to keep in mind are the neck, the axilla, and the inguinal region. In the abdomen and chest, and so on, you get acute glands in connection with abdominal and lung conditions, but we do not trouble about them from the surgical point of view. The cause of the acute lymphadenitis is inflammation in the region drained by the special set of glands which are enlarged. In the case of the neck we get the inflammations of the throat, the teeth, the ear, and so on.

There is, to begin with, a large group of cases in which the local lymphadenitis is simply one result of a general disease; I refer to the exanthematous fevers. In all these cases there is sore throat, and the glandular enlargement is, as a rule, of no special surgical importance, for when the throat condition clears up, the glands also disappear. Unfortunately this is not invariably the case, for the glands may suppurate.

Of the acute fevers in which we get glandular enlargement, scarlet fever is perhaps the one where the lymphadenitis is most likely to go on to abscess formation, particularly when the sore throat is of a severe type.

We may next refer to the *acute glands* secondary to acute tonsillitis, acute pharyngitis, rhinitis, etc., and unconnected with any general disease. It is hardly necessary to enter into the subject of the bacteriology of sore throats; suffice it to say that the number and variety of micro-organisms which are isolated, and are, apparently, the cause of the sore throat in any particular case, seems to increase every year. I need not do more than name such sources of infection as necrosis of the jaw, otitis media and its complications, sores on the scalp, and pediculi capitis; with the latter the glandular inflammation is not uncommonly associated with temporary torticollis.

With regard to the groin, the chief sources of infection are sores about the feet, such as a sore heel rubbed by the shoe, and a septic blister on heel or toe.

Inflammations about the external genitalia is another common cause.

In the case of glands in the axilla, of course, sores and whitlows about the fingers and cracked knuckles which become septic are common enough causes. As regards the organisms which are the cause of the inflammation, they usually belong to one of the ordinary pyogenic groups, such as staphylococci and streptococci. But it is not necessary for the germs themselves to reach the glands at all: the absorption of their toxins is quite sufficient to produce the inflammation. Perhaps I may be allowed to turn back for a moment to one of the acute fevers, namely, erysipelas. Erysipelas is always associated with enlargement of glands, usually of a simple non-suppurative nature. But the interesting thing about it is, that very often the glands become enlarged and very tender before the characteristic erysipelatous rash makes its appearance. There is one particular instance which I met with recently at the Highgate Convalescent Home, where a double congenital dislocation, which I had treated and sent there, developed tender glands in both groins. The matron very naturally thought there was something wrong with the hips after the Lorenz's operation. The child had a few little sores about the labia, and I thought it was just an infection from those; but when I went a day or two later I found there was a definite erysipelatous rash, which had spread from these sores on the vulva. For a day or two beforehand the glands were markedly tender, but not very much enlarged. They made their appearance certainly two or three days before the characteristic rash, and that is a point to remember.

With regard to the pathology of these glands, what happens is that they become hyperæmic and there is a proliferation of cells in the glands. This includes not only the cells like leucocytes, the ordinary round cells of lymphatic glands, but also the endothelial cells of the lymph-sinuses or channels. The final result depends upon the virulence of the infection and the degree of resistance of the individual, as is the case with infection elsewhere. There always seems to be a greater tendency in the younger children to abscess forma-

tion than in the older. In connection with erysipelas there is a further point to mention, and that is you sometimes get a streptococcal abscess in the neck or elsewhere which, when opened, gives rise to erysipelas of the skin. I have seen this happen more than once: An acute abscess is opened in the neck; bacteriological examination of the pus reveals streptococci only, and within a day or so the characteristic erysipelas rash appears round the wound. Undoubtedly the same germ is responsible both for the abscess, which probably has its origin in the glands, and also for the cutaneous erysipelas.

The signs of acute lymphadenitis one need hardly refer to at all. The glands may be separated and discrete, or they may be united into a solid mass by peri-adenitis. In the latter case the matting is suggestive of suppuration, though by no means absolutely diagnostic of that condition. Redness of the skin, and, of course, fluctuation, are certain signs of suppuration.

As regards treatment, we must treat, first, the source of the infection and if possible remove it, and secondly, the glands themselves. To achieve the first object we order gargles, etc., for the throat, we remove septic teeth, we endeavour to clean septic sores of the skin, and so on. For the glands themselves hot fomentations are the best applications. If you prefer ice-bags you must use them in the early stages, and not in the youngest children. Personally I believe fomentations are more comforting and more useful, while they are within the reach of the humblest patient. If fluctuation occurs, of course you must open the abscess. Sometimes considerable difficulty is experienced in deciding whether an incision should be made or not. I would draw special attention to those cases, which commonly occur amongst the youngest children, in which there are masses of matted glands behind the angles of the jaw, usually more marked on one side than the other, perhaps without any discoverable cause in the throat, which persist for a long time, but become if anything more swollen and in which the glands are obviously going to slowly break down. In such cases it is not wise to operate early, for if you do you will probably miss the pus. What happens is that a tiny point of pus forms in one or more glands, and this little abscess increases in size very slowly until it reaches the periglandular lymph-space, which is then distended with pus, till eventually the capsule gives way and a more diffuse abscess is formed. It is during the early stages, when the pus is entirely within the gland, that we may be tempted to explore and fail to strike the pus. When the whole capsule of the gland is distended with pus and when the latter has given way there should be no difficulty, in most cases, in localising the pus. Fomentations should be continued till the situation of the pus is beyond doubt.

I do not believe in using iodine and other similar local applications to the skin over acutely inflamed glands. Possibly unguentum hydrargyri may hasten resolution when the glands are obviously not going to suppurate.

Please do not understand me to say I would wait and watch in all cases of suppuration in the neck. In many an abscess can be localised early and should be opened. Again, when the suppuration is obviously of the cellular tissues—Ludwig's angina—of course you must incise early, and insert sinus forceps, and open up the suppurating cavity. It is obvious that the inflammation is in the cellular tissue and not only in the glands. The size of the incision should be what seems reasonable, keeping in mind the anatomical relations of the parts and the after appearance. In the neck you would not make a larger incision than you felt was absolutely necessary. There is one other disease which comes under this heading of acute lymphadenitis, and that is "glandular fever." Certain cases occur in which you get enlargement of glands, particularly in the neck, but sometimes more or less generally throughout the body and including the spleen, associated with high fever, and without any sufficient cause in the way of sore throat. I have recently seen a case of this nature where a child æt. 3 years had a temperature for ten days—for four consecutive nights the temperature was 104° F., dropping to 101° F. in the morning—with a large mass of glands on one side of the neck, particularly under and behind the sterno-mastoid. They were hard and slightly matted. The throat was perfectly clean. If there was any focus, such as an ill-drained supra-tonsillar fossa or anything of that sort, to account for the infection, there was no inflammation around it. He was treated by local applications to the throat and fomentations to the neck. Quinine was given, and the bowels were kept open. After those four nights the temperature began to drop, and in ten days from the onset the

child was practically well. But the glands are still enlarged, several weeks after the acute illness, though they have decreased in size considerably. They did not suppurate, but for some days we—the child's medical attendant and myself—were in doubt as to their fate. In glandular fever suppuration is said not to occur. One cannot help thinking that in all probability there is always a focus of infection in the pharynx or elsewhere in these cases, though none is discoverable. In other words, glandular fever is probably only acute secondary lymphadenitis.

We pass on now to the simple chronic and subacute inflammations of glands, and here I shall particularly refer to the neck glands. You will no doubt remember that it is quite a common thing to find a certain amount of enlargement of the glands persists after an acute attack of lymphadenitis. Such a chronic enlargement of the glands is more likely to ensue if they have been subjected to repeated attacks of acute inflammation. On the other hand the infection may be continuous, or at any rate recurrent, and give rise to chronic lymphadenitis without any obvious acute exacerbations in the glands. In children we meet with numerous instances of such chronic glands in the neck, a very large number of which are simple, that is to say, non-tubercular, and we usually find that the history given is that of recurrent attacks of sore throat or colds in the head. There may, of course, be otorrhœa, or some other obvious cause to account for the infection. The glands are usually not very much enlarged, and they are inclined to be soft rather than hard. They are not matted, but are discrete, and they are particularly found in the upper part of the neck in front of the sterno-mastoid, *i. e.* affecting the group of glands which drain the throat, nose, etc. Almost every case which comes complaining of the throat or is sent up here to have the tonsils removed has a few chronic enlarged glands in the neck. These are not tubercular, and when the throat is treated they disappear more or less rapidly according to the duration of the enlargement. The interest in them lies in the diagnosis between these chronic simple glands and tubercular glands of the neck. The points of differentiation are, first, that there is a fairly obvious cause, in the simple variety, in the way of tonsillar enlargements, adenoids, a history of recurrent attacks of tonsillitis, carious teeth, etc. Furthermore, the glands are always discrete, and do not adhere to each other, they are not fixed to the skin or to the fascia, and they do not soften. They are not very much enlarged—not as big as tubercular glands very often may be. As I have already said, they tend to be softer as opposed to the harder tubercular glands. The simple gland is a discrete gland, which is soft, but not softening and not fluctuating. Another diagnostic sign—and this is the most important thing of all—is that the simple lymphadenitis will clear up fairly quickly when the tonsils have been enucleated, the adenoids and septic teeth have been removed, and possibly when the child's general health is improved by fresh air and medicines. Tubercular glands will probably not clear up in this way, or at all events not rapidly. Then we have the absence, in the simple variety, of other tubercular lesions, and also the absence of the general appearance presented by tubercular children. It is not necessary for the child to look "tuberculous" for the glands to be certainly tubercular. But if you see a child who is obviously tubercular as far as the face goes, any enlargement of the glands in the neck is much more likely to be tubercular. In the diagnosis of a large number of the cases it comes to this: that you have to remove all sources of infection and then wait and see what happens. If improvement takes place in the glands the enlargement is simple in nature, while if no improvement takes place, it is probably wisest to regard them as tubercular and to treat them accordingly. There is another investigation which we can make as an aid to diagnosis, and that is to try one of the tuberculin reactions: von Pirquet's reaction is the least harmful of them all. The conjunctival reaction is sometimes a dangerous matter, and I regard the use of it as unjustifiable. One of the late house-surgeons at this hospital, Mr. Percival Mills (1), investigated the value of the von Pirquet reaction in a large number of tubercular, non-tubercular, and doubtful cases. Of eleven cases of tubercular adenitis, all gave a positive reaction. He tested forty-eight healthy children, *i. e.* children in whom no sign of tubercle could be found and whose family histories were above suspicion, and he obtained no reaction in any of them. The tests may, therefore, be regarded as, at least, of some value. Of another set of eight cases which were diagnosed as tuberculous adenitis, but were not proved to be so by operation or any other definite

means, all gave the positive von Pirquet. In one case of lymphadenoma Mills obtained a positive reaction. The ætiology of that condition is, as you know, doubtful.

Another point investigated by Mills was the reaction of the cases to bovine tuberculin and human tuberculin. He found that they reacted to the two tuberculins in an exactly similar manner, and his series included many suffering from various forms of surgical tuberculosis. He found that there was some modification of the reaction if he had previously given a subcutaneous tuberculin injection, and that the modification of the reaction was similar to both forms of skin inoculation.

To sum up, the local treatment of these simple enlargements consists in removing the cause; the general treatment consists in sending the patient to the seaside if possible, or to some convalescent home, and giving him iodide of iron and cod-liver oil.

Now with regard to *tubercular glands*. We have here to consider the glands in the neck, in the chest and abdomen, and in the groins and axillæ. Tuberculosis of the glands in the neck is rare in infants; it is more common in them to find the glands in the abdomen and chest affected. The commonest age for cervical tubercular adenitis is about ten to twenty-five. A quarter of the cases are said to have lung disease. In a third of the cases heredity is said to play a part (Bergmann) (2). It is uncommon to find a definite primary tubercular focus to which the glands are secondary. The apex of a lung is probably the most frequent seat of primary disease to give rise to secondary disease in the cervical glands, while other sites less frequently met with are the skin of the face and the mastoid process.

The majority of the cases are probably infected through the tonsils or the teeth, but no obvious change in the tonsil is found other than enlargement, which may be of a simple nature. We may divide the enlarged glands in the neck in tubercle into those in which the high glands are affected and those in which the low supra-clavicular glands are involved. In the first group there is infection from the throat and mouth, and in the second from the lung, either directly or *via* the bronchial glands. Occasionally one finds the glands beneath and behind the sterno-mastoid about the middle or lower part of the neck enlarged: in my opinion the localisation of the affection to the regions named is strongly in favour of the condition being tubercular. The glands in these regions are not liable to non-tubercular infections to anything like the same extent as are the glands higher in the neck. I have hesitated on more than one occasion in making a diagnosis in such a case, only to be convinced eventually of the tubercular nature of the trouble.

It is not easy to understand by what path the infection has reached a mass of tuberculous glands half-way down the neck, when the glands above and below the mass appear to be normal, and the larynx is free from disease. Does the infection enter through the mucous membrane of the lower part of the pharynx, or the larynx, or does it come down from above from the mouth, passing through the higher glands without causing any marked increase in their size. We all know that, during an operation for tubercular glands in the neck, it is the rule and not the exception to find the disease far more extensive than could be demonstrated before an incision was made. This holds good when the glands half-way down the neck form the centre of the disease. Perhaps the friction of a collar or some undiscoverable factor aggravates the disease in these glands, while those above or below, through which the infection has extended, resist the disease with greater success.

You have to remember that it is common for tuberculous glands to be irritated by some chronic infection other than tubercle. Moreover, a child with chronically enlarged glands due to a septic throat is more likely to get tubercular glands than the child without such, if any predisposition to this disease is present. And we shall find, when we come to consider the treatment of tubercular glands, that it is important for those glands that any source of secondary infection should be removed, so that the individual may have only one germ to fight. As regards the throat itself, tubercle bacilli have, of course, often been found in the tonsils; the exact frequency I do not know. But I think this fact is certainly an argument in favour of doing *enucleation* of the tonsils in a case of suspected tuberculous glands, rather than the older method of imperfect removal. As with the simple enlargements, so here, again, we divide the tubercular glands from a clinical point of view into those with and those without peri-glandular infiltration, the

non-matted and the matted. When the glands are matted the prognosis is less favourable than when they are discrete.

With regard to the morbid anatomy of these glands, the various conditions met with may be classified according to the extent to which the disease has advanced in them. First, there are glands which present simple hyperplasia, and in which the bacilli only, without the so-called "tubercles," are found. Secondly, there are glands similar to those in the first group, but, in addition, they show definite miliary "tubercles." The third group includes those which contain caseous masses, and those which have partially broken down into so-called tuberculous abscesses.

In connection with abscess formation, I would remind you that the persistence of a sinus, which has resulted from the breaking down of a tubercular gland, depends entirely on the presence of diseased gland-tissue at the bottom of the wound. What happens in such a case in this: a gland is infected with tubercle bacilli, and a certain portion of it caseates, and breaks down into an abscess. A good deal of the gland is destroyed, but not all. The shell or capsule of the gland is distended as the wall of the abscess, but eventually this gives way, and the *pus* finds its way to the surface and is discharged. But all the time there is a certain portion of infected gland-tissue which has not been destroyed, and which is partially lining the depths of the abscess cavity formed by the gland capsule, and until this diseased tissue is removed completely the sinus cannot close.

The diagnosis of tubercular glands is in many cases easy enough, while in others it may only be arrived at after the lapse of some time. No difficulty is experienced in arriving at a correct diagnosis when the cervical adenitis forms one of many tubercular manifestations in the case. When there is an obvious primary focus which is unmistakably tubercular, or when the enlargement of the gland is considerable and widespread, and is unaffected to any great extent by cleansing of the mouth and throat, when the child's face is strongly suggestive of the disease to which the condition of the glands themselves points, or when one or more of the glands has broken down and formed a characteristic subcutaneous abscess, or yet again, when the amount of matting is considerable though the disease is chronic, in all such cases as the above the diagnosis is easy, while in many others the true nature of the infection may be almost a matter of certainty. But there are a large number of cases in which it is impossible to do more than suspect the nature of the disease. With experience the number of cases in this group diminishes. Fortunately, the making of a correct diagnosis is not of such great importance as might be expected, since the treatment of simple chronic and early tubercular adenitis is the same. Sometimes we must refuse to express an opinion on a case till we have had time to watch the effect of our treatment.

Treatment.—In the treatment of tuberculosis, whatever be the region attacked by the disease, we have to provide two things—rest to the diseased part, and an abundance of fresh air. In the case of "glands" we can provide rest by removing all possible sources of tubercular and secondary infections. If a definite primary focus is present it should, if possible, be removed. So far as the secondary infections are concerned, the teeth throat, etc., must be attended to. An abundance of fresh air is provided by the adoption of what is usually known as the "open-air treatment," which should be carried out to the fullest extent the circumstances of the patient will allow. This treatment is preferably carried out at a bracing seaside resort, such as Margate, while the improvement in the general health may be hastened by the administration of an abundance of fresh milk and such drugs as cod-liver oil, maltine, and iron.

Then there is the question of tuberculin treatment. Personally, I have not been greatly impressed with the value of tuberculin, though I have had the advantage of watching cases in the hands of some of the most experienced workers in this line of treatment. In no case could I see any reason for thinking the treatment was of the least benefit, while in some cases it did harm, indirectly at any rate, since to give the treatment a fair trial I was induced to delay operating longer than I should otherwise have done when operative measures were undoubtedly called for. It would perhaps be fairer to regard such a circumstance as an indication of our want of judgment in the selection of suitable cases rather than a condemnation of the treatment itself. When one sees fresh tubercular foci make their appearance during the treatment, one cannot refrain from suspecting the tuberculin of doing harm. When improvement follows inocula-

tion or when the latter precedes an exacerbation of the disease, it is always open to the critic to say that the vaccine was innocent of either change.

However, it is, I believe, the opinion of the majority of surgeons that in early tubercular glands we have a condition of affairs which is most likely to be benefited by tuberculin treatment. It is said to be one of the instances where tuberculin treatment *is* of use. But unfortunately the early cases of tubercular glands are just the very cases in which the diagnosis is often uncertain, while no one will deny that a number of these cases do extremely well without tuberculin.

Sir William Bennett (3) writing in the June number of the 'Practitioner' of this year, states that in his experience cases of advanced disease are not benefited by tuberculin. While emphasising the difficulty of telling how far the disease has advanced, he suggests that X rays should be employed more extensively than they are at present for this purpose. He says " . . . tuberculous deposit in glands, especially if caseation is in progress, is resistant to the X rays . . . glands which show a shadow upon X-ray examination are unlikely to be amenable to treatment by tuberculin." This statement is not supported by the results obtained at St. Mary's Hospital, judging from Carmalt-Jones's (4) careful review of the cases treated in the inoculation department of that hospital. In more than half the cases of gland disease included in the review the disease was undoubtedly severe. The results in seventy-nine cases are classified thus: Cured 27, much improved 22, better 18, unchanged 8, and worse 4. Of eleven cases of complete recovery with absolute disappearance of the glands, in four an abscess had been present and had been treated by aspiration.

At Margate, where this method of treatment has been given an extensive trial, the results have not, I believe, been encouraging. One of the medical officers at the Royal Sea-bathing Hospital informed me that he had not seen benefit result from the use of tuberculin in any of the various forms of surgical tuberculosis with two exceptions, viz. early tubercular adenitis, and certain instances of genito-urinary tuberculosis.

Now as to operation, I believe in operating upon all cases which do not respond to less radical measures within, say, three months. If one of the glands softens operative measures should be resorted to without delay, and by this I mean that the abscess should be incised, the whole of its lining membrane removed with a sharp spoon, the cavity left swabbed out with pure carbolic acid, and the wound carefully sutured. As a rule it is better to delay the removal of the rest of the glands till the abscess wound has healed, but the more extensive operation should never be delayed indefinitely. If one of a group of glands has broken down there is sufficient justification for removal of all the diseased glands. If a sinus has formed it should be scraped out thoroughly, care being taken to reach the broken-down gland beneath the deep fascia and to remove the whole of the diseased tissue there found: the cavity is then swabbed out with pure carbolic acid, and either packed or closed by excising the edges of the opening and closing the wound with sutures. I have said that it is usually better to treat an abscess or sinus first and attack the other glands later through sound skin, but occasionally circumstances demand that the whole should be done at one sitting. If a sinus refuses to heal or the wound made in excising it heals and then breaks down, it is advisable to clean out the sinus as before and then proceed at once to the excision of its wall and the glands around. It is not necessary to enter fully into the details of removal of glands in the neck, for which an accurate knowledge of the anatomy of the part is essential, but one or two points may be mentioned. The incision should always be made in, or parallel to, the natural creases of the neck, and if the operation is very extensive two incisions may be made, one above the other. To avoid unnecessary scarring the deep fascia should be accurately sutured with catgut, and the skin edges drawn together with a subcuticular thread stitch. Immobilisation of the head and neck between sand-bags for a week or more is of service in securing rapid and perfect union of the deeper parts of the wound. It must not be forgotten that even when an abscess or sinus is present the throat, etc., should receive attention. If the tonsils require removal, as they almost invariably do, if this has not already been done, this operation should be done when the abscess is dealt with, but not at the same time as the major operation on the neck is performed.

Time prevents me from dealing with tubercular glands in other situations, but the principles of treatment are the same as those which we have discussed.

With regard to tubercle of the mesenteric glands, there are no symptoms associated with that disease. The symptoms which may arise are simply due to complications connected with the presence of the glands, such as acute or chronic intestinal obstruction from bands compressing a coil of intestine, or from kinking of the gut.

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A PARALYTIC STROKE.

A Lecture given at the West London Post-Graduate Medical School.

By SEYMOUR TAYLOR, M.D., F.R.C.P.,
Senior Physician to the West London Hospital.

GENTLEMEN,—I make no excuse in bringing before your attention this common ailment, which is known commonly as a "stroke." And I use this word advisedly, as I wish to speak to you especially about the causes of the stroke, and therefore I do not use the word "hemiplegia," since this only implies a condition, a one-sided palsy which may be due to trauma, or to a neoplasm, or which, indeed, may be functional only.

The word "stroke," on the other hand, conveys to the minds of all, both professional and lay, a sudden catastrophe due to hæmorrhage, by which a patient's future life is shortened and his usefulness seriously impaired. But hemiplegia, without any qualifying prefix, may be, and often is, a curable condition—a condition which may be remedied by the skilful use of drugs, or by the craft of the surgeon.

At the onset it is necessary for me to remind you that in the vast majority of cases of paralytic stroke the exciting cause is some disease, some degenerative change in the arterial system, or to vegetations in the valves of the left side of the heart. Here, again, it is necessary that I should eliminate any discussion on hemiplegia due to embolus, as a limited time will not allow one to wander over so extensive a field.

It is only necessary, therefore, in confining our attention to hæmorrhagic hemiplegia, to remind you of the concise dictum of a distinguished French physician, who laid down the formula "that a man is as old as his arteries." I here present to you a skiagram in proof. It is one showing by the X rays the extensively diseased radial and ulnar arteries of a man, æt. 52 years, but who, patho-

logically speaking, was æt. 102 years, and whose life was worth about six months' purchase. Indeed, this was a liberal limit, as he died suddenly about three weeks after insisting on his discharge from the hospital.

It is not necessary for me to give you, who are post-graduates, a list of predisposing causes, which shall include age, sex, occupation, climate, and season, and also excitement or muscular efforts, which suddenly throw undue stress, pressure, and tension on the arterial system. You probably know all this. But I should wish to draw your attention to some neglected or overlooked predisposing causes.

First, men æt. 50 years and upwards furnish by far the greater number of cases. A man æt. between 50 and 65 years is in the danger zone, or storm centre, of hæmorrhagic hemiplegia. His vessels have probably been the seat of decay for some years previously; but he is still in active work, both of mind and body; he still is making exacting demands on his circulatory system, until some artery, weaker than the rest, suddenly gives way, and the catastrophe occurs. If he shall live in good health beyond the sixty-fifth year he may be said to have passed the decade of cerebral hæmorrhage. Certainly fewer deaths occur from this disease in oldish men than in men æt. between 50 and 65 years.

There is another point in which my experience is not in accord with that laid down in our text-books. Although most authors say that hemiplegia is more frequent in hot climates, or in this country during summer, I am firmly convinced that, so far as Britain is concerned, we encounter more cases in winter and in early spring, especially when a dry east wind prevails, or during a frost, when extra pressure is exerted by the influence of cold on the smaller and thinner-coated arteries. I have in some three or four instances been able to predict the occurrence of cerebral hæmorrhage in patients whom I had previously seen, prior to the advent of frost and bitterly east winds.

It is also of more than passing interest to record the heavy percentage of cerebral hæmorrhages which occur between Saturdays at noon and Monday mornings. Here, again, the victims have already damaged arteries; then supervenes the excitement of a football struggle, or of a cricket match; or they attend a prolonged banquet, or indulge in a heavy meal, the result being a hemiplegic attack during the night, an attack which in not a few instances is overlooked, or, at least, wrongly diagnosed as a heavy, though stertorous, sleep, incidental to the excitement of the previous day.

And with this I would desire to make some remarks concerning alcohol and habitual overfeeding, and the parts which they play in producing hemiplegia.

No one with any clinical experience can doubt the pernicious effects of a too free indulgence in alcohol. Many of our hemiplegics have for some years before an attack consumed more alcohol than was good for them, though they were never considered drunken, and possibly were never known to have been what is vulgarly called "drunk." But many cases of hæmorrhagic hemiplegia occur in men and women who have been most careful as regards alcohol. The so-called learned professions —the Church, the Bar, and Medicine—easily furnish us with many names of eminent men who have died from hemiplegia, and yet who have led almost abstemious lives, at least so far as alcohol is concerned. They have led strenuous lives spent in mental work, and their cerebral vessels have not been able to bear the strain.

On the other hand, I have seen a goodly number of hemiplegics in teetotallers. There is as much danger in the knife and fork as in the wineglass, and total abstainers, as a class, eat more than moderate drinkers. This is the sort of daily life which many men belonging to the class wrongly termed abstainers lead, at least so far as their gastronomic exercises are concerned. A man of this class has an uncommonly good appetite, for which he is duly thankful, and at the same time unduly proud of. He consumes, for breakfast, ham, eggs, and perhaps a chop, or some cold viand; he likes a lunch consisting of at least two "meat" courses; at dinner he indulges in good soup, followed by fish, poultry, or game, butcher's meat and sweets. Probably at at least two of these meals, and sometimes between, he imbibes a large quantity of that very nasty and by no means harmless concoction called cocoa, and towards the end of the day he may pat his comfortable parietes and thank God "that he is not as other men."

A paralytic stroke will overtake that man as soon as, nay, sooner than, it will a man who does

not wear a ribbon but who has enjoyed the good things of this world in moderation and is thankful for them. If over-eating produced half the visible effects on our legs or made our speech anything like as thick as does alcohol, how many would escape the frequent attentions of the police and appearances before the magistrate's court? Intemperance, therefore, is by no means limited to alcohol.

Then as regards heredity, I would remind you that a vulnerable arterial system is often an heirloom. Of this our life assurance offices afford abundant evidence. We have records of a grandparent, a father, and some of his sons all dying at about the same period of life from cerebral hæmorrhage, notwithstanding their careful living, born of their knowledge of their inheritance, by the individuals of the third generation. They die prematurely of cerebral hæmorrhage, as others do from inherited tendency to tuberculosis.

The prodromata of an outbreak of cerebral hæmorrhage are often overlooked by the physician. He notes the patient's history, he detects the brittle artery and the high-tensioned pulse, he records the polyuria with a low specific gravity of the excretion, but he does not always take sufficient alarm at attacks of vertigo, of distressing "buzzings in the ears," and of numbness of the hands and feet, the former especially. Yet not only are these dangerous indications, but they are in very many instances the symptoms pointing to an outburst of hæmorrhage in the almost immediate future.

It must not be supposed that everyone who is the subject of arterial degeneration will have hemiplegia. It is true that a goodly number of these do have hæmorrhage from one or other branch of the mid-cerebral artery, and in these instances complete recovery seldom, or perhaps never, takes places. Others have retinal hæmorrhage, and the eye involved is irretrievably ruined. Others, again, have attacks of severe epistaxis, and may thank their good fortune for this relief. In these latter cases the danger is not so much in the nose bleeding, severe though it be, as in the state of the vascular system which causes the bleeding.

The subject of recovery of the patient's power over his limbs is an interesting one. You know the well-ascertained clinical fact that a hemiplegic may recover the use of his palsied leg, and, indeed, he often does so almost completely. But the upper limb lingers in a state of helplessness for months and perhaps for ever after a cerebral hæmorrhage. We constantly meet such poor maimed subjects, limping along by the aid of a staff, with a jerky spring on the sound leg so that the opposite helpless limb may clear the ground, and with the thumb of his paralysed side carefully hooked into his vest or into a side pocket.

Various theories have been adduced to prove the why and the wherefore of this condition. Broadbent considered that the leg recovered before, or rather than, the arm, because of many motor commissural fibres running across the middle line of the spinal cord in their course to the lower limbs. Horsley argues, and with some anatomical reason, that the collected fibres from the leg centre are so situated in the internal capsule as to be furthest away from the artery of hæmorrhage. I would rather put it this way, viz. that the lower limbs being more important than the upper limbs, the fibres in the internal capsule which go to supply these (lower) limbs are situated further from the danger zone of hæmorrhage by a process of natural selection. You must certainly regard the lower limbs as the more important, and, in addition, you must remember that we have not so much two legs as a pair of legs, one being of itself of very little use, and perhaps of no more comparative service to us than is one blade of a scissors or one blade of a forceps. Not so the arms. They are less coupled together as it were, and even one of them is of great and separate service. Bearing this in mind, it has often occurred to me to ask our veterinary *confrères* if horses suffer from hemiplegia due to hæmorrhage, and if so, does the fore leg and the hind leg on the paralysed side become equally affected, and does the hind leg in that case recover sooner than the fore leg, or are both equally profoundly palsied? For you must remember that in quadrupeds all four limbs, so far as progression is concerned, work in unison, and to some extent automatically.

But not only is the arterial system generally affected, but, so far as the brain is concerned, the site at which the artery gives way is, in the majority of cases in which the hæmorrhage is severe, the point at which there is an aneurysmal dilatation in the vessel. This pathological fact is important. If you have an opportunity of a post-mortem examination and then carefully dissect or

wash out the arterial ramifications in the brain, you will be suprised to find several such small aneurysms. As a rule such careful research is not made. It would appear to satisfy most of us if we find the hæmorrhage, whether on the surface, or tearing up the white matter, or flooding the ventricles of the brain. But many museum specimens which I have examined, and a goodly number of necropsies, have confirmed this interesting fact. A similar condition may be observed in cases of suffocative hæmoptysis in phthisis. This is no new pathological observation, though it has recently been revived as though it were an original discovery. Fearn, a provincial practitioner, first called the attention of the profession, about the middle of the nineteenth century, to these minute aneurysms.

The physical signs and symptoms will vary according to the site of the hæmorrhage and its volume. Instant coma is by no means so common as one would suppose. If the hæmorrhages occur from a slowly leaking vessel the first symptoms may consist chiefly of headache, with numbness in the affected limbs; but none the less, as the bulk of the extravasated blood increases, the symptoms may gradually become more profound; articulation may be more indistinct, with speech incoherent, till coma supervenes, and this is the last stage. I have known patients to expire in twenty-four hours after the onset of the attack, although the earliest symptoms were considered to be by no means grave.

The pupils, unless the hæmorrhage be in the middle line, so as to cut off impulses to both sides, are unequal, with loss of conjunctival reflex on the palsied side. Conjugate deviation of the head and eyeballs may or may not be present.

As regards these two conditions I would desire to say a few words of possible explanation. Much confusion arises as to their exact physiological cause.

A man suffering with hemiplegia, say of the right side, may be found with his head and his eyeballs turned to the right, and on a subsequent examination we find the opposite condition exists. Similarly the pupil on one side may be smaller than its fellow; but on another examination, it may be some hours or a day or two later, we are surprised to find their condition exactly reversed. What is the cause of all this? To my mind Waller's explanation is the most satisfactory. Deviation of the head and of the eyeballs, contraction or dilatation of the pupil on the affected side will be influenced or brought about according as the lesion is a stimulating (irritative) one or a paralytic one. If a stimulating lesion, the muscles of the opposite side which are concerned in rotation of the neck will draw it towards the side of the hæmorrhage; a similar remark applies to the ocular muscles.

Then, again, as regards the condition of the pupils, a lesion which stimulates the third nerve of one side will produce a contracted pupil, and subsequently, as the effect of the lesion is a paralytic one, the same pupil will be found to have become dilated.

But I must confess that these distinctions are more academic than useful.

The bodily temperature at the time of the onset of an attack is usually below normal. This is probably due to shock. The pulse even may be slower than is natural. Then, as the hæmorrhage increases, the temperature rises, especially in basic hæmorrhages, the pulse tension tends to diminish, but its frequency increases, and its regular rhythm is disturbed. This latter symptom is of grave omen.

As the end approaches the respiration, which may hitherto have been attended by only some puffing of lips, becomes stertorous, especially as the patient in nearly every instance lies on his back, the final scene closing with noisy rattle in the larger bronchial tubes, lessening depths of inspiration, feeble flickering pulse, and a moist, clammy skin—symptoms pointing to paralysis of the medullary centres.

Another phenomenon often observed is the irregular tidal volume of respiration, known as Cheyne-Stokes' type of breathing. It is not always present, but when it is it is almost always a fatal omen, though the end is not always so near as some observers would affirm.

The cerebral vessels which most frequently rupture are those in the anterior perforated space, or the lenticulo striate branch supplying the internal capsule, and hence termed "the artery of hæmorrhage." But hæmorrhage from the basilar artery or from one of its branches to the pons Varolii is fairly frequent. And this, indeed, is not surprising when one remembers the frequency with which degenerative changes are seen in the basilar artery. This vessel I have found to be the seat of disease quite

as often as any other central artery. And with this opportunity I would wish to combat the well-copied statement that the cerebral arteries are tortuous with a view to lessening the shock of the blood-current as it courses through the brain. This is an old anatomical fallacy which is copied from one manual to another, and handed down by successive editions. The arteries of the brain are not specially tortuous. Indeed the basilar artery is the straightest in the whole body. The vertebral arteries, again, are for the most part straight vessels, and they only become tortuous in such a locality (the atlas) where movement has to be permitted. On the other hand, tortuosity in an artery is almost invariably associated with movement of an organ, such as the tongue, or with—and this almost amounts to the same thing—enlargement or distension of a viscus or organ.

This is well seen in the tortuosity of the vessels which supply the spleen, the uterus, or the erectile tissues. All organs, therefore, which are capable of any amount of movement or enlargement require that their arteries shall be curved or tortuous so as to allow of a certain amount of "slack," and so obviate rupture.

As regards *prognosis*, I would venture to offer a few words of caution. If we can reasonably come to the conclusion that the hæmorrhage is a small one the prognosis may be favourable; we may look for recovery with more or less use in the affected leg, and possibly even to some return of power in the palsied arm. But much will depend on the clinical aspects which each case presents.

If the general condition of the arteries affords evidence of widespread and extensive degeneration, we must expect that a hæmorrhage, however small, is the precursor of a more serious and probably fatal attack. Again, if insensibility continues for forty-eight hours, recovery, or even a return to consciousness, may rarely be looked for.

The common notion that a third attack is always fatal is, in the main, true. But one wonders how a patient ever recovers from a second attack even.

I shall, perhaps, better enlighten you on the important point of prognosis if I make certain statements in a tabular form.

The prognosis is always grave if—

(1) A second attack supervenes after a short interval;

(2) If there be deep coma, with stertor, increasing cyanosis, and flapping cheeks.

(3) A sudden rise in temperature and an increased frequency of pulse after the shock symptoms have subsided.

(4) Intra-ventricular and pontine hæmorrhages are almost invariably fatal, or, at least, recovery is very rare, and at best only partial.

The *treatment* of this condition requires deep consideration. Although my address is headed "A Paralytic Stroke," our most useful treatment should be, and may be, directed towards obviating or postponing an attack. More good will be done in this direction than by treatment during or after the catastrophe. The patient who has threatened cerebral hæmorrhage should eat sparingly, especially of butcher's meat. But this even does not mean that he may make up for a diminished consumption of beef and mutton by an over-allowance of fish, poultry, game, supplemented by rich puddings, sweets, and cocoa. As I have stated above, the satisfied teetotaller who eats inordinately, who drinks with much self-satisfaction large quantities of cocoa and other supposed harmless beverages, often falls an early victim to cerebral hæmorrhage. I know nothing better for these people than a twenty-four hours' fast every week. But the fast should be a real fast, and it should be observed regularly for some weeks, until the arterial tension is much reduced, and it should be regarded "as well for the body as the soul."

The bowels should act daily without effort or straining. Only one or two of the many aperient waters which are so extensively advertised are of any use by themselves. They may be given every morning before the patient has had breakfast, but they should be supplemented by a mild dose of calomel (gr. $\frac{1}{4}$) given every night on going to bed. By these means an easy watery action of the bowels is assured daily.

The morning cold bath is better avoided. It may sound somewhat startling to speak against some of the cherished notions of an Englishman. But a cold douche is a distinctly dangerous procedure in any case of thickened arteries. It may with advantage be replaced by a warm bath (98° to 100° F.) on retiring to rest.

Then some precautions are necessary so far as mental exertion or excitement are concerned. It is well to advise your patient to retire from active business, with its worries, and its calculations, and its mental concentration. You know of the experi-

ment, mentioned by Ferrier in his Harveian oration, by which a man, firmly fixed on a plank, is allowed to rest on a balance in equilibrium. When the man went to sleep the plank was tilted towards his feet; when he awoke the plank again assumed the horizontal position; but when he was given some exciting intelligence to read the plank was tilted towards his head, indicating the greater flow of blood to the brain and basic ganglia consequent on mental stimulus or excitement, no matter how slight. Here, then, is a physiological experiment which is of the utmost use in the treatment of arterio-sclerosis. It proves emphatically that where possible the sufferer must let his mind be freed from anxieties, and his heart from excitements.

As regards drugs and medicinal treatment, I am convinced that a prolonged trial of iodide of potassium will yield good results. It is not necessary to give the drug in large doses. Five grains three times a day are sufficient. Larger doses act as a diuretic, and appear in the urine without having had much effect on the system. The so-called lowering effect of the drug may be counteracted by adding some carbonate of ammonia, or by giving the iodide in a glass of white wine such as Chablis or Sauterne. But recently another remedy has been introduced to the profession, viz. guipsine. My attention was drawn to this latter drug by a pamphlet from a French source. Prescribed in pills or tabloids, six or more daily, I have found it of considerable value in relieving the headaches and giddiness, of which patients who have contracted kidneys complain. The remedy should be taken steadily for three weeks, and then left off for a time until the pulse-tension, which becomes steadily diminished whilst the drug has been in use, begins to rise again.

Effervescent baths are also of considerable advantage to the patient. They can now be given in this country quite as well as at Nauheim. They should be administered in series of about twelve, with a fortnight's respite. The condition of the pulse and the heart-sounds will be our best guide as to the further continuation of the baths.

Much advantage is said to accrue to the patient by the use of electricity, and properly regulated exercises. I hesitate to speak authoritatively on this point, as one's experience is naturally somewhat limited. But I have notes of some half dozen patients who, having high arterial tension with headaches, derived considerable benefit and relief from the graduated exercises, which are so frequently ordered in cardiac affections, in combination with high-frequency electricity.

However this method of treatment acts, it certainly does act for the benefit of the patient. But I have a shrewd notion that its *modus operandi* is found in the action which is brought to bear on the muscles and on the circulatory apparatus. Nothing is so conducive to arterial changes, once the disease has set in, as lethargy; and to counteract this, I would call in to our aid all such exercises and physical means as shall counteract lethargy—in other words, let us always do our best to prevent our patient growing old.

Treatment during the seizure requires totally different measures. It makes one pause when one witnesses the quiet but scientific treatment which is meted out to a hospital patient and then compares the fussy and yet vigorous methods which are demanded of the private practitioner. Briefly summarised, I think that the routine practice observed in a hospital ward is best. The patient is put on a hard bed, with scanty clothing, his collar is loosened, an ice-bag is applied to his (possibly shaved) scalp, and a dose of calomel or of croton oil is placed on his tongue. His urine is, of course, drawn off from time to time, a precaution which must never be overlooked or forgotten, and the bowels are relieved by enemata if the above purges have had no effect.

But in large hæmorrhages leechings and vivisections are of little or no avail. Notwithstanding the confluence of the two vertebral arteries, and notwithstanding the anastomosis across the middle line at the base of the brain, it has always appeared to me that surgery by the aid of ligation of arteries holds out more hope of arresting the hæmorrhage than do such feeble methods as leeching or other form of bleeding.

So far as drugs are concerned, I am of opinion that in some cases such powerful medicaments as aconitia and veratrium viride are of distinct use in lessening the arterial tension.

After an attack of hæmorrhage the problem how to prevent a recurrence is the all-absorbing one. Two classes of patients who have had cerebral hæmorrhage appear to have the best chances of comparative longevity, viz. those at the extremes of the social scale, the rich or well-to-do, and the very

poor. They both can enjoy a tranquility and ease of mind which is so conducive to recovery, the former in the comfort of a quiet home, the latter in our poor-house infirmaries, both without the worry and anxiety of daily toil. On the other hand, the man who has led a strenuous life, or who has need to support himself and a family, will probably in the comparatively near future have a recurrence of the hæmorrhage, and his life will always be precarious, and usually brief.

Some, indeed most, of the remedies which I have mentioned above, may be continued as prophylactic measures after the hæmorrhage has stopped and absorbative processes are going on. In addition the patient may be ordered continuous small doses of calomel, say one sixth of a grain every night for twelve nights, to be resumed after a week's interval.

By this means the bowels are kept active, though not purged. Free perspiration also should be encouraged, if possible by warm vapour baths. The man with a partially paralysed side will have some difficulty in taking an ordinary warm-water bath; but if assistance be at hand this difficulty may be lessened, and in view of the advantages of free diaphoresis bathing as a remedy should not be neglected.

His diet must be spare. Of this there can be no question or doubt. Three small meals a day should suffice, and the constituents of the meals should consist of a minimum of proteid. Alcohol should be excluded entirely.

But with every care and attention on the part of nurses and friends, with all the experience and skill which medicine can bring to bear, the life of a hemiplegic is always precarious, and its duration not to be estimated.

One word of advice in conclusion is, so far as my experience goes, very necessary. Your patient should be firmly, yet gently, urged to make his or her deposition. I have seen cases in which great distress and embarrassment have occurred to the surviving family owing to a will not having been made. I have also seen cases where the tranquility of mind which has supervened to the patient after he has made an equitable will has been a strong factor in prolonging life, even though his immediate relatives and friends had discountenanced such a step, owing to fear of its possible effect on the patient's brain. Who of us cannot, on the other hand, recall scenes of agonising grief because medical science is not able to rouse a man from coma or even lethargy, which marks the final hours of life in the cases which we have been discussing?

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STRICTURE OF THE URETHRA AND ITS TREATMENT.*

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MR. CHAIRMAN AND GENTLEMEN,—Before reading this paper I must apologise for its somewhat sketchy nature, but owing to the limited time at my disposal I have been obliged to refrain from entering into details, and have simply confined myself to a brief description of those methods of treatment which in my own experience have proved the most satisfactory.

Stricture of the urethra may be congenital or acquired.

The congenital variety is extremely rare except at the meatus or in its immediate vicinity, and then, being frequently due to balano-posthitis, it cannot be called congenital. However, a narrowing of the meatus may exist from birth without giving rise to appreciable difficulty, and calls for no treatment unless accompanied by symptoms of urinary obstruction.

Acquired strictures are suitably classified as—

(1) Spasmodic.
(2) Congestive.
(3) Organic, (*a*) inflammatory, (*b*) traumatic.

A spasmodic stricture may be defined as a temporary narrowing of the urethral canal, due to contraction of the muscles surrounding it. This contraction is either reflex or psychical in origin, and the compressor urethræ is usually the muscle at fault.

Psychical influence is instanced by the inhibiting effect which shame or a sense of hurry exerts over the function of micturition.

Reflex muscular spasm leading to retention commonly follows inflammatory conditions of, and surgical operations upon, the anus and rectum, though in all probability vesical inhibition is a more important factor than muscular spasm in these cases. As is well known, the condition is predisposed to by a nervous temperament and encouraged by undue haste in relieving the symptoms.

* Delivered before the Lambeth Division of the British Medical Association.

The diagnosis based upon the sudden onset of retention without preceding symptoms of urethritis or urethral obstruction is easily verified by the passage of a full-sized sound or catheter, which will readily pass into the bladder, after overcoming with gentle pressure the resistance offered by the compressor urethræ, while withdrawal of the instrument is accomplished without exerting the traction which is necessary to free it from the grip of an organic stricture.

Treatment.—The spasm may be overcome by the occasional passage of a large sound and by correcting any possible cause of irritation that may be located in the rectum, the anus, or the external genitalia. When the condition is complicated by retention, and follows a surgical operation, every possible effort should be exerted on the part of the medical attendant to induce the patient to pass urine naturally. If judicious persuasion or subtle references to the discomforts of instrumentation are unsuccessful, the penis should be placed in a small bowl of hot water, or in the female hot pads may be applied to the perinæum; failing this, with the surgeon's consent the patient should be assisted out of bed and allowed to attempt the act in a natural manner before resorting to the catheter.

Congestive stricture is due to inflammation of the mucous membrane of the urethra combined with swelling and œdema. It is usually associated with acute gonorrhœa or operations upon the urethra, but may be determined by a drinking bout in a patient who is already the subject of an organic stricture.

Treatment.—If the bladder is not unduly distended the patient should endeavour to urinate while sitting in a large hot bath or in a sitz bath. If relief is not obtained by this means and if the symptoms are not urgent he should be put to bed, given a brisk purge and a morphia suppository. When the bladder is greatly distended palliative measures should never be prolonged. In the event of the symptoms being unrelieved by a hot bath, catheterisation must be practised at once. Prior to passing the instrument the glans penis must be thoroughly cleansed, and the urethral canal irrigated with some mild antiseptic, such as a 1 : 4000 solution of potassium permangate.

Organic stricture.—Ætiologically such strictures are best classified as inflammatory-cicatricial and traumatic-cicatricial. The inflammatory form, in the vast majority of cases, is due to antecedent gonorrhœa, but in rare instances it follows a urethral chancre, or ulceration produced by the lodgment of a calculus or foreign body. Gonorrhœal stricture rarely follows a single acute attack, but almost invariably develops after repeated attacks in the course of chronic gleet. It occurs in men between the ages of twenty and fifty (though women are not entirely exempt), and may be single or multiple. When single the stricture, though generally in the bulbous urethra, may form in the balanic or penile portions; when multiple they are usually both penile and bulbar; prostatic stricture is extremely rare.

The usual selection of the bulbous urethra is due to the fact that this portion of the canal is extremely vascular, is richly supplied with glands and follicles, and therefore appears an attractive and eminently suitable habitation for a prolonged stay on the part of the gonococcus. The condition slowly follows its cause, for it is not until the submucous tissues are involved by the inflammatory process that cicatricial contraction commences.

Inflammatory strictures vary greatly in extent from a narrow ring-like band to a contraction that may involve an inch or more of the urethra, and convert it into a tortuous irregular channel; the lumen is usually situated near the roof of the urethra for the simple reason that the inflammatory process that produces the scar tissue is more active in its floor owing to the aggregation, in this situation, of Littré's glands which harbour the micro-organisms.

Clinically strictures may be classified as follows:

(1) *Simple.*—These are strictures which only include a small portion of the urethra, which readily dilate after the passage of sounds, and give rise to no symptoms other than those associated with a narrowing of the canal.

(2) *Irritable*, or strictures in which instrumentation is accompanied by pain and hæmorrhage, and followed by urethral or catheter fever.

(3) *Resilient*, meaning a stricture which readily dilates during the passage of a sound, and quickly contracts again a few days later.

(4) *Cartilaginous*, a term applied to strictures formed by dense cicatricial tissue which only yield to a cutting instrument.

Traumatic strictures are the result of urethral

lacerations of greater or less extent, which have been unsuccessfully treated. They are nearly always single, generally situated in the bulbo-membranous part of the urethra, and rapidly follow their cause; they are usually very narrow, and belong to the resilient or contractile class.

Changes in the urethra.—The urethra behind a stricture gradually becomes thinned and dilated, and this dilatation, by extending backwards to the neck of the bladder, leads to atony of the sphincter and incontinence of urine. As the stricture becomes smaller the dilatation and pouching of the urethra increases, and finally, as a result of decomposition of urine, or deposition of lime salts, the mucous membrane becomes ulcerated, urine escapes into the surrounding tissues, a perinæal abscess forms, and after the pus is discharged leaves a urinary fistula. As the stricture is no longer subjected to the same degree of pressure from the flowing urine, it becomes still narrower, until finally all the urine is voided through the fistulous opening.

The symptoms of stricture.—The subjective symptoms, of which frequency, dribbling, and gleet are the most characteristic, are so well known to you that I will not waste time in discussing them, but will immediately pass on to the treatment.

The treatment of organic stricture.—Treatment is of necessity purely mechanical, and resolves itself into one of the following methods—(*a*) dilatation; (*b*) urethrotomy.

Dilatation may be gradual or rapid. Such symptoms as continuous dilatation, carried out by leaving a catheter *in situ* for several days, and forcible dilatation or divulsion by means of an expanding instrument, have been abondoned in favour of urethrotomy, for if gradual dilatation by gentle manipulations cannot be accomplished, or if the stricture repeatedly contracts after dilatation, a cutting operation is the only method which will prove successful. Gradual dilatation is accomplished with nickel-plated steel sounds or gum-elastic bougies. Metal sounds are excellent instruments for this purpose, as they can be sterilised with ease and certainty by boiling, but when the stricture will only admit sizes below the $\frac{10}{14}$ French scale they must be abandoned in favour of gum-elastic bougies, for the beak of the small sound is so relatively sharp as to make them dangerous weapons.

Gum-elastic bougies should be pliant and smooth; the end of the instrument should be bulbous and the tapering neck flexible, in order to allow the latter to follow the curves of a tortuous stricture. Before use they should be well washed with soap and water, placed in sterilised gauze, and boiled for five minutes. If the varnish becomes cracked or blistered by this process the bougie must be discarded and another prepared in a similar manner.

Presuming now that dilatation has been selected as the line of treatment to be adopted, after thoroughly cleansing the glans penis and irrigating the urethra, a metal Clutton's sound gauge $\frac{18}{22}$ should be lubricated with glycerine or sterilised paraffin and gently passed along the urethra until its beak meets the obstruction. This size is selected because the beak is blunt enough to render it harmless and large enough to prevent it entering or making false passages, while the resistance offered to it by the stricture enables the manipulator to localise the seat of obstruction with accuracy. If the beak is now felt to engage in the stricture, the sound should be passed with the utmost care and gentleness for a further distance of about $1\frac{1}{2}$ in.; it is now left in position for a couple of minutes while the next size larger is selected. This process is continued until a sound of such a size is reached that force is required for its introduction.

Further instrumentation being now contra-indicated, the urethra is again irrigated, the patient given a cup of hot tea, sent to bed, and told to return for treatment again in four days. When the stricture will easily admit a $\frac{26}{30}$ sound, the patient may be discharged as temporarily cured for a couple of months, when sounds should again be passed and the degree of contraction, if any, noted. If no contraction has taken place he may then be sent away for a further period of four months, this procedure being continued until there is no sign of recurrence six months after the last instrumentation. If, on the other hand, the sizes from $\frac{18}{22}$ down to $\frac{10}{14}$ will not pass the stricture, dilatation must be performed with gum-elastic bougies until the latter sound can be employed.

The method of passing a sound is as follows: The surgeon, standing upon the left side of the recumbent patient, holds the sound in his right hand and takes the penis between the forefinger and

thumb of the left. The organ is gently put on the stretch, care being taken to keep the dorsum towards the abdominal wall; the sound is inserted, and being kept parallel with the groin, is gently passed on until the beak comes in contact with the triangular ligament. The handle of the instrument is now swept inwards to the middle line, taking care to keep the shaft parallel to the anterior surface of the abdominal wall. The sound is now gently pushed towards the patient's feet, while the handle is slowly raised, leverage being effected if necessary against the fingers of the left hand, which may now be placed on the curve of the staff in the perinæum. After the shaft has passed the perpendicular the handle may be taken in the left hand and depressed between the legs, carrying the point of the instrument through the membranous and prostatic urethra. When a false passage is made or entered a sensation of roughness will be conveyed to the hand, the beak of the instrument will be deflected from the middle line, and free hæmorrhage will occur immediately. In this eventuality, and in the absence of retention, further instrumentation should be avoided for at least a week.

Rapid dilatation is carried out by passing a series of sounds up to full size under a general anæsthetic. It is only suitable for penile strictures and for strictures of large calibre.

In certain cases, when no metal sound or gum-elastic bougie can be made to enter the stricture, filiform whalebone guides must be used unless an external urethrotomy is selected as the operation of choice. The method of employing these guides is as follows: Half a dozen of them, some with "corkscrew" ends, are passed, one after another, down to the face of the stricture; then, with the penis kept on the stretch, each guide is seized and gently lifted up and down until one of them can be introduced into the bladder. The reason for employing several guides is a two-fold one: in the first place the orifice of a tight stricture is seldom in the middle of the urethra, but at some point around its circumference; consequently if a number of guides are made to impinge upon its surface at several points, one of them will certainly be close to its orifice, and secondly, if the condition is complicated by the presence of false passages, some of the guides, by entering and obliterating them, will allow the remainder to reach the face of the stricture. Once the guide is in position two courses are open to the surgeon: (1) He may run a tunnelled (Gouley) catheter over it, leave it in position for twenty-four hours, and subsequently employ gradual dilatation, or (2) he may either pass the Gouley catheter down to the face of the stricture and perform an external urethrotomy, or, provided the guides have screw sockets which fit a Maisonneuve urethrotome, he can divide it by internal urethrotomy.

Urethrotomy.—As a description of the technique of urethrotomy hardly enters into the aims or scope of this paper, I will content myself by briefly discussing the indications for the operation.

Internal urethrotomy is the operation of choice for penile or bulbar strictures, irritable strictures, resilient strictures, cartilaginous strictures, and for strictures accompanied by urethral fever.

External urethrotomy is indicated for bulbo-membranous strictures, for traumatic strictures, for strictures that have formed again after internal urethrotomy, when extravasation of urine has taken place, when urinary fistulæ are present, when cystitis or renal disease complicate the case, when the bladder has become atonic, and when severe signs of urethral fever follow instrumentation.

After either of these operations, when the wound has healed, sounds should be passed regularly once a month for six months at least to prevent contraction recurring.

Excision of the stricture with immediate urethral anastomosis has come into fashion of late, particularly in America. Its application is practically limited to traumatic strictures, which are narrow and therefore associated with very little surrounding sclerosis. In suitable cases the results are excellent, but the operation *per se* does not do away with the necessity for after-treatment with the sounds.

To sum up:

(1) Contractions at or near the meatus should always be cut.

(2) Strictures of large calibre should be treated by gradual dilatation.

(3) Strictures of small calibre (smaller than 14 French) should be divided by external urethrotomy when in the bulbo-membranous urethra, and by internal urethrotomy when in the bulbo-penile urethra.

(4) Recent, soft, uncomplicated strictures should be dilated.

(5) Resilient, cartilaginous, irritable strictures, complicated by urinary fever, fistulæ, etc., should be cut.

I will now briefly refer to the line of treatment that should be adopted when stricture is complicated by retention or extravasation of urine.

Retention.—When the bladder is not greatly distended the patient should be placed in a hot sitz bath for fifteen minutes; if this fails or is not obtainable, a good-sized gum-elastic catheter should first be passed gently down to the stricture in order to locate it. If small, soft catheters cannot then be passed (or if false passages are present), it is wisest to lay them aside at once and use filiform whalebone guides and a Gouley catheter. Provided that the urethra has not been damaged by repeated attempts at catheterisation with other instruments, particularly small silver catheters, with a little patience one of the guides will certainly slip through. After emptying the bladder the catheter should be withdrawn, a brisk purge administered, and the patient sent to bed. Unless retention again demands it, further instrumentation should not be attempted for four days, when in all probability a No. 3 or 4 catheter will slip in with ease. After the lapse of another period of four days a still larger catheter can be introduced, and then the process of gradual dilatation can be completed or a cutting operation performed. When the guides cannot be introduced in the first instance, a second attempt should be made under a general anæsthetic, and, if successful, a Gouley catheter can be tied in for twenty-four hours. A few days later urethrotomy should be performed. If the stricture is impermeable, or the patient old and obviously suffering with consecutive renal disease, immediate relief, by Cock's perinæal puncture followed later by external urethrotomy is undoubtedly the best procedure. When the patient's surroundings do not permit of an operation of this nature, or the medical man does not feel competent to perform it, supra-pubic aspiration, by relieving tension and relaxing spasm, will often be followed by success on subsequent attempts to pass instruments through the urethra.

Extravasation of urine.—Extravasation may be sudden or gradual. Gradual leakage of urine into the connective tissue surrounding the urethra leads to the formation of a perinæal abscess. This should be incised, and when the inflammatory symptoms have subsided the resulting urinary fistula and the stricture should be laid open by external urethrotomy.

Sudden extravasation occurs when the patient is straining to evacuate the bladder; he becomes conscious of a burning pain in the perinæum accompanied by sudden relief of tension, though very little urine escapes from the meatus. A rapidly increasing swelling forms in the perinæum, and gangrene soon sets in.

Free drainage by multiple incisions must be established as soon as possible; and secondly, in order to prevent the urine flowing over the inflamed tissues the bulbous or membranous urethra behind the site of extravasation should be deliberately opened, and perinæal drainage of the bladder established with a Watson's tube to which a length of rubber tubing is attached. The inflamed tissues should be syringed over and dressed with a mild antiseptic every few hours. Whenever the patient's condition permits he should be placed in a boracic bath for half an hour or more twice daily. Meanwhile his strength must be maintained by stimulants and a light nourishing diet. When the cellulitis has subsided and the sloughs have separated the Watson's tube should be removed, the stricture divided by external urethrotomy, a rubber catheter passed through the penile urethra into the bladder and left in position for ten days while the perinæal wound granulates When the catheter is eventually removed a small quantity of urine will escape through the wound for some time until healing is nearly completed. During this period, and for some months afterwards, sounds must be passed at frequent intervals in order to minimise the risk of cicatricial contraction.

December 26th, 1910.

Influencing of the v. Pirquet Tuberculin Reaction by Various Diseases.—Rolly reports experiences showing that in the course of various infectious diseases the tuberculin skin test may elicit a negative changing to a positive reaction during convalescence even in the absence of tuberculosis. He presents arguments to show that this is due not to some general immunity process, but rather to local modifications in the skin itself.—*Journ. A. M. A.*, vol. lv, No. 24.

A DEMONSTRATION OF CASES AT THE POLYCLINIC.

By DAN McKENZIE, M.D., F.R.C.S.E.,
Assistant Surgeon, Central London Throat and Ear Hospital.

GENTLEMEN,—I shall show you a series of three classes of cases, practically all everyday cases. First of all here are a couple of frontal sinus cases. I shall first cocainise them, so as to examine them later, and if possible, to pass the frontal sinus cannula. With regard to cocainising the nose for general purposes a 10 per cent. solution of cocaine is sufficient. It is better that it should not be weaker than this, and it does not require to be stronger. There is an important hint with regard to putting cocaine in the nose which is worthy of your attention. Take your swab of cotton-wool, and while it is dry tease it out into a flat membrane as it were, then soak it in the cocaine and express the excess of cocaine out of it. Then pass it into the nose, and make the patient hold the head forward. If you do not attend to these trivial simple instructions your patient may swallow some of the cocaine solution, and remember that if he swallows ten minims he swallows a grain of cocaine. If you attend to this little point you can cocainise the part of the nose on which you wish to operate with accuracy, delicacy, and safety. Any cocaine which runs away passes out from the front of the nose, instead of backwards, if you make the patient hold the head forward.

CASE 1.—This girl came to hospital a few weeks ago complaining of deafness, and on testing her ears we discovered that it was a case of simple obstructive deafness—this is to say, she heard the tuning-fork placed in contact with the mastoid longer than I did;—the bone-conduction was increased. In addition, we found that her bone-conduction was greater than her conduction by air; she herself heard the tuning-fork for a longer time over the bone than she did at the meatus. Those are the symptoms in general of simple obstructive deafness—that is to say of deafness due to a lesion which interferes, not with the perception of the sound, but with its conveyance through the meatus or middle ear. On examining her with the speculum we discovered the disease was not situated in the meatus, and by exclusion, therefore, we decided that it was in the middle ear. Then we catheterised the ear, blowing in air. After this she said that she heard a little better, and our diagnosis was that she suffered from obstructive deafness due to a simple catarrh of the middle ear. Examination of the membrane confirmed that diagnosis, because the membrane was thickened and "in-drawn," which after all is not a correct expression, for it is "in-pushed." When there is obstruction to the ventilation of the middle ear, the air which is normally residual in the middle-ear cavity undergoes absorption, and there is, therefore, a minus or negative air-pressure in the middle-ear cavity compared with the atmospheric pressure, and as a consequence the atmospheric pressure drives in the membrane, so that instead of facing the outer world obliquely, it comes to lie horizontally, in other words, it is pushed in and pushed up. This appearance of the membrane is always a sign of middle-ear or Eustachian tube trouble. I have brought this girl for a special purpose, and that purpose is that in girls about this age you get another kind of obstructive deafness which is of much graver prognosis as regards the hearing than simple catarrh. This girl is suffering from obstructive deafness due to catarrh. The other form of obstructive deafness is due to what is known as "otosclerosis." I do not know whether otosclerosis is popularly recognised or not; if not it should be, because it is a disease which leads to irremediable loss of hearing. In its early stages it produces obstructive deafness, that is to say the same symptoms as this girl has. But there is this great difference between deafness due to chronic catarrh and that due to otosclerosis. In the latter, the patient's hearing is not improved after inflating the middle ear with Politzer's bag or a catheter. And secondly, if you look at the membrane in otosclerosis you will see a beautiful picture of the normal physiological membrane. Otosclerosis is a disease of the bone of the ear; it is not a mucous membrane disease. The internal ear, as you know, consists of a series of convoluted chambers and canals driven through dense, hard, ivory bone. In otosclerosis this ivory bone is absorbed and replaced by spongy bone, and this bulky spongy bone compresses and destroys the delicate structures in the labyrinth and so sets up irremediable deafness. The disease-process begins

near the foot-plate of the stapes in the middle ear, and it is said that the fact that there is here a little inflamed spot accounts for the peculiar pinkish glow in the glistening tympanic membrane, which makes it such a beautiful object in these cases. It is important that you should be able to distinguish between ordinary chronic catarrh which you can improve by simple measures and the otosclerosis for which as a rule you can do no good.

CASE 2.—Here is another ear case. This boy came to hospital three weeks ago complaining of earache, mental dulness, and giddiness; he was looking very ill indeed, and his temperature was 102° F. The story was that he had had a cold, which set up earache, and two or three days later a purulent discharge appeared in his ear. In other words, it was a case of acute suppuration of the middle ear. The question was, was the middle-ear suppuration simple, or was it complicated? Here we had a boy with a history of pain followed in a few days by discharge. But—and here is an important point—there had been no fall in the temperature when the discharge appeared, or at least it had not come down to normal, and there was no improvement in the general condition. The headache and giddiness persisted, and so did his general malaise. The question therefore was, Was this boy suffering from simple suppuration of the middle ear, or was there a complication? The complications of middle-ear suppuration are mastoiditis, labyrinthitis, and sometimes extension of the infective process to the endocranium. On examining his membrane I found there was a perforation which was discharging freely. But in addition there was visible a little feature which you should all know because of its great importance. Here is the ovoid membrane with the long process of the malleus passing downwards and backwards. When there is acute suppuration such as this boy had then there is a bulging of that membrane, and the bulging takes place, not of the membrane as a whole, but of one limited localised area, namely the postero-superior portion of the membrane. And that bulging may be more or less extensive. It may involve only half the membrane, or it may involve the whole of it so that the bulging portion seems to be in contact with the anterior wall of the meatus. Look what effect that has on treatment. What I saw in this boy was not complete bulging of membrane but something like I show you in this drawing. And the perforation seemed to be in the anterior portion of the membrane. One could not see the perforation because there was too much swelling. The question was, then, whether we should admit this boy into hospital and open his mastoid cells at once. Probably we should not have done any harm if we had, but events proved that this was not necessary. We put him under gas and I inserted a very fine-bladed knife into the tympanic cavity, bringing it through the bulging part of the membrane so as to leave a fairly large gaping wound. Pathologically we know that acute infective suppuration of the middle ear is most intense in the attic, and not in the lower portion of the tympanic cavity. So when we open the membrane we bring our incision through the upper portion of the membrane, in order to drain the upper portion of the tympanic cavity—the attic. That is the theory. If, however, you ever get a case like this, of a patient with or without discharge, in whom there is deafness, earache, fever, headache, mental dulness, giddiness, and a bulging membrane, take a fine knife and put it in where you see most bulging, and do not bother about the anatomy, and you will know you will be doing the right thing, because the portion of membrane which bulges is that which corresponds to the seat of the intensity of the lesion. It is a simple and easy thing to do. The procedure is to put your patient under gas or chloroform, put in a decently wide speculum, dry out the ear, take a fine knife bent at an angle, make an incision, and leave the rest to Nature. This boy is now absolutely free of discharge and is hearing perfectly well. How should I have known that there was any necessity for mastoid operation here? If, after the incision of the membrane, the pain and fever had continued for two or three days, I should have known from that that it was necessary to open his mastoid cells from behind the ear.

CASE 3.—This man exemplifies another common type of ear case. He used to suffer from chronic suppuration of both ears of many years' duration. The interesting fact about him is this, that he has had a radical mastoid operation performed on both ears, and yet he can hear nearly as well as I do. We operated upon one ear first of all, and the result as regards his hearing was so favourable that we proceeded to operate on his other ear without any hesitation. We have now

cured the suppuration in both ears by the radical mastoid operation without having caused any material loss of hearing. That is an extremely good result, and not a common one. I think the patient is worthy of your notice, because he shows that the mastoid operation does not always lead to diminution of hearing. The general rule is as follows: If the patient hears very badly as a result of the suppurative disease, then the hearing will be rather improved after the radical mastoid operation. But if the hearing is good before operation, then the radical mastoid will probably make it worse. The indications for operating on chronic suppuration of the middle ear are, I am afraid, rather loosely defined in general otology. In other words, every man has his own idea with regard to the indications for the mastoid operation. I am rather radical on the point. I do not think it is advisable that an ear should continue discharging for longer than five or six months if under treatment. If at the end of that time the discharge is continuing, it is better to open up the ear and look at the cavities. I have seen many cases of radical mastoid operation in which on opening the middle ear one met with lesions that had been unsuspected before the operation was undertaken. And over and over again in the literature you will find descriptions of cases where on opening the middle ear a fistula is discovered in the roof of the tympanic cavity leading into a brain abscess, of which there was no indication whatever in the symptoms. In other words, when you are dealing with suppuration in an obscure deep-seated cavity like the middle ear, you do not know what is going on unless you open it up and look at it. The argument against operating so early in suppurative middle-ear disease is that the operation damages the hearing. The operation may damage hearing no doubt, but the disease process itself will damage the hearing if you leave it to go on, and, indeed, it may do very much worse, it may end the patient's life.

Case 4.—Here is a case which will interest you all as general practitioners. This little girl came into the hospital six months ago with a discharge from her ear. We operated upon her. We first of all did a partial mastoid operation, but it did not heal up. We then did a radical mastoid, and that did not heal up. I mean that not only did the discharge from the middle ear continue, but that the wound behind the ear did not heal up, for granulations sprang up from the bony cavity and kept the skin edges apart. We had her under chloroform two or three times and curetted those granulations, and each time we thought we had got rid of the trouble, but we had not. I may say that we suspected tuberculosis, because tuberculous disease of the middle ear is not uncommon in children, and when we fail to get healing after the radical mastoid operation, it is frequently due to tuberculous disease of the petrous or mastoid process. Our suspicions were confirmed on examining her abdomen, when we found enlarged glands in the mesentery. We also applied the von Pirquet test (vaccinating the child with tuberculin), and found she responded actively. She was tuberculous. What were we to do? She was in hospital two or three months, and was not any better for our surgical measures, and so we sent her to Ramsgate. She stayed there a couple of months, and came back with the wound behind the ear quite dry, the ear itself practically healed, all save one tiny spot of granulation which is now getting well. It is true that in addition we tried a new application—"pyoktanin." But if I had tuberculosis I would rather rely on Ramsgate than on pyoktanin.

Case 5.—You have all come across cases of asthma. It begins, as a rule, very early in life. There are, of course, forms of asthma due to renal and cardiac disease, which come on late in life, but most of the obstinate intractable cases of paroxysmal asthma come on in childhood. Here is a boy who is, or rather was, the subject of asthma. Of recent years it has been recognised that there are spots in the nose from which irritation may proceed of such a nature as to produce asthma, and this boy exemplifies this condition, because by a very simple method indeed we have been able, for the moment at all events—I do not know how long it is going to last—to cure him of his asthma. With regard to the treatment of nasal asthma, there has been a good deal of discussion of late, and I do not pose as an authority on the point. I have tried many different plans, and sometimes I have succeeded and sometimes I have failed in curing it, but there are two definite rules which we may follow: One is that if there is nasal disease it should be treated;—if there are polypi they should be removed, and if there is an enlarged

turbinal it should be reduced in size. The other rule is that if there are none of these conditions present then you apply the cautery lightly to the nasal septum opposite the end of the middle turbinal. This will frequently cure the asthma, as it has done, for the moment at all events, in this case. I do not know why that should be so; it sounds and looks very much like magic, but there are certainly a number of cases in which success follows this very simple procedure, which, by the way, was first introduced by Mr. H. A. Francis. And if you cure one or two cases of asthma in that way there is nothing which will redound more to your credit as practitioners. The point of the cautery must be so lightly applied that it seems almost as if you have not cauterised at all; if you cauterise deeply you will not do good. Cauterise both sides of the septum at once.

CASE 6.—Here is a girl who has been suffering from nasal suppuration, the disease being in her antrum. I have brought her because she illustrates one of the steps in treatment. Antral suppuration is easier to diagnose than suppuration in any of the other nasal sinuses. In some of the more remote sinuses indeed it is very difficult to detect suppuration, and the diagnosis is only reached after several months' treatment and examination on the part of experts. In the case of the antrum, on the other hand, the diagnosis is simply and easily effected. In antral suppuration there is a story of pus coming from one side of the nose, or it may be from both sides. Our first object is to examine the nose in order to see where this pus is coming from. You will probably remember the three meatuses in the nose, one of which is particularly interesting to us here, the middle meatus, namely, where the middle turbinal is situated. If a patient has purulent discharge from the nose examine the interior of the nose by reflected light and a mirror and direct you attention to the middle meatus and middle turbinal body. If the suppuration is proceeding from the antrum or frontal sinus, or both, you will find the pus is welling out from underneath the middle turbinal. Looking up into the meatus of the nose you will see the middle turbinal hanging down from the lateral wall of the nose. In antrum suppuration it often happens that the middle turbinal looks as if it were split, and in the cleft there is a little streak of pus. Sometimes instead of that picture, which limits your diagnosis to two sinuses, the whole middle meatus is seen to be filled with polypi. But that is another story. Let us fix our attention on the former condition. When you find this pus you have to decide whether it is coming from the antrum or from the frontal sinus. Remember the shape of the antral cavity. The antrum has its natural orifice situated at the spot I show you, and when suppuration takes place the pus accumulates in the cavity until it reaches this orifice which opens under the middle turbinal, and then it pours out into the nose. After applying cocaine you wipe away the streak of pus, and lay your patient down on one side, with the supected antrum uppermost. That will enable the pus to run out of the natural opening quickly, and when you look again you will find that another streak of pus has formed. That affords your proof. Sometimes this plan fails. Then we puncture the antrum through the nose with a fine trocar and cannula and wash it out. The treatment of antrum suppuration has altered of recent years. The old-fashioned plan was to drill up through a tooth-socket into the antrum, and if a carious tooth was not provided by Nature in the proper position for drilling into the antrum, a sound tooth was removed for the purpose. Nowadays we do not consider it necessary to sacrifice a sound tooth to drill up into the antrum. Although we recognise that antral suppuration may be due to a carious tooth, and although we remove the tooth because of that suppuration, we do not, however, drill the alveolus. And I will tell you why. If you drill a suppurating antrum and drain it into the mouth, the patient must necessarily swallow the pus, and that, of course, is not advisable. And if you prevent the patient from swallowing the pus by putting in a plug, why drill the hole at all? What we do nowadays is this: We make an opening into the antrum from the nose, not from the mouth. We remove the inferior turbinal by means of a snare and scissors, and break down the nasal wall by means of a burr or chisel enough to enable us to get our finger in. The free nasal opening made in this way will not only drain the antrum, but the drying effect of the fresh currents of air will retard the progress of the disease. And about 75 per cent. of cases of chronic antrum suppuration can be cured by this simple measure. Moreover, while it is admittedly easy for a patient to wash out the antrum through an alveolar puncture, it is just as easy for him to

wash out the antrum through the nose by means of a curved tube. If this simple measure fails to cure we perform the radical antrum operation—that is, we make an opening through the canine fossa, to get through the cheek into the antrum. The hole must be big enough to enable you to see what you are doing, and then under good illumination the diseased mucosa is removed from the sinus. I do not think that will be necessary in this case, for the discharge is drying up. In any event we shall give her time, which is the great element in the cure of nasal sinus suppuration. Sinus suppuration of the nose may last for years, even under the best treatment. If the suppuration is multiple, that is to say, if more than one sinus is affected, it lasts for many years, and the treatment consists of continually persevering with our measures. Of late we have used vaccines for these cases with considerable success.

CASE 7.—The next case exemplifies the growth of polypi from suppuration; it is a case of frontal sinus suppuration. In polypus formation due to sinus suppuration you cannot cure the polypus unless you remove the whole of the ethmoidal region. This is done by curetting. In this case, when the polypi had been curetted away our way was made clear to the frontal sinus. I ask you now to direct your attention to the middle turbinal. If the frontal sinus alone is affected, if the ethmoidal cells are not badly affected, and there is no great growth of polypus, you cannot wash out the sinus because of the middle turbinal, for it is a solid, substantial bony structure, and an effort to pass the cannula up under cover of the turbinal would cause your patient considerable pain. In these cases the turbinal must first be removed. But where there has been polypus formation, as in this man, the polypoid growths absorb and destroy the middle turbinal more or less entirely. And when we come to curette, so as to remove the polypi, we in so doing remove the broken-down middle turbinal, and after the curetting is over and the wounds have healed up you can see the whole inside of the nose smooth and lined with mucous membrane. In the anterior part of the nose you can then see some pus. It is coming from the frontal sinus. Here I show you a frontal sinus cannula with a double curve, and this we pass up through the infundibulum into the frontal sinus in order to wash it out. I first flush the sinus out with a mild antiseptic, and then I am in the habit of injecting alcohol in strengths varying from 50 per cent. upward. This I find has an astringent and healing action upon the sinus suppuration. If this treatment fails, and if the patient is sorely troubled with headaches, vertigo, or signs of pus retention, we shall open into the frontal sinus through the eyebrow.

CASE 8.—As you see, this little girl is wearing a tracheotomy tube, and when I ask her to phonate you will hear that she is very hoarse. She is suffering from multiple papillomata—warts—in the larynx. She came to hospital about two months ago on account of the hoarseness. On listening carefully we could detect some stridor, and on examination with the laryngeal mirror I managed to spy some rough papillated growths in the larynx. So she was admitted, and after tracheotomy I succeeded in removing a fairly large growth from the larynx by means of Killian's direct method of operating. I pass the growth round for your inspection. There are still a few papillomata left in her larynx, which I will try to remove at a later date. Papillomata are common growths in the larynx of a child. They are very apt to recur after removal, and they last a long time. But by repeated removal and by providing the patient with a tracheotomy opening, they gradually cease to recur, and the patient gets well.

December 26th, 1910.

Diphtheria Epidemic.—The study of this epidemic by Perkins and Furrer emphasises the importance of administering a prophylactic dose of antitoxin to all children on admission to a hospital; the necessity and value of taking cultures and making bacteriologic examinations; and the advisability of taking cultures from the nose as well as from the throat before discharging patients from quarantine. The nurse in attendance on a diphtheria case should not be discharged from quarantine until negative cultures have been obtained. Isolation, quarantine, disinfection, and fumigation are found to be of little or no value in controlling an epidemic until a systematic search is made for latent infections among inmates.—*Journ. A. M. A.*, vol. lv, No. 24.

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A CLINICAL LECTURE
ON
ADAMS-STOKES' DISEASE.

Delivered at the Seamen's Hospital, Greenwich.

By FREDERICK TAYLOR, M.D., F.R.C.P.,
Physician to the Hospital, and Consulting Physician to Guy's Hospital.

GENTLEMEN,—To-day I propose to say a few words about a disease which has been called Stokes-Adams' disease, and recently Adams-Stokes' disease. It is interesting sometimes in the case of diseases which are named after those who have first described them to go back to the original articles upon which the names are based, and which form, it may be, the earliest information with regard to them. I have here the 'Dublin Hospital Reports' for 1827, in which is a paper on "Cases of Diseases of the Heart," by Mr. Robert Adams, who was a member of the Royal College of Surgeons in Ireland, and one of the surgeons to Jervis Street Infirmary. Among these cases is recorded on p. 396 the following: An officer in the Inland Revenue Department, æt. 68 years, had for a long time been incapable of exertion, and was subject to continued cough. When Mr. Adams saw this gentleman he was recovering from the effects of an apoplectic attack which had suddenly seized him three days before. He was oppressed by stupor, having a general disposition to sleep, and still a very troublesome cough; but what most attracted Mr. Adams' attention was the irregularity of his patient's breathing, and the remarkable slowness of the pulse, which generally ranged at the rate of thirty in a minute. His doctor had been in almost continuous attendance on this gentleman for seven years, during which time the patient had had at least twenty apoplectic attacks. Before each he was heavy and lethargic, with loss of memory; and he would fall down in a state of complete insensibility, so that he was on several occasions hurt by

the fall. When they attacked him his pulse was even slower than usual, and his breathing was loud and stertorous. He recovered from these attacks without any paralysis. Œdema of the feet and ankles had occurred five months previously; his cough was now more urgent, his breathing more oppressed, and his faculties became weaker. Six months after Mr. Adams saw him the patient had an attack which carried him off in two hours, and before the arrival of the medical attendant. I need not read you the full account of the inspection, which was made fifty-six hours after death. The right auricle was much dilated, and the right ventricle "externally presented no appearance whatever of muscular fibres, and seemed composed of fat throughout almost its whole substance. The left ventricle was very thin, and its whole surface was covered with a layer of fat. Beneath this the muscular structure was not a line in thickness; it had degenerated from its natural state, was soft and easily torn, and a section of it exhibited more the appearance of liver than of a heart. The septum of the ventricles presented the same appearance." There were also some lesions in the aortic valves, which were studded with specks of bone. This patient, then, had these apoplectiform attacks, from which he recovered without any paralysis; he had a very slow pulse, slower at the time of the attacks; and after death he was found to have a fatty heart with extremely thin ventricle and auricle, and obviously a fatty septum. Then we come to the article by Dr. Stokes, Physician to the Meath Hospital, entitled: "Observations on Some Cases of Slow Pulse," and published in the 'Dublin Medical Journal' for 1846. He begins as follows: "In the fourth volume of the 'Dublin Hospital Reports' Mr. Adams has recorded a case of permanently slow pulse, in which the patient suffered from repeated cerebral attacks of an apoplectic nature, though not followed by paralysis. The attention of subsequent writers on diseases of the heart has not been sufficiently directed to this case." Dr. Stokes's first case is that of a man, æt. 68 years, who three years previously had been suddenly seized with a fainting fit, in which he would have fallen if he had not been supported. This occurred several times during the day, and always left him without unpleasant effects. He had then repeated pseudo-apoplectic attacks, as they were called, which were not followed by paralysis. He had a slow pulse and also a murmur over the aortic valves. From the time of the first attack three years previously till the time he was observed by Dr. Stokes he had at least fifty attacks. The fits were variable and appeared to be brought on by exertion, by distension of the stomach, and by constipation. There is a very interesting point about the first attack observed by Dr. Stokes. He writes: "There is little warning given of the approach of the attack. He feels, he says, a lump in the stomach, which passes up through the right side of the neck into the head, where it seems to explode and pass away with a loud noise resembling thunder, by which he is stupefied." So that these epileptiform or apoplectiform attacks were preceded by a kind of aura, such as precedes attacks of ordinary epilepsy. Nevertheless, he was never convulsed nor did he froth at the mouth, but he occasionally injured his tongue. The duration of these attacks was four or five minutes. The pulse frequently went down to 28 per minute. It is noted here as having a frequency of 36, 30, 28, and 32. And curiously, he was able to avert the attacks sometimes by a peculiar manœuvre: "As soon as he perceives symptoms of the approaching attack he directly turns on his hands and knees, keeping his head low, and by this means he often averts what otherwise would end in an attack." On March 4th he had what Dr. Stokes described as semi-beats between the regular contractions, unattended with impulse, and corresponding to a similar state of the pulse, "which thus probably amounts to 36 in the minute, the evident beats being only 28." On March 18th these abortive contractions were about four in the minute. He went out, and was readmitted in June, at which time pulsation was noticed in the veins of the neck, the pulsations in the neck being more than double the manifest ventricular contractions. This is an important observation. Nothing further is heard of him.

In a second case there was a similar condition of the heart, in which pseudo-apoplectic attacks did not occur. The third case which Dr. Stokes records is the one which we have heard of—Mr. Adams's original case; and he refers to another he has heard of in the person of the Earl of K—: His pulse was 25 and he was subject to syncope; post-mortem he was found to have thickened, ossified valves and disease of the cerebral arteries. The condition of the myocardium is not recorded.

Dr. Stokes includes another case of Mr. Adams' of slow pulse with a lesion of the aortic orifice, and a heart which, Mr. Adams said, was one of the most friable he had ever met with; and another case with repeated pseudo-apoplectic attacks, not followed by paralysis, in which the pulse was 30, and a valvular murmur was present. Dr. Stokes remarks that the preceding observations go to prove that a condition of permanently slow pulse and diseased condition of the aortic opening is not uncommon.

The seven cases which Dr. Stokes refers to were not of precisely similar nature so far as the apoplectic attacks were concerned. They were, however, cases of slow pulse. Five of them had organic disease of the aortic valves without murmur, two had an abnormal second sound, two had an aortic regurgitant murmur, and four of them had syncopic or apoplectic attacks.

That is the disease or combination of symptoms to which the name Adams-Stokes' disease or Stokes-Adams' disease has been applied. They have become more familiar of late in consequence of the special attention which has been directed to the fact that the venous pulsation in the neck is more frequent than are the beats of the heart. Some years ago I remember seeing a patient who had a slow pulse of about 30 in the minute, and my endeavour was to find out if I could hear any beat in the chest which was not represented in the pulse. I thought he probably had alternate beats of the ventricle, of which one was strong and resulted in a pulse which could be felt, and the other was feeble and produced insufficient movement in the blood-current to cause a radial pulse; that is, I thought I heard a slight sound between the radial pulses on some occasions. But it is now probable that these were auricular beats. As you see, Stokes recognised that the veins in the neck were beating more frequently than the pulse was beating. And therein, apparently, is the essential pathological basis of this disease.

Gaskell, experimenting upon animals' hearts, produced, by constricting the auriculo-ventricular ring, a condition in which the ventricular movements were dissociated from the auricular movements, and then the auricular movements were faster than the ventricular. Erlanger experimented directly upon the so-called auriculo-ventricular bundle of His; he compressed this, and by gradual compression succeeded in altering the relation of the auriculo-ventricular beats as follows: With a moderate degree of compression the auricular beats were twice as many as the ventricular; with greater pressure the ventricular beats dropped to 1 in 3, and with more still to 1 in 4. These two experiments show that by some interference with the heart in the position between the auricles and ventricles, the relation between the two cavities can be definitely altered. This auriculo-ventricular bundle of His has, as you know, been comparatively recently discovered. The auriculo-ventricular node is apparently a mass of mixed nerves and muscular tissue, which is situate in the auricular septum not very far from the opening of the coronary sinus, and from that auriculo-ventricular node there proceeds a band of fibres along the septum until they reach the position in the heart at the top of the ventricular septum, just below the *pars membranacea septi*, or undefended space; and there this band of muscles divides into two parts, one going down the right side of the septum in the right ventricle and the other going down the left side of the septum in the left ventricle. The fibres in each case terminate in moderator bands. In the above experiments the compression of the auriculo-ventricular muscular bundle of His prevented the transmission of impulse along it. In the ordinary process of cardiac movements impulses start near the junction of the great veins with the auricle, and are transmitted to the auriculo-ventricular node, and thence by the auriculo-ventricular bundle to the ventricles. And so the movements of the auricle are communicated in the ventricle. If that bundle is interfered with by disease it brings about this dissociation of movements which was effected experimentally by Gaskell and others. If the bundle is relatively little affected, then it appears that in certain cases an impulse from the auricle may fail to be transmitted, and so the ventricular contraction does not occur. The next auricular contraction is transmitted, and is followed by ventricular contraction; the next one fails, and so on. This condition has been termed "heart-block." I confess it is a term which I have no affection for, but I know no better. The block means the obstruction, whether structural or functional, in the bridge of tissue between the auricle and the ventricle which prevents the regular transmission of impulses from the one

to the other. Fatty degeneration, or gummatous change, or the administration of digitalis may each cause heart-block. When the ventricle responds to the auricle only once in two beats, or once in three beats, or less frequently, that is a partial heart-block. But there is a complete heart-block, when the auricle and ventricle are entirely dissociated, and the ventricle starts its own movements and contracts quite independently of the auricle. And then as a rule the auricle beats faster than the ventricle. Adams-Stokes' disease is characterised by partial heart-block, with its infrequent pulse, and not by complete heart-block; but a case of complete heart-block with cerebral symptoms has been published (Gossage).

I can, of course, mention other cases. One of the most interesting cases is the case published by Dr. Alfred Stengel, of the University of Pennsylvania, five or six years ago, of which he says: "Up to the present communication, however, no case has been observed pathologically in which a lesion has been found involving the bundle of His." That is to say, he claims this as the first case in which pathological evidence of the nature of Adams-Stokes' disease was demonstrated. It was the case of a man, æt. 57 years, who suddenly fell backwards and became unconscious for a few moments. There were no convulsive movements. He did not bite his tongue; and he speedily recovered, and went back to his work. Three months later he had another exactly similar attack. He then had attacks in more rapid succession every two or three weeks. Two years previously he had spasmodic movements in the legs during the attacks. The radial pulses were equal and synchronous—36 in the minute. During the seizure there seemed to be only partial unconsciousness, lasting a few seconds, the eyes sometimes rolling from side to side or upwards, and the eyes staring at the beginning of the attack. The face was pallid, and the lips were cyanosed. The patient died, and there was found "a patch of atheromatous character, sclerotic and white, and it extended to the endocardium, exactly over the bundle of His," where this band passes from the ventricle to the auricle. In the 'Lancet' of February 20th, 1909, is an important case, that of the late Sir William Gairdner, of Glasgow, who died three years ago from this disease. At that date he was 82 or 83 years of age. In August, 1900, that is to say, when he was aged 75 years, he was attacked by unconsciousness and muscular rigidity; later he was semi-conscious, dull, and listless. The pulse at that time was only 6. In a few hours the attack passed off, the pulse returned to normal, namely, 60 or 70. He had another attack in October of the same year, and two attacks the next year, and on each occasion the normal condition returned between the attacks. He had a sudden attack in 1902, when the pulse was 16, with syncopal symptoms. In 1903 he had another, and on the same day another. And so it went on for a long time. After that the pulse was constantly slow, mostly 32, and it varied between 28 and 34. Dr. Gibson describes the case, and says the end came with dramatic suddenness. The examination showed that there was a dense fibrous transformation spreading from the aortic cusp of the mitral valve into, and largely replacing, the normal tissue of the auriculo-ventricular node and first part of the bundle. Here, then, are two cases, and many others have been published in which with the characteristic symptoms there has been found some pathological change involving the auriculo-ventricular band.

That, then, is Adams-Stokes' disease. I have recently seen a case of the kind, but I was not able to make those observations on the pulse or jugular veins which have been made on some of them. It was that of a gentleman, æt. about 57 years, who came to me first complaining of faintness and attacks of dyspnœa. I examined him, and found that the first sound of the heart was a little lengthened, and was sometimes accompanied by a short murmur. He appeared to me to be getting into that condition of cardiac feebleness associated with arterio-sclerotic changes which many male subjects have with advancing years. The first definite attack which he had suggesting the complaint which we are speaking of was that about fourteen months later, viz. on November 4th, when a fainting, or epileptic attack took place; another occurred on the 8th, and another on the 24th. In the intervals he had slighter attacks. He felt dizzy, rolled his eyes, twitched in the hands and feet, and his face flushed, and then he became pale. On the first of the month he was twitching on and off for an hour, though he was conscious most of the time. The pulse was sometimes as low as 38, 36, and 32, but the

respirations were 50 per minute. I saw him about this time with Dr. Chadwick, of Lynn, and Dr. Forrest, of Terrington-St.-John; his pulse was 80 and 81, and his respirations 20. There was very little evidence of positive heart disease. His heart was somewhat covered by lung, so that the impulse could not be felt, the sounds were faint, and there was no definite murmur. Five weeks later I saw him again; he had been unable to do his business, and he was inclined to be quiet, and avoid exertion. He seemed to be feeling the effects of a failing circulation. Five months later he had epileptic attacks, with a pulse of 24, and sometimes the pulse stopped for twenty seconds at a time. I saw him again; he was lying in bed, and while I was there his face turned grey and livid, his eyes were rotated to one side. When awake he was rather dazed and delirious, and had some delusions. He died five or six days later. That was a case in which, though I had not the evidence from jugular or radial tracings, the ventricle probably did not receive all the impulses starting in the auricle.

Before going further I will show you tracings from other cases, upon which is based the inferences as to the movements of the auricle and ventricle. In each case one tracing is from the jugular vein and the other from the radial pulse, taken by the sphygmograph in the case of the artery and by a tambour applied to the vein, so that the movements of the levers due to its impulse can be written on the same slip of paper as the radial tracing for purposes of comparison. The tracings show that there are two right auricular beats to one ventricular beat. In the next one there are three auricular beats to every one ventricular beat.

I may say a little more on one or two points in these cases. One is the slow pulse. You will have noticed that it may become as slow as 6; and in some cases it stops fifteen or twenty seconds during the occurrence of the attack. In most cases it becomes as slow as 20 to 35 in the minute. At first the condition is not permanent, that is to say, the pulse is found to be abnormally infrequent only when the attacks occur, and this may be at long intervals. It is curious that with a definite pathological condition like this the physiological disturbance happens in this irregular manner. I suppose we have very little knowledge of what actually takes place. The explanation of the association of the cerebral and cardiac symptoms which is almost by common consent advanced is that the cerebral attacks are the result of the cardiac failure. In the earlier conditions there appears to be a lack of evidence that that is the case. One knows that in most cases the patient has an apoplectic or epileptic attack, sometimes with a definite aura, and when observed the pulse is found to be slow; when he recovers the pulse also recovers.

Then as to the character of the fits. You hear them described as apoplectiform or epileptiform. There is no doubt that they are both apoplectiform and epileptiform. The patient suddenly loses consciousness, and is to a certain extent convulsed. But in a comparatively few cases does one read an account of complete convulsions like those of true epilepsy. Extensive general convulsions have not been described in any one of these cases which we have dealt with to-day. But there is a comparatively sudden falling, and in some cases there has been an aura. In other cases, as you will observe from what I have told you, there have been faintings, or attacks described as syncope; and in one of the cases, the notes of which I have read you, there have been visible changes in the state of the circulation; the skin has been dusky and cyanosed, accompanied with twitchings of small muscles, and this, on passing off, has been followed by definite flushing.

Then I would like to mention the condition of the heart. Aortic disease has been recorded as present in some of these cases. Dr. Stokes' cases were mostly aortic disease, and probably he was making his observations from that point of view. But Mr. Adams and Dr. Stokes knew nothing of the bundle of His, and the point of interest is, that in Mr. Adams's first case there was extreme fatty degeneration of the heart, involving the septum ventriculorum, and in another of his the heart was extremely friable. There can be little doubt that in these cases the bundle of His was badly damaged. I have read you the account of the condition of the heart in Sir William Gairdner's case. In both cases there were fibrous changes in the tissues and in the endocardium involving the bundle of His.

Is it not probable that the cases in which a fattily degenerated heart has been diagnosed on

account of a very slow pulse are really instances of fatty implication of the bundle of His?

With regard to other clinical symptoms, there is not *necessarily* any physical sign of valvular disease. We cannot deny the value of the stethoscope as a means of discovering a lesion of the valves. A patient comes to you and wants to know whether there is a murmur or not, and he attaches very great importance to your observations: but the matter is not settled by the presence or absence of a murmur. There may be no murmur in these cases, or there may only be a modification of the cardiac sounds, in spite of which there may be grave lesions of the myocardial tissue and of the septum of the ventricles.

If there are changes such as those which result from rheumatism, or degenerative changes accompanying disease of the valves in advanced years, there may be accompanying signs, and evidence that the valves are involved; but they are not specially related to this affection. And one can, on the other hand, imagine, and, indeed, see that both fatty changes and syphilitic degeneration and fibrous changes may occur in this part of the heart without leading to any definite auscultatory evidence that anything is wrong.

I have already referred to the probable sequence of events, to the certainty that the apoplectic symptoms are due to cardiac failure. You will remember that Dr. A. E. Russell a year or two ago delivered some lectures on the subject of epilepsy and other functional disorders of the brain, in which he argued strongly in favour of epilepsy being caused by circulatory disturbances rather than being purely a primary disturbance in the cerebral nerve-cells. And another interesting point is the Cheyne-Stokes' respiration, of which you have seen instances. In this very paper of Dr. Stokes', from which I have read extracts, he refers to Cheyne's observation of cases of breathing of the kind. Cheyne-Stokes' breathing, which is obviously due to a cerebral failure, starts in the respiratory centres of the medulla, and that disorder was first observed in the very same class of cases as those we are dealing with, that is to say, in aortic cases. And Stokes himself remarked that they were aortic cases, and not cases of nervous disease. The observations of Cheyne-Stokes' breathing occurring in tubercular meningitis, apoplexy, and hemiplegia were of much later date. Here, then, we have another instance of nervous, that is, of bulbar disturbance, being secondary to cardiac vascular lesions.

Patients may live for a very long time. There are some cases on record where the duration has been twenty years from the commencement of the attacks. The patient to whom I have previously referred as presenting a pulse of 30, with intervening sounds, of probable auricular origin, was æt. 48 years. He suffered from headache, and was discovered to have a pulse of 16 in the minute. Just before I saw him he had a sort of vertigo, and he forgot absolutely something that he did or said, and did not recall it when reminded. Six weeks later he was fairly well, but his pulse was still only 35 to 40. That was six years ago, and I have heard from the doctor who attended him, Dr. J. D. Thomas, to the effect that he is, as far as he knows, alive, that he certainly became very much better, and that his pulse came back to its normal frequency. If, then, it is a case of the kind, it is one in which the condition was temporary, a functional condition perhaps which might occur at that time of life without any definite ascertainable structural cause.

Now with regard to the treatment. The treatment of these cases is, unfortunately, as you might expect, very unsatisfactory. It is a difficult thing to get into the interior of the heart by means of medicines, and surgery can do nothing there. If we can be sure it is syphilitic, if we know or suspect it is gumma, we should give anti-syphilitic remedies, such as mercury and iodide of potassium, and for that object one should always ask after a syphilitic history. But if it is a fibrous change associated with rheumatism, or a degeneration associated with senile advances, or if it is a fatty change, very little can be done. Digitalis is not a drug in any case suited to bradycardia, but it is especially not suitable to these cases because it functionally interferes with the conductivity of the auriculo-ventricular bundle, and may actually itself induce a condition of heart-block. You can protect the patient from himself, and save him as you do patients suffering from other epileptiform conditions, and preserve his powers as far as possible by tonics, careful regimen, and judicious rules of life. The majority of cases are fatal, and the ultimate prognosis must be very bad. You will note that it is a disease very largely of old age. Many of these cases are in men of advanced years. Two of them were 68, and Sir William Gairdner was 75 when he had his first attack. It is, then, in many cases a condition of degeneration which is not amenable to direct treatment.

January 2nd, 1911.

SEPTICÆMIA HÆMORRHAGICA; SEVERE CAPILLARY AND PARENCHYMATOUS HÆMORRHAGES OF SEPTIC ORIGIN.*

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THERE is a class of grave and fatal hæmorrhages which are not included under the subjects of primary or secondary hæmorrhages, nor can they be ascribed to any congenital lesion such as hæmophilia, nor to degenerative lesions, such as occur in Bright's disease or are induced by the toxic influence of alcohol. Indeed, these causes of hæmorrhage are excluded from consideration in this paper, and none of the cases referred to were predisposed to hæmorrhage from these causes.

The peculiarity of the hæmorrhage I propose to discuss is that it may occur from a mucous or serous membrane without any naked-eye breach of surface; or it may happen from a raw surface without any special blood-vessel being opened; or if into an organ, there is parenchymatous hæmorrhage without any preliminary traumatism or degenerative process in that organ.

Such hæmorrhages are well-known complications of the specific and tropical fevers, as well as of scurvy. One cannot attribute such hæmorrhages to an excess of pulse tension, for indeed the hæmorrhages, whether from the nose or gastro-intestinal canal, continue to recur when the patient has become very weak.

In bringing forward some cases which have come under my observation, I am well aware that I can deal only inadequately with the essential part of the subject—the bacteriology. For that is the clue to the treatment, whether by an antitoxin given immediately or by auto-vaccination as soon as possible. Yet, however imperfect the bacteriological details, the cases I have seen have been so grave as to merit consideration, especially as there has been no satisfactory treatment hitherto.

My attention was first drawn to the subject by Dr. Klein's experiments on a variety of streptococcus causing hæmorrhage. In 1897 Dr. Klein (14) received from Dr. J. E. Colby, of Malton, fluid taken from vesicles on the skin of a man who, two or three days before, had been engaged in skinning sheep dead of puerperal septic œdema after lambing, the so-called "Gargel." Dr. Klein cultivated a streptococcus from the material, which, when injected subcutaneously into guinea-pigs, caused subcutaneous and intermuscular connective-tissue hæmorrhages around the site of inoculation, along with blood-effusions into the pleural and peritoneal cavities. The same results followed the injection of cultures into rabbits and sheep.

* Delivered before the Surgical Section of the Royal Society of Medicine.

SEPTICÆMIA HÆMORRHAGICA IN ANIMALS.

The term "septicæmia hæmorrhagica" is one adopted in veterinary pathology for an important class of epizootic fevers. It has been applied to the disease called in Italy "Barbone bufali," or buffalo disease, concerning which much has been written in that country (10) (16).

In 1878 Bollinger described the disease as causing a great fatality among deer, wild boars, cattle and horses, in or near the Royal Park at Munich, and in Bavaria generally. Lignière discovered in the so-called hæmorrhagic influenza of horses a coccobacillus; in the blood he found and cultivated streptococci only, but the streptococci could not be cultivated in a virulent form. In France a disease of cattle distinct from rinderpest has been termed Pasteurellosis bovis; in it there occur hæmorrhagic pneumonia, pleurisy, and peritonitis.

In India and Egypt hæmorrhages occur with malignant sore throat, or strangles, in horses and cattle, and even in hardier animals, such as mules and elephants (23) (7). Lingard (15) has made extensive researches in respect to this disease in cattle and deer, terming it "septicæmia hæmorrhagica pleuriformis." He produced a cultivation of the organism so virulent as to kill both hill and plain cattle with certainty in nineteen to forty-two hours. If the cultivation became weak it could be restored to virulence by passing through rabbits. The organisms described in these animal and tropical infections are variously referred to as bacilli, coccobacilli and streptococci; but the resulting pathological changes have a general similarity.

Returning to human pathology, there occur cases of *hæmorrhagic empyema and ascites*. Oliver (17) has described cases of thoracic empyema followed by fatal hæmorrhagic oozing from the wall of the cavity; the organisms found were the diplococci of Friedländer, not the pneumococcus.

(In these cases injury to an intercostal artery or pulmonary vessel was excluded.) Hæmorrhagic ascites (9) has proved very fatal, doubtless due to infection by organisms, but I am not aware of any definite report as to the organisms present.

Under the term of *purpura* a collection of hæmorrhagic diseases are discussed in works on medicine. Some of these cases are traceable to streptococcal infection, especially when complicated by puerperal fever. *Melæna neonatorum* forms another class of fatal hæmorrhagic disease. The subject has been recently summarised by Thomson (21) in Allbutt and Rolleston's 'System of Medicine.' Among the organisms identified are not only streptococci, but also *Bacillus pyocyaneus*, staphylococci, short bacilli and Friedlander's pneumobacillus. Klebs (13) in 1875 found a micrococcus in nine new-born children who had died of melæna, the cultivation of which produced in young rabbits fatal hæmorrhagic lesions. In three cases which Bar (5) examined he found streptococci. On the other hand, Tizzoni and Giovanni (22) reported three children of the same family who died one after the other of a contagious pustular impetigo accompanied by hæmorrhage. *Staphylococcus pyogenes aureus* was the organism found in both primary and secondary lesions. The hæmorrhages were traced by microscopical examination to a thrombosis of the smaller vessels by the staphylococci.

The acute hæmorrhages which complicate *diphtheria of the fauces* are very fatal: fifty-eight cases were reported by Austen and Coghill (3), representing 6·59 per cent. of admissions; these all died, the organisms found besides diphtheria bacilli being streptococci and staphylococci.

Acute hæmorrhages into the stomach as shown by black vomit constitute a grave complication. In medical practice they follow the specific and tropical fevers; in surgical practice they occur late in such cases as intestinal obstruction, gall-bladder suppuration, and gangrenous appendicitis (6), and may follow operations when sepsis cannot be prevented. I have seen this fatal termination in the various forms stated, but unfortunately I have no bacteriological information to give.

Hæmorrhagic pancreatitis is a well-known form of parenchymatous hæmorrhage, in which the exact type of organism causing the affection cannot yet be stated with certainty. There does not appear to be sufficient reason to ascribe the hæmorrhages to *Bacillus coli communis*, which is the organism generally found in such abdominal complications.

Dr. F. W. Andrewes (1) has published the post-mortem examination of a case of *muscular hæmorrhage following typhoid fever.*

A man, æt. 18 years, the subject of double chronic otitis media, was admitted to St. Bartholomew's Hospital during the third week of an attack of typhoid fever. Before the temperature became normal he had a relapse, and died on the fifty-second day. On the day before he died subcutaneous hæmorrhages appeared on the feet and elsewhere; in addition, there formed a considerable swelling in the left thigh with œdema of the limb below. Post mortem, in addition to the lesions characteristic of typhoid fever, there was found a diffuse extravasation of blood into the muscles. From the muscles of the thigh and from the blood-clot a pure cultivation of streptococci was obtained, but no typhoid bacilli. The streptococci were not evenly distributed in the muscles, but had especially collected about the arteries and veins. Of leucocytic aggregation there was little or no evidence.

Hæmorrhages of a similar character may be produced by poisons such as phosphorus (12) and benzine (19) both in human beings and, experimentally, in animals. It would seem likely that the actual influence on the blood-vessels is through the agency of toxins produced by such poisons, and therefore the actual production of the hæmorrhages may essentially not differ from those originated by organisms.

Hæmorrhagic Cellulitis.

I will, in the first place, refer to cases of acute hæmorrhagic cellulitis in which on incision no distinguishable amount of pus escaped, but blood poured out most freely.

Case 1: *Hæmorrhagic cellulitis of the arm.*—A previously healthy man, æt. 25 years, was admitted under my colleague, Mr. Stonham, with an extensive swelling of the arm and forearm, which had followed upon a slight abrasion of the hand. At first sight there appeared nothing uncommon in the case; the subcutaneous tissue of the arm and forearm seemed to be distended by pus. But no sooner was an incision made than blood-clot escaped, then fluid blood poured out, and the

patient showed alarming signs of acute anæmia. Not a single vessel could be seen to bleed, although incisions were extended along the line of the blood-vessels. It was only by very firm packing of every part of the involved area, followed by similar bandaging, that the bleeding could be controlled. There was no further hæmorrhage, the exposed tissue soon became covered by granulations, and healing followed.

CASE 2: *Acute hæmorrhagic cellulitis of the left buttock and thigh.*—A labourer, æt. 47 years, was admitted under me in a very weak state, but what attracted my attention was his lemon-coloured pallor. He had been ill for six weeks without evidence as to cause. He presented a deep-seated elastic resistance, filling the left iliac fossa, also the left buttock, and upper third of the thigh. In the thigh the swelling was more superficial, and the skin over it showed a dusky brown, brawny œdema. The swelling did not pulsate nor yield a bruit, and the circulation in the left leg and foot was as good as on the opposite side. He had a very rapid pulse, 152 per minute, the respirations 40 per minute, the temperature 100·4° F. Impressed by his pallor, which I took to indicate concealed hæmorrhage, I made a narrow puncture with a knife in four places into the swollen thigh, keeping well away from any vessel. From each puncture bright blood poured out and quickly clotted. No pus was reached. Each puncture was plugged firmly with gauze. He would soon have bled to death had any freer incision been made or a drainage-tube been inserted. Dr. Lazarus-Barlow cultivated a diplococcus from the blood which had flowed out. At each dressing clots of blood with turbid serum escaped, but no pus. Locally the swelling got less, but the man became weaker, his temperature rose to 104° F., his pulse to 172, and he died three weeks after the incision. No post-mortem was allowed, but the nature of the case was unfortunately confirmed by the infection of two patients in neighbouring beds with erysipelas.

A young man was in the next bed with whitlow and palmar suppuration, and he had a severe attack of general erysipelas, from which he recovered.

An old man in the opposite bed across the ward, who had tuberculous disease of the shoulder, was likewise attacked with general erysipelas, and he died. This happened in 1900, and among my cases in that ward there has not been an outbreak of erysipelas since.

CASE 3: *Hæmorrhagic cellulitis of the thigh.*—My colleague, Mr. Tubby, had a very similar case of hæmorrhagic cellulitis of the thigh, who was admitted in an even weaker state; blood flowed freely from the punctures made, and he died the same night.

CASE 4: *Hæmorrhagic cellulitis round the knee.*—A rather less severe case than the foregoing occurred in a carpenter, æt. 27 years, who, a month before, had felt on returning home at night great pain in his right knee. He therefore rubbed horse oils and turpentine into his knee; then the knee was poulticed for a fortnight. A week before admission a discharge commenced, and the swelling began to spread down the leg and up the thigh. On admission he was very pale, weak, and half delirious. On making incision a little pus but chiefly broken-down blood-clot escaped, so that it was impossible to irrigate out the cavities. As compared with the previous cases, blood did not pour out, but only re-collected in the cavity as clot. There was no sign of arterial lesion or of disease of bone. A week later, the man in the meantime having improved a little, I amputated through the thigh, a little above the knee, and the patient recovered good health. The lower end of the thigh and upper part of the leg removed was extensively infiltrated by pus and broken-down blood-clot, but the bone was unaffected, and there was no lesion of the large blood-vessels. Here there is no doubt that the cellulitis arose from the pustules raised on the skin by the horse oils followed by the poulticing.

CASE 5: *Acute subcutaneous hæmorrhage following a viper's bite.*—A man engaged in stealing pheasant's eggs was bitten by a viper, the two punctures being evident on the dorsum of the proximal interphalangeal joint of the middle finger. I saw him about two hours after, very pale and collapsed. His arm and forearm was very much swollen, bright red, and there was marked pulsation when one grasped the forearm. The swelling was due to extensive extravasation of blood into the subcutaneous tissue, and in the course of the next six weeks the coloration of the limb passed through the stages of red, blue and yellow, as the blood-clot was slowly absorbed. Suppuration was limited to the area around the actual punctures made by the viper's fangs.

I pass on to quote the obscure cases of—

CASE 6: *Hæmorrhagic enteritis.*—I saw a patient at the end of a long and obscure illness, which had been carefully watched by Dr. Seymour Taylor and Dr. Michael. A girl, æt. 24 years, had diarrhœa and at first occasional hæmorrhages from the bowel, vomiting and foul stools. Then she began to pass large quantities of blood by the stools, and lapsed into a state of acute anæmia, with a pulse-rate of 120, but at no time did the temperature vary much from normal. There was an absence of all signs of typhoid fever, and also of any sign in favour of the second hypothesis, tuberculous enteritis. On the day preceding death there was noted for the first time a tenderness in the right iliac fossa, followed by clear signs of commencing infective peritonitis. I could detect an enlarged appendix, yet the general condition of the patient was quite different from the common type of acute gangrenous appendicitis. She was very pale with sunken cheeks, but had no earthy look. The tongue was pale, moist, tooth-indented but not furred. The pulse was very rapid and weak, 130 or 140 per minute, but the temperature continued normal. The abdomen was a little tumid, but nowhere persistent nor tender, except over the appendix. The appendix was removed as quickly as possible. No gangrene nor pus, nor sign of typhoid fever or tubercle, was met with; there was congestion of the peritoneum around and blood-stained fluid was sponged out of Douglas's pouch. The patient failed to react to saline infusion; she died four and a half hours after the operation; there was no post-mortem. The appendix removed was distended to the size of the end joint of the little finger by blood and mucus, but there was no evidence in the interior of gangrene or pus. The microscopical examination showed acute hæmorrhagic inflammation of the mucous and submucous coats, in which were numerous cocci and rods.

Several grave cases of hæmorrhage from the kidney and bladder have come under my notice which cannot be classed with common causes of hæmaturia.

CASE 7: *Renal hæmorrhage.*—An unmarried female, æt. 20 years, had had for three years pain in the left side, of a dull character, especially when standing and walking, with slight and irregular albuminuria, especially in the evening. In addition she had had several attacks of severe pain with sweating and nausea. She had not passed pus or blood, and an X-ray examination was negative. The kidney could not be felt to be swollen, nor was it markedly movable. The pain became so severe as to prevent her from exercise and games; although when lying in bed the pain was less, yet she still felt it in the lumbar region radiating down over the hip, and a tender spot could be felt corresponding to the position of the pelvis of the kidney. An operation was decided upon on account of the persistence of the pain; the kidney when exposed did not seem very movable, so the interior was explored through an incision on the lower border just sufficient to admit a finger. Finding no sign of stone, the kidney was fixed by sutures and the wound sewn up except for a drain. The case took a normal course until a week after the operation, when menstruation set in, and with this there occurred free bleeding from the wound. To check this the drainage-tube was removed and a plug of gauze inserted. Three days after the hæmorrhage the temperature rose owing to decomposition of the clot, so the clot which had collected around the kidney was wiped out. Twenty days after the operation the patient commenced to have severe rigors, and as all the clot had been cleared out, and no tracking of pus could be discovered, the kidney was removed, twenty-four days after its fixation. The removed kidney was found to be infected by numerous small abscesses in the cortex due to staphylococci, which had supervened upon old-standing fibrotic changes, presumably caused by the intermittent kinking of the ureter. The renal veins were not thrombosed, nor was there any evidence that the hæmorrhage had come from a large branch of the renal artery. Auto-vaccination with a culture of the staphylococci failed to prevent the rigors from recurring. Repeated examinations failed to detect any pyæmic metastases, but the patient died thirty-two days after the operation. In this case the hæmorrhage seems to have been concomitant with the menstruation, which set in earlier than was expected but otherwise appeared normal. The infection of the blood-clot appears to have been by staphylococci lying latent in a kidney which had been affected by intermittent kinking of the ureter.

CASE 8: *Renal hæmorrhage.*—For more than ten years a man, æt. about 45 years, had had what was

supposed to be intermittent albuminuria and attacks of pain in the *right* side which were thought to be colic. He perhaps occasionally had slight hæmaturia. He was next seized with acute pain on the *left* side with obstinate constipation for a week. After a second attack of acute pain on the *left* side hæmaturia and albuminuria were noted. He was therefore examined by the X rays. Then it was discovered that the position of the *right* kidney was occupied by a large branched calculus. On the *left* side in the region of the kidney pelvis was a stone the size of a large orange-pip. It became then evident that the *right* kidney had been slowly destroyed by a calculus; that the so-called intermittent albuminuria was traceable to the discharge of a small amount of pus; and that the left-sided attacks were due to a calculus temporarily blocking the *left* ureter and threatening life. Therefore I operated first upon the *left* kidney, which was enlarged and vascular, making a small opening with blunt forceps and extracting the stone from the pelvis. There was no special hæmorrhage at the time, but three days later the same thing happened as in the previous case; free bleeding set in, the wound had to be plugged, the clot which collected in the wound and around the kidney decomposed and had to be wiped away. The infection in this case was easy to understand. Fortunately, after the wiping and syringing out of the broken-down clot, no further hæmorrhage occurred. But the left ureter remained for a time blocked by clot. The patient improved, so that a month after the first operation I was able to remove the *right* kidney. This had been reduced to a friable abscess cavity enclosing a large branched calculus. The second operation wound healed in about six weeks. The first wound continued to discharge urine, and very little passed into the bladder until three months from the operation, when the sinus had become very narrow. Then one day he was seized with pain down the course of the left ureter, the obstructing clot moved on, and urine passed into the bladder, upon which the sinus closed. The patient now, two and a half years after the operation, is following his occupation in fair health, but there are traces of pus in the urine.

Case 9: *Renal hæmorrhage.*—A footman, æt. 42 years, was admitted to hospital suffering from severe hæmaturia. He was in such acute pain that at first sight some abdominal perforation seemed probable. On further observation the pain seemed situated in the right lumbar region, extending down the right ureter. The urine contained a large amount of dark blood. Eighteen years before he had been attacked with severe pain in the right side and hæmaturia. Thirteen years before the pain recurred and he went into a hospital in London, where he was told that the pains were "functional." A year later he went into another London hospital, but received no active treatment. All that could be made out before operation was that he was markedly anæmic, and had a resistance in the right lumbar region. There was no fever, and nothing was discovered by X-ray examination. On incising the right lumbar region the transversalis fascia was found deeply infiltrated by blood. After cutting through the fascia a large amount of blood-clot was scooped out; some of the clot was recent, some of old standing. The roof of the cavity was formed by the under surface of the diaphragm and liver. The upper half of the kidney seemed to have disappeared in the mass of blood-clot. With much difficulty the pedicle was incompletely isolated and ligatured and cut across, leaving a little kidney substance within the grasp of the ligature. There was no sign of pus, nor was there a urinous nor foul odour. The ureter was patent and not thickened. The wound healed well, except for a sinus, which was persistent for two months; he has been seen lately in good health. One explanation of the case is that a mild infection caused parenchymatous hæmorrhage which destroyed most of the kidney; the other, a more mechanical hypothesis, that a kinking of the ureter was followed by hydronephrosis, or cystic formation, upon which partial rupture supervened.

The two following cases have been previously described in the Clinical Society's 'Transactions' (20):

Case 10: *Renal hæmorrhage recurring.* — A butler, æt. 42 years, was sent into hospital by Dr. Bunn on account of recurring hæmaturia sufficient to cause anæmia. About eighteen months before he awoke in the night feeling "full of wind" and "unable to get his breath." Not at the time, but next morning, he passed blood-stained urine. Three months later he had another attack, commencing with pain in the hypogastrium. He had several more attacks, complicated in some by retention. He went into a hospital, where he was

examined under an anæsthetic and by the X-rays, after which he was told that no operation was indicated. The examination did not provoke hæmaturia, but an attack recurred three days after leaving the hospital. He was then anæmic; had normal micturition four times a day. The urine was the colour of port-wine, and specific gravity 1030, without excess of albumen, and it contained nothing abnormal but blood. Whilst lying in bed the patient continued to pass large clots, which sometimes obstructed micturition until a catheter was passed. It was impossible to examine with a cystoscope, so supra-pubic cystotomy was performed. After washing away all the bloody urine and some sediment the wall of the bladder looked quite normal; it showed no tendency to bleed as a result of sponging. Clear urine was collected from both ureters, and no clot or blood came from the urethra when a catheter was passed. The supra-pubic wound was drained for three weeks, and he then passed urine by the urethra. For a year the patient had no return of hæmaturia. Afterwards by letter I was informed that he had had some slight recurrence.

Hammersley (11) has described a patient who had attacks of pain in the right kidney with severe hæmaturia which was relieved by removing an inflamed appendix, and he attributed the renal affection to infection spreading from the appendix.

Eve (8) recorded a case of fatal hæmaturia due to psorosperms.

Posner (18) in a man subject to attacks of hæmaturia found amœbæ.

Case 11: *Hæmaturia, fatal without obvious lesion.*—A man, æt. 44 years, first noticed blood in his urine a fortnight before death. He had previously been in good health; he had not been abroad; with the hæmaturia there was neither pain nor frequency of micturition. The hæmorrhage increased to an alarming extent, and I saw the patient immediately after his admission to the hospital. He was then very anæmic, almost pulseless, and was breathing rapidly. The bladder was tensely distended, but not tender. The temperature was below normal; there was no skin eruption. He was infused subcutaneously and *per rectum* with saline fluid; he passed urine containing a large quantity of blood; he gradually became pulseless, and died within twelve hours of admission. Dr. Hebb made a complete post-mortem examination, without finding anything to throw light upon the cause or source of the hæmorrhage. The bladder contained a small quantity of bloody urine, which was not found in either kidney pelvis. This is the only evidence in favour of the source of hæmorrhage being the bladder rather than the kidneys. No organisms were identified.

Perhaps this case is like those of which Gull spoke and named "renal epistaxis," and Klemperer called "angio-neurosis."

Conclusion.

It is obvious that I have included in this paper cases which differ widely from one another. But they were all grave and obscure. Some cases of septicæmia hæmorrhagica have been certainly caused by streptococci. If the classification of Andrewes and Horder (2) be adopted, one may include under streptococci the pneumococci, coccobacilli, and other varieties which have been found.

The occurrence of the hæmorrhage may be explained by supposing the organism to collect, as described by Andrewes, especially in contact with the walls of the blood-vessels. Under the effect of the toxins the walls of the vessels become weakened and give way, so as to allow the escape of blood. This occurs without intra-vascular thrombosis, which in ordinary acute inflammation must be the chief factor in preventing hæmorrhage. Neither is there any massive collection of leucocytes at the inflammatory focus, so that very little pus forms.

The identification of streptococci as the commonest organism suggests an immediate recourse to streptococcal antitoxin, whilst the identification of the particular organism is being made.

The first case of success by the use of streptococcal antitoxin was reported by Ballance and Abbott (4). A finger was pricked at a post-mortem on a case of suppurative peritonitis, and acute septicæmia set in, complicated by cutaneous hæmorrhages. Immediate relief followed the use of streptococcal antitoxin.

When the infection is not due to streptococci, or is a mixed infection with staphylococci or *Bacillus pyocyaneus*, the preparation and injection of an auto-vaccine should be carried out as quickly as possible.

It has been noted above that the extravasated blood clots readily, and this explains the fact that the administration of calcium chloride has had no influence on the hæmorrhage.

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GASTRIC AND INTESTINAL HÆMORRHAGES (PROBABLY OF SEPTIC ORIGIN) OCCURRING AFTER ABDOMINAL OPERATIONS.

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A. B—, a woman, æt. 48 years, was sent to me by Dr. M. W. Loy, of Gillingham, Kent, with symptoms of gall-stones, complicated by inflammation in the region of the gall-bladder. For three years she had suffered from attacks of pain, which started at the lower right costal margin and passed up to the right shoulder and between the scapulæ. These attacks were not accompanied by vomiting, nor were they followed by jaundice, but they were very severe and lasted several days. They were brought on by heavy work or by indiscretion in diet. In addition, she had a constant sense of throbbing in the region of the liver, increased to actual pain when she sneezed or took a deep breath. As usual with the patients with gall-stones she was the subject of habitual constipation.

Examination of the abdomen revealed two lumps below the ninth right costal cartilage, one superficial to the muscles and adherent to the skin, the other beneath the muscles, moveable from side to side, continuous with the liver, and evidently a distended gall-bladder. She was not jaundiced and the urine was normal. The liver dulness was normal in extent.

Operation (November 18th, 1909).—A 4-inch incision made vertically at outer border of right rectus and below costal margin. In the muscle layer a lump was found which, when cut, had somewhat the appearance of a gumma. It proved to be the thick wall of an abscess situated between diaphragm and liver. The peritoneal cavity was opened and the gall-bladder found to be very adherent to under surface of liver. Gall-stones were felt in its interior and one firmly impacted in the cystic duct.

The peritoneal cavity was carefully closed off by means of flat sponges and gauze. The subphrenic abscess was then opened up by the channel it had made for itself to the abdominal wall. The patient was turned on her right side and the contents were allowed to escape. They consisted chiefly of thin watery pus and a little blood. The cavity was explored with the finger and proved to be median, *i. e.* in the falciform ligament. Subsequent bacteriological examination of the fluid proved it to be sterile.

The abscess surface was then shut off with sponges held back by retractors reaching into the peritoneal cavity. The edge of the liver was held up by the fingers of an assistant. It was decided to perform cholecystectomy.

The laborious work of paring the gall-bladder from its dense adhesions began. The liver was right under the costal margin and in consequence the work had to be done at a great depth from the surface. The gall-bladder was incised and three large stones and several smaller ones were extracted. Very little bile escaped. The neck of the cystic duct was then tied, and in cutting it away an artery in the neighbourhood of the portal vein was cut.

This vessel was possibly the cystic artery at its origin from the right hepatic. The hæmorrhage was very free and much difficulty was experienced in securing the vessel, which was ultimately tied with silk. The gall-bladder was finally removed; the two cavities left above and below the liver were isolated and separately drained, rubber tubes and gauze packing being employed for both. Silkworm gut was used to unite the wound.

I wish to emphasize the difficult nature of the operation. Cholecystectomy is not infrequently a trying performance for the surgeon, and this particular case (owing partly to the depth and obliquity of the gall-bladder and also to the many firm adhesions around it) was the most difficult one I have as yet met with.

It was a case ill-suited for cholecystotomy alone, but possibly this would have been the wiser course to pursue in view of the presence of an abscess in the falciform ligament. It was, however, natural to attempt the removal of the cause of this abscess, just as one prefers in dealing with appendix abscess to remove that organ if it can be done safely.

The patient went on fairly well for six days, her temperature being normal and the vomiting, which is so frequent an occurrence after difficult operations in the epigastric region where drainage-tubes have to be used, was the only trouble during this time. The gauze and tubes were removed and replaced by smaller tubes. On the seventh day it was found that the lower tube was blocked with blood-clot, and next day she vomited two or three ounces of bright blood. The wound looked somewhat red and angry but there was no suppuration. Her temperature, which had been normal, rose slightly (to 100° F.), the hæmatemesis recurred, and some hæmorrhage also took place through the tube. Various drugs were tried without avail, and as the the patient was rapidly losing ground I decided to explore the stomach. This was done, on the eleventh day after the operation, through a median incision. No peritonitis was found, On opening the stomach and examining the mucous membrane through a speculum I found three spots from which arterial blood was oozing. Two of these were cauterised; the third (a definite erosion on the posterior wall) was encircled with a fine purse-string suture. The opening in the stomach was sewn up with a double row of continuous suture.

Vomiting recurred, and after a day or two melæna showed that fresh bleeding was taking place. Continuous saline infusion was tried, but the patient became steadily weaker and died six days after the second operation, her temperature having risen to 102° to 103° F.

Post-mortem.—Slight amount of blood in peritoneal cavity; a hæmatoma in the site of the removed gall-bladder. A fine opening existed in the right hepatic artery where the cystic branch had come off. Blood-clot in falciform ligament (site of abscess). Local fibrous peritonitis. Great fatty degeneration of the liver with cholangitis (?). Slight atheroma of the aorta. Small aneurysm on right middle cerebral artery. Broncho-pneumonia and purulent bronchitis. Spleen large and soft: peri-splenitis. Great anæmia of kidneys. Dermoid cyst in right ovary. No melæna present.

The foregoing case may be summed up thus: Gall-stones and chronic cholecystitis, complicated by an abscess in the falciform ligament (*i. e.* subphrenic). No clinical signs of pyæmia. Operation: The abscess cleaned out and drained; pus thin and watery, found to be sterile on examination. Cholecystectomy—a prolonged and difficult procedure in this case. Subsequent vomiting on several occasions, and on the tenth day after the operation sharp hæmatemesis. Arterial blood vomited at intervals. On the twelfth day after the operation the stomach was opened and bleeding point (erosion) secured by ligature. Melæna and vomiting continued until death on the sixteenth day.

This was the first case under my care in which unexplained hæmorrhage into stomach or intestine had followed an operation, and the problems presented by it, both as to diagnosis and treatment, caused me grave anxiety. It may be noted at once that the decision to perform gastrotomy and the discovery of a bleeding erosion in the stomach wall seem to have had no effect upon the progress of the case, either good or bad. It can be justified in such cases, though hardly on the ground of actual curative results. Of the twenty-four cases to be summarised later this was, I believe, the only one in which operation was resorted to.

The chief problem with regard to this grave complication after operations is as to its cause. I commenced to collect records of other such cases that had occurred in the London Hospital, and found to my relief that Mr. A. J. Walton, our

Surgical Registrar, had already gone over the ground during the years 1907, 1908, and 1909.

A valuable and surprisingly large collection of cases had thus been noted, and I am indebted to my colleague, Mr. Sherren, for whom it had been prepared, for the opportunity of using it.

During the three years mentioned no less than twenty-four patients died after operation in the London Hospital with the complication of recent acute ulcers or hæmorrhagic erosions in the stomach or duodenum.

Three of these cases were examples of acute ulcers of considerable size; the following are brief particulars:

CASE 1.—William M—, æt. 32 years, admitted with diffuse peritonitis due to appendicitis. The abscess was drained and the appendix removed. Two days later he began to vomit blood in large quantities and died of acute anæmia.

Post-mortem.—A large recent ulcer was found on the lesser curvature. Much altered blood in stomach and intestines.

CASE 2.—Robert B—, æt. 37 years, admitted for severe lacerated wound of leg; this was cleansed and sutured, but spreading gangrene supervened. He died five days later. There had been melæna but no hæmatemesis.

Post-mortem.—An acute ulcer was found on the front wall of the first part of the duodenum.

CASE 3.—Joseph S—, æt. 21 years, admitted with appendix abscess. The appendix was removed and the abscess drained, but thoracic complications —empyema and subphrenic abscess—supervened. Both abscesses were drained, but the patient succumbed at the end of a month.

Post-mortem.—Two recent gastric ulcers were found on the anterior wall of the stomach near the pylorus.

Far more numerous are the cases of multiple erosions of the mucous membrane, some causing such persistent loss of blood as to bring about the patient's death, others merely assisting in this end.

Out of twenty-one cases the majority come into a very distinct group—appendicitis with septic complications, either localised abscess or diffuse peritonitis. This group includes 11 (*i.e.* 55 per cent.). In most the operation had consisted in drainage of the abscess with removal of the appendix, in a few only drainage had been employed, and in one the abscess had already opened and the "operation" consisted merely in dilating the aperture. The virulent nature of the septic process in this group of cases was shown by such complications as empyema, subphrenic abscess, purulent broncho-pneumonia, etc.

The only case that requires special mention is the following:

CASE 4.—Francis G—, æt. 50 years, admitted for right lumbar abscess of about three weeks' duration. This was drained; the pus was found to contain the *Bacillus coli*. The appendix was not seen. On the tenth day after operation severe hæmatemesis came on; this recurred (with melæna) repeatedly during the next five days, and gradually ceased.

No operation was performed (besides the original draining of the abscess). It must remain uncertain whether the cause of the abscess lay in the duodenum, the vermiform appendix or elsewhere.

This case is noteworthy, as it proves the possibility of recovery. During the three years I believe there was only one other instance of this, though of course it is probable that others may have been overlooked, *i.e.* those in which slight melæna only was present. Whether this is so or not we must conclude that the prognosis of well-marked hæmatemesis and melæna after an operation is exceedingly grave. Probably not more than 5 per cent. of the patients recover.

The reason will have been already inferred, that the gastric acute erosions or ulcers are due, not to any peculiar influence excited by the operation itself, but to the septic intoxication or infection which already existed. In the fourteen cases considered so far the septic condition was due to appendicitis in thirteen, to gangrene of the leg in one.

An analysis of the remaining ten cases bears out the importance of sepsis as a cause, though there are a few striking exceptions.

Cholecystectomy (for inflamed gall-bladder with calculi, in one case with subphrenic abscess, in the other with cancer of duodenum), 2; cholecystenterostomy, for cancer of the pancreas, etc., 1; supra-pubic cystotomy for papilloma, 1; external urethrotomy, peri-vesical suppuration, 1; cancer of colon, colotomy, 1; strangulated umbilical hernia, colotomy, abscess, 1; chronic peritonitis, intestinal obstruction, colotomy, 1; pan-hysterectomy for fibroids, followed by diffuse peritonitis, 1;

abdominal section for recurrent intestinal obstruction, ? ileus, nothing found, 1 ; total 10.

It will be seen that in only five out of a total of twenty-four cases (*i. e.* 20 per cent.) was a condition of sepsis absent at the time the operation was performed. In two or three of these five it rapidly followed the operation.

Two cases remain to be noted of special interest in which the existence of a chronic ulcer of stomach or duodenum was revealed after an operation for a totally different disease.

Case 5.—John N—, æt. 66 years, underwent four operations for epithelioma of the tongue. Seventeen days after the last operation, at a time when the operation wound had healed, he was attacked with severe hæmatemesis and melæna, from the effects of which he died.

Post-mortem.—A chronic ulcer was found on the lesser curvature of the stomach which had eroded the coronary artery.

There had been no symptoms pointing to the existence of the ulcer, and there was no septic cause.

Case 6.—Sarah G—, æt. 41 years. Abdominal hysterectomy (by Wertheim's method) performed for cancer of the uterus. The operation had to be incomplete owing to the extent of the disease. Eight days after the operation severe melæna came on.

Post-mortem.—A chronic ulcer was found on the posterior wall of the duodenum.

In both these cases the operation was performed for cancer, and in both it seems that the ulcer, whether duodenal or gastric, had been practically latent, that any symptoms due to it were wholly concealed by the major trouble.

It has been shown that previous sepsis is the main factor in the causation of gastric and intestinal hæmorrhage after operation, that the operation and the operator are alike guiltless save in the rare cases where the sepsis has followed the operation. Nevertheless in a small proportion of the cases (certainly under 20 per cent.), if some poison is responsible for the production of gastric or intestinal erosions it is not an ordinary septic one.

It will be interesting to test these conclusions by cases of similar nature where no operation has been performed, where acute ulcer or erosions have developed whilst the patient was under medical treatment for some other disease.

During the three years 1907 to 1909 the total number of post-mortems made at the London Hospital reached 3755. Mr. Walton collected from this list sixteen examples of the kind referred to.

Of these there were—multiple erosions of the stomach, 9 ; larger acute ulcers of the stomach, 6 ; ulcers of the duodenum and adjacent part of jejunum, 1.

None of these patients were admitted with symptoms referable to the stomach or duodenum. The main cause of death being the following :

Ulcerative endocarditis, 3 ; pyæmic pericarditis and endocarditis, 1 ; purulent bronchitis, etc., 2 ; purulent peritonitis, 1 ; severe burns, 2 ; tubercular peritonitis, acute cystitis, 1 ; acute septicæmia after child-birth, 2 ; diabetic gangrene, 1 ; total, 13.

In all of these the septic element is either obvious or may fairly be invoked.

In addition there are only three in which sepsis could be excluded, namely two cases of chronic nephritis terminating in uræmic coma, and one of isolated cancer of the gall-bladder.

One of the cases of burns deserves to be quoted.

Case 7.—Nathan D—, æt. 3 years, admitted with burns of the chest and abdomen, etc.

Post-mortem.—Minute hæmorrhagic ulcers of the stomach, due, as the microscope showed, to miliary abscesses in the follicles.

It is remarkable that the same proportion of non-septic cases (20 per cent.) prevails in the medical series as in the surgical one. An extreme lowering of the vital strength (*e. g.* from internal cancer, chronic nephritis) is the only cause of gastric erosion that seems to be common to these non-septic cases.

A similar profound asthenia is certainly present in most of the septic ones, and may be more important than any particular micro-organism in the blood. It is this that probably explains the absence of marked fever in many of the cases, the temperature and pulse-rate remaining near the normal, and thus the nature of the infection may be made obscure.

Conclusions.

(1) Hæmorrhage into stomach and intestine due to small ulcers or erosions may develop after an operation, usually abdominal, has been performed. In 80 per cent. of the cases a severe septic infection with secondary anæmia is present before the operation, the latter in itself having no direct influence upon the hæmorrhage.

(2) Similar lesions of the stomach or duodenum are met with in certain cases of disease, where no operation has been performed. Eighty per cent. of these cases are septic in nature.

(3) It is probable that a profound alteration in the blood, due in most cases to toxins of septic origin, is the chief factor in the production of these gastric erosions.

(4) Such hæmorrhage is of very grave significance, though a few patients recover from it.

(5) Operative interference does not appear to hold out much prospect of success in these cases, but further experience is needed on this point.

January 2nd, 1911.

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** Specially reported for the Clinical Journal. Revised by the Author.*

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THREE LECTURES
ON
CANCER OF THE RECTUM.

Delivered at the Polyclinic.

By FRED. C. WALLIS, M.B., B.C., F.R.C.S.
Surgeon to Charing Cross Hospital.

LECTURE I.

GENTLEMEN,—It is some years since I lectured here but I am very glad to do so again, and the subject that I have taken for my lectures is that of rectal carcinoma. The more I see of this disease the more I recognise that not only all surgeons but all medical men ought to know the latest ideas about it. I do not mean that they should acquaint themselves so much with the operative technique —that appeals, practically speaking, to only a few —but with points that are more generally interesting, such as the question of diagnosis and treatment, and particularly the question of after-treatment. But I wish to say as a preliminary that I am not going over the stereotyped old ground more than I am obliged. I want to discuss to-day any new ideas which I may have gathered in my experience and which may be of use to you.

The *early* recognition of rectal carcinoma is the first essential thing necessary to enable us to get the best results in treating this too prevalent disease—results which it is most difficult to obtain, for two main reasons. First there is the fact that as a rule the patients do not complain of any particular symptoms and do not seek advice until the disease is far advanced. That I am sure will be the experience of a good many of you. Secondly, it has been too often the custom in the past for the medical man to take a great deal for granted from the patient over some rectal trouble and to prescribe without any proper rectal examination. I believe that early rectal examination has now become far more the rule than it was formerly.

It would seem almost a pity that people generally are not made more conversant with the fact that

even obscure rectal symptoms after a certain age may have an important significance. But I think more knowledge concerning this particular thing will come with the general education—knowledge, that is, of the fact that if anything is wrong it is much better to have it seen to and put right as soon as possible.

In reference to the *situation* of rectal carcinomata the common situation is three or four inches from the anus in the infra-peritoneal portion of the rectum. About 50 per cent. of the cases occur in this region. The next common situation is about two inches from the anus, and after that comes the sigmoid, and these latter cases are absolutely supra-peritoneal. The least common situation for a growth is the anal region, which only accounts for about 5 per cent. In these latter cases pain is the most predominant, and is, comparatively speaking, an early symptom, and therefore these cases come under one's care much sooner than those where the growth is situated in the other regions mentioned.

Generally speaking, however, it is an unfortunate fact that pain is an uncommon or late symptom in this disease, and although no doubt people suffering from carcinoma feel a sense of discomfort, yet it is unusual for them to take notice of it sufficiently to call in advice. On the other hand, I saw this morning a patient who had been visiting his medical man for six months with rectal symptoms, and yet through all that time no rectal examination was made. That patient has a well-advanced infra-peritoneal cancer of the rectum. The growth is even now possibly removable, but six months ago an operation could have been performed with of course much better prospects of recovery than at present exist.

Dealing with the ordinary symptoms, pain, hæmorrhage and discharge, these are all sufficiently obvious and should be followed by careful rectal examination. If nothing can be *felt*, then a sigmoidoscopic examination should be made after the patient has been properly prepared. I have no doubt you are all conversant with sigmoidoscopy here. I do not want to dwell on the ordinary symptoms any longer, but would now particularly draw your attention to the more obscure symptoms which occur in people after a certain age, and which I am convinced should be regarded with great suspicion until all possibilities of the presence of rectal growth have been cleared up.

I am going to bring before you two or three cases from which I have learnt a great deal. There are many obscure symptoms—I dislike the word "obscure," but what I mean is that they are not such definite symptoms as pain and hæmorrhage and discharge—and I think I shall best illustrate my point by giving you the history of three cases.

The first of these is probably the most curious and is still under discussion. The case is that of a single lady over 60 years of age. I was asked to see her because she had some pain in her rectum and was also markedly hypochondriacal. All sorts of treatment had been tried in her case, but with no result. The medical man whom I saw her with had made rectal examinations at various times, and he said, "There is a curious line in this woman's rectum that I do not understand." And on examination there was a white linear scar with a sharp edge, which ran around the bowel for about 1½ in. The scar was particularly what I was consulted about, and it had been so persistently painful that I said I would remove it if the patient wished. I removed the whole circumference of the mucous membrane, as I invariably do in these cases. I make it a point to have all specimens examined microscopically, and as a mere matter of curiosity this specimen was sent to Dr. Ernest Shaw. He pronounced it to be a colloid carcinoma. This was a shock to me, and I said I did not believe it to be a carcinoma. He said, "I can assure you it is, Mr. Wallis, and I can demonstrate it." Of course Dr. Shaw knows more about the microscope than I do or ever hope to do, and he has worked a good many years at it. This is a micro-photograph which was taken of this particular case. The patient made a rapid recovery and went away into the country. Another surgeon happened to be in the neighbourhood where she was living, and about three weeks after the operation her local doctor said to him: "I wish you would come and see ——. They say she has carcinoma, and really I cannot find any evidence of it." The surgeon, for whom I have great respect, went and examined her, and he, too, could not find any clinical evidence of carcinoma. But this was only natural after the operation. The section was shown to him, and he took it to some microscopists, who, after examining it, said they could not see any evidence of cancer. Now, look at the micro-photograph. You see in various places

the cells are breaking down and undergoing colloid degeneration. That was what Dr. Shaw demonstrated to me. The last time her London physician went down to see her he said he felt a distinctly granular feeling on the mucous membrane, but time alone will show the result of this unusual case.

The next case was that of another lady, who was also no longer young. She was a married woman, and she had suffered for some time from mucous discharge from her bowel, but it was not considered a serious trouble. She had not been examined by a doctor *per rectum*. When I saw her she had a definite tumour in her rectum, about $1\frac{1}{2}$ to 2 in. from the anus. It was a sessile adenoma with a smooth outline but rather uneven. When I examined her in my room I thought seriously of it. It had a curious granular feeling which in ordinary mucous membrane is not felt, and I thought that was a very suspicious indication of commencing carcinoma in an innocent or simple growth. Bear in mind that simple growths are very apt, after a number of years, to become carcinomatous. I concluded that this case showed an early typical condition of that kind, and so I removed the growth in the same way as I did in the other case just mentioned. The whole of the mucous membrane was taken out right round the anus and the healthy mucous membrane was brought to the anus and stitched down. The microscope revealed the fact that the simple tumour had become malignant. Here are micro-photographs of the various sections showing the condition of the cells. This woman made an excellent recovery and I think the prognosis is quite good.

The third case was that of a lady sent to me from South Africa with a malignant growth about $4\frac{1}{2}$ in. up the rectum. About three weeks ago, when she came to this country, I saw her in a nursing home. On examining her I felt sure she had a villous tumour, an innocent growth, which had become malignant. The importance of this case is that the prognosis is so much better than in the case of a genuine carcinoma.

The points which led me to this diagnosis were: (1) Healthy mucous membrane extending right up to the edge of the tumour; (2) the character of the edge of the tumour, as compared with— (3) the character of the centre of the tumour.

Here, again, the microscope supported the diagnosis that a malignant growth had developed upon an innocent tumour. There are other instances in other parts of the body which I could give you where this has occurred. But the practical point here is that when there is in the rectum an innocent growth I am perfectly certain that it should be removed whilst it remains innocent. One should not wait until these growths become malignant. If a person has within easy reach a definite growth of a benign character and it gives rise to any symptoms whatever which call your personal attention to it, then without unnecessarily frightening the patient I think it is one's obvious duty to suggest that the growth should be removed. The reasons for my saying it is one's duty I have just given in the history of the three cases I have quoted to you. Those are the points which I particularly wanted to bring out to-day. They are not new, but I think they should make one think rather more deeply with regard to one's duty in advising treatment for these innocent tumours. They may have malignant elements long before they show them to you from a clinical standpoint. I do not think there are any other definite symptoms which I need bother you about, and I do not want to weary you about pain or hæmorrhage or discharge, because they are so obvious.

Having established the fact that there is a growth, the next point is whether it is operable or not. That is not so easy as it sounds. The old idea that if a growth is fixed it is inoperable, and if movable it is operable, does not altogether hold. In the first place it depends where the growth is. For instance, you can have a growth of the rectum which is fixed posteriorly, but not anteriorly, the growth having orginated on the posterior surface. This will be made evident under an anæsthetic, and it will be possible to remove the growth, whereas formerly one would have said, "It is fixed; I cannot touch it." On the other hand, there may be a growth which to all intents and purposes, as far as examination goes, is freely movable and comes down quite easily. But as a matter of fact this may not be so at all, but the growth is fixed anteriorly and simply turns on its axis—and this is especially the case when the growth is attached to the bladder, uterus or vagina. So that it does not do to say, when you are examining a growth, "This growth is quite freely movable and therefore removable." It is probably far more likely to be removable, but you cannot say definitely that it is until you have opened the abdomen. It is not right to embark on a large operation on the rectum without being certain of your surroundings. The abdomen should be opened and the growth examined from above as well as from below. If the patient is in the Trendelenberg position one can examine the whole thing thoroughly. I did not mean to get as far as that to-day with my lecture, because it rather goes into another subject which I propose to deal with to-morrow.

January 9th, 1911.

A CLINICAL LECTURE

ON

ACUTE AND CHRONIC INFLAMMATORY LESIONS OF BONE.

Delivered at the Hospital for Sick Children, Great Ormond Street.

By G. E. WAUGH, M.D., B.S., F.R.C.S.,
Senior Surgeon to Out-Patients to the Hospital, and Assistant Surgeon North-West London Hospital.

LADIES AND GENTLEMEN,—I have chosen the title for my lecture this afternoon—"Acute and Chronic Inflammatory Lesions of Bone"—with the direct intention of avoiding so far as possible the terms "acute periostitis," "osteitis," "osteomyelitis," etc., which have a widespread currency. They are inaccurate and misleading, they are founded upon a misconception of the layers involved in an acute inflammation of bone in children, and they are responsible in consequence for the many failures that have been recorded of the treatment of this condition. Moreover they are responsible for an attempt to distinguish by clinical methods the infected layer of the bone, with a consequent modification of the treatment that must frequently end in disaster. Let me say at once that only a direct inspection of the layers of bone themselves can offer reliable evidence as to whether they are involved or not in an acute inflammatory process, and that the experience of a large number of cases during the last eight years has shown me that where the infection has started in an epiphysial line—as it always does in children—all the layers are involved from the onset, and that therefore surgical treatment must be accorded to all the layers. If you will refer to any standard text-book of the present day on surgery you cannot help being impressed by the note of pessimism that pervades all the writings upon this subject. There is an atmosphere of gloom that envelops the discussion of the patient's chances of escape both from the immediate and remote sequelæ of this condition, and the most frequently recurring illustration in the text-books is of an amputated limb, the subject of "chronic osteomyelitis," which has supervened as a hopeless legacy of immediate recovery from the acute inflammation. The immediate causes of anxiety appear to be the death of the patient from the primary lesion, and the remoter causes to be the appearance of metastases in other regions such as the pericardium, pleura, brain, meninges or other bones with often fatal results, or the supervention of amyloid disease or chronic osteomyelitis. Now with regard to the second group, *i.e.* the remoter sequelæ, I have not seen a single instance in the whole of my experience during the last eight years, and I have many of the cases here to-day to show you that not only can surgery effect a saving of life, but that in the course of a few years' time there is no appreciable structural alteration of the skeleton as the result of this disease. One case in particular illustrates this point—a girl, now æt. 10 years—from whom I removed simultaneously, about five years ago, nearly the whole of the femur and tibia of the right leg for an acute purulent infection. Both bones have completely regenerated, there is no shortening of the limb, and but for the scars extending over nearly the whole extent of both segments of the limb she is to all appearances a normal, healthy individual, as you can perceive for yourselves.

With regard to cases ending fatally, it is of interest to note that they present a definite clinical picture, the salient feature of which is the great disproportion between the extent of the bony tissue involved in the infection and the well-marked signs of a profound general toxæmia. Such a disproportion can only mean that the virulence of the infective agent is extremely high or that the resistance of the patient is extremely low, or that both conditions are associated together. Evidence of a serious general toxæmia would be usual and expected in those cases in which the medullary cavity of one or more long bones was full of pus, and would not of necessity be of grave significance. But when that evidence is present in association with a lesion that at the most does not involve more than one third of the shaft of a long bone and the lesion has not been present for more than twenty-four hours, then the prognosis appears to be hopeless. Within a few hours of the onset cerebral symptoms develop. In the two cases in my series one was delirious within twelve hours and the other semi-comatose within twenty-four hours of the commencement of the illness. The pulse-rate was uncountable, and although immediate operation was performed both patients died within the next twenty-four hours.

In both cases only the lowest fourth of the tibia was involved. There was no pus formation and only a chocolate-coloured fluid exuded from the bone, which was freely drained for the whole extent of the involved area. It is possible that immediate amputation might have saved them, and there is justification for trying it in similar cases, which can easily be recognised by a consideration of the details I have just described to you. But in view of the fact that the duration of the whole illness from start to finish, did not exceed forty-eight hours, I am inclined to think that the hope for these cases in the future lies in the development of bacterial therapeutics rather than in surgical procedure.

In descending order of severity the other acute cases may be grouped as those in which a long bone and a joint are involved, those in which more than one bone are simultaneously involved, and lastly, those in which only one bone is involved. But in all these cases the immediate and remote prognosis is good. The diagnosis, as a rule, is simple, but because of the frequent antecedent history of an injury these cases are sometimes mistaken for fractures or sprains. The main points of distinction are these: The appearance of the segment of the limb is often characteristic. Its natural contour has become obliterated, and it has assumed that of a symmetrical cylinder. This is due to the fact that the inflammation is situated in the central axis of the limb and is spreading towards the surface in all directions, so that the surface tissues seem to be floated up upon an underlying water cushion. Palpation confirms this by proving that the enlargement of the limb is not due either to an inflammation or œdema of the subcutaneous tissues. Pain is present continuously, and deep-seated tenderness can be elicited by pressure *over the whole* of the swollen area. In a fracture, pain, as a rule, is absent when the limb is at rest, and tenderness can only be elicited by pressure over the site of the injury or by the application of a force to the limb tending to mobilise the fragments. The pulse-rate is markedly accelerated. The temperature may be high in both instances and is of little value in establishing a differential diagnosis. Lastly, if the epiphysial line in which the inflammation has started is subcutaneous the overlying tissues are early involved and a small red area of superficial inflammation is present. In a boy—whom I shall show you afterwards—from whom I removed almost the whole of the iliac portion of the innominate bone and the ulna at the one operation four years ago, the only physical signs of the involvement of the innominate bone were a slight fulness of the buttock and a small red and excruciatingly tender area over the anterior superior iliac spine—that is, at the part at which the epiphysial line of the crest of the ilium was nearest to the surface. From this line pus had extravasated beneath the periosteum, covering both the pelvic and gluteal surfaces of the iliac bone right down to the margin of the acetabulum and between the two layers of the bone itself. He made an uneventful recovery, and, as you will see both from an examination of the boy himself and from skiagrams, there is an almost complete regeneration both of the ulna and of the innominate bone.

In all cases of inflammation requiring treatment by operative procedure this is a safe surgical principle upon which to found the line of treatment, and it has been followed consistently in all these cases. Find the greatest diameter of the inflamed area and then establish drainage for the whole length of that diameter. By such means only can the desired escape of the maximum amount of the infective agent in the minimum amount of time be achieved. As a consequence the regenerative powers of the tissues, which have only been depressed in the earlier stages of these cases, are enabled to survive instead of being completely destroyed by prolonged contact with the infective agent, which must necessarily be the outcome of less adequate and quite ineffective methods of drainage. The incision through the skin to the periosteum must extend over the whole length of the swollen segment of the limb. The incision in the periosteum must lay open the whole of the periosteal sheath as far as there is evidence of periostitis; and lastly, the medullary cavity of the bone must be opened and the bone guttered to the full limits of the spread of the infection in the medullary cavity. The incisions from the surface of the skin to the centre of the bone as a rule become progressively smaller, so that the extent to which the bone is opened is generally rather less than the extent of the opening in the subcutaneous tissues. On no account must the opening of the medullary cavity of the bone be delayed. It is impossible to tell except by actual inspection

whether it is already infected or not. In nearly all cases it is already extensively involved, and in the very few cases in which it has appeared to be healthy on inspection in the course of operation, no subsequent infection of the exposed cavity has occurred. Ample protection is afforded to it by the free surface drainage provided. But a melancholy toll of lives has been exacted from the omission to open the bone at once, in the hope that the subsequent course of the case would reveal whether the medulla was involved or not. When to the followers of that traditional course of procedure the indications do appear clear the patient is generally in a dying condition. The amount of bone taken away in the process of guttering is generally from half to two thirds of the total circumference of the bone, and the medullary cavity is wiped clean with sterile gauze as far as it has been laid open. I regard it as important to leave at least one third of the circumference of the bone in the infected area, as the subsequent course of events proves that even when drenched by pus the bone is not dead, but that its vitality is merely depressed. The wound is then washed by warm saline solution and sterilised gauze dressings applied. No antiseptics are allowed to touch the tissues under any circumstances whatsoever, and it is difficult to see how any bone already invaded by bacteria could withstand the destructive action of pure carbolic acid and yet retain the power of regeneration without extensive sequestrum formation. The subsequent dressings for about a week are aseptic gauze wrung out in sterilised salt solution, and after that plain dry gauze. After seven to ten days from the date of the operation the exposed bone appears as a white mass at the bottom of the wound. It is not dead, and merely requires the dressings to be conducted with perfect cleanliness to prevent re-infection. I find this phenomenon of white bone being regarded as dead bone so widely spread that only with difficulty is each new house-surgeon restrained from trying to pull it out. But in a short time granulations appear springing up through the interstices of the bone, and the wound is generally healed completely in about two months' time. The healing process, moreover, is unaccompanied by sequestrum formation, and the sequel of chronic osteomyelitis is entirely unknown. A few flakes of bone about the size and thickness of a finger-nail as a rule make their way to the surface. In about a year's time a sturdy new bone will be found to have formed over the whole site of the operation area. I have already referred to two of the cases awaiting your inspection nearly five years after the date of their operations, and there are several others also for you to see in which the tibia, the lower jaw, the pubic bone, and the vertebral column have been treated with equally satisfactory results, both immediate and remote.

Acute purulent inflammations of the vertebral column are rare, and in 1900 Hahn was only able to collect forty-one cases altogether. The mortality from this condition is 60 per cent. The one case included in my list was in a boy, æt. 10 years. He had a fluctuating swelling extending from the superior angle of the scapula to the crest of the ilium. All the signs of a complete transverse pressure lesion of the cord at the level of the eighth thoracic nerve were present—there was no sphincter control, and trophic sores had already formed over the bony prominences of the lower limbs. The superficial abscess was laid open for its whole length, and a tract was then found passing through the erector spinæ muscles in the mid-thoracic region. These were drawn aside and then four laminæ removed from the right side. Pus gushed under pressure from the extra-dural space, and after this had been cleansed the pedicles of the vertebræ were nibbled away and the primary focus of infection in the bodies of the vertebræ freely gouged out. Within a fortnight from the date of operation he had acquired control of his fæces, and sensation began to return in the lower sacral segments two weeks later. He was able to move his big toe at the end of six weeks, and at the end of six months he was able to walk with a slight ataxic gait. At the end of a year he appeared to be perfectly well.

In performing these operations it is important to remember that the epiphysial lines in which the infection starts are usually outside but closely related to joint capsules. It would be a great disaster to inadvertently wound the joint capsule under such circumstances. Fortunately the spread of the disease is nearly always along the diaphysis away from the joint and not in the epiphysis towards the joint. This is probably determined by the absence of a true medullary cavity in the epiphysis, so that its more compact tissue offers a greater resistance to the spread of infection. In

the girl with simultaneous infection of both the femur and tibia of the right leg, the starting-points were the lower epiphysis of the femur and the upper epiphysis of the tibia, but the intervening knee-joint was not infected. In two of the cases which I shall show you in which the primary focus was in the lower epiphysis of the tibia, the ankle-joint was also full of pus. This was freely drained at the same time as the tibia was guttered, and it is of interest to note the freedom of movement they both possess two years after the date of operation. You will see, then, that the proper treatment of chronic osteomyelitis of a simple nature is preventive. For those cases which are unfortunate enough to have acquired that condition before coming to you for treatment, most surgical procedure is tedious and disappointing. Many methods of drainage have been advocated—metal tubes have recently found warm supporters—but the alternate layers of liquefied bone tissue and areas of sclerosis preclude the establishment of appropriate drainage over the whole area. Personally I am content to evacuate the contents of the abscesses as cleanly as possible, and then treat the limb by Bier's passive congestion method combined with the administration of vaccines made from the patient's own infection.

Chronic osteomyelitis of a syphilitic nature involving the whole of a long bone is also a difficult and unsatisfactory lesion to deal with. Operative interference seldom does any good unless there is obvious sequestrum formation. Reliance must be rather placed upon prolonged inunctions of mercurial ointment combined with the administration of iodide of potassium. Arsenic and sulphide of calcium also have some use in this condition.

Localised tubercular lesions in bones, on the other hand, respond most satisfactorily to operative treatment. The infected area must be as freely excised as in the cases of acute simple infections. But the wound must be closed and not drained. As a rule no second operation becomes necessary. Many cases of tubercular osteitis in children follow injudicious attempts to straighten green-stick fractures. In one case that I am showing you a green-stick fracture of the ulna had been straightened, and ten weeks later the child was sent to the hospital with advanced tubercular osteitis of the lower half of the shaft of the ulna. I guttered it in the manner I have described, closed the wound, and now one year later you will see she has almost complete regeneration of the involved area. Green-stick fractures require no treatment beyond protection by appropriate splints from further injury. Spontaneous straightening will occur as a natural process, whilst the unnecessary and often ineffective forcible "straightening" may be attended by the disaster that I am bringing before your notice. These cases are not uncommon features of a large out-patient department, and the responsibility for their occurrence must be laid to the discredit of an empirical and traditional method of treatment.

January 9th, 1911.

Erratum.—In the article by Dr. Seymour Taylor published in the CLINICAL JOURNAL, December 28th last, on page 182, line 32, the word *vivisection* should read *venesection*.

Bloodless Treatment of Phimosis in Childhood.—Stuhl describes a substitute procedure for circumcision which may be done without narcosis, and the production of a wound surface with persistence of a so-called preputial apron. The operation is not only bloodless, but no cutting instruments are used. The foreskin is gradually pulled back over the glans, one sitting or several being required. After the meatus is exposed a blunt sound may be used to separate adhesions. To avoid para-phimosis the prepuce must then be replaced. A very narrow preputial orifice must be dilated progressively for several days. If, despite this resource, paraphimosis result from the next exposure of the glans, this must be at once reduced, care being taken to avoid contact with the newly exposed surfaces. From four to eight days are required for the entire treatment. A few slight tears in the prepuce may be unavoidable and should be treated with some antiseptic dusting powder. Phimosis is often accompanied by an unusually short frenulum, which should in such a case be incised.—*Med. Record*, vol. lxxviii, No. 26.

THE ACUTE ABDOMINAL CASE FROM THE STANDPOINT OF A COUNTRY GENERAL PRACTITIONER.*

By F. W. LANGRIDGE, M.R.C.S., L.R.C.P.

MR. PRESIDENT AND GENTLEMEN, — I think I may say, without fear of contradiction, that within the whole realm of medicine and surgery there is no one class of case of greater importance than that of the acute abdominal case. It is of importance to the consulting physician, it is of still greater importance to the operating surgeon; but I say without hesitation that, so far as the welfare of the patient is concerned, it is of infinitely the greatest importance of all to the general practitioner, for it is under his care in the first instance that these cases usually fall, and on his ability to recognise the gravity of the case, and to take prompt measures for placing his patient under the most favourable conditions for treatment, it is no exaggeration to say that, in a large majority of cases, the life of that patient absolutely depends. It is difficult to conceive of a graver responsibility, and it is a responsibility that may be thrust upon any one of us, at any hour of the day or night; for these cases are no respecters of times, persons, or places; they come upon us like a bolt from the blue; Dives in his palace, and Lazarus at the gate, are both liable to these abdominal catastrophes. As some one has said, in speaking of another subject—and the remark applies equally well to this—"A touch of colic makes the whole world kin."

Such being the case I do not think that I, as a general practitioner, need apologise for bringing forward this matter for discussion, seeing that to the general practitioner, more than to any other member of the medical profession, it is a matter of vital importance, and if not properly understood is, I fear, sometimes of fatal import to his patients.

Now, I think we are all agreed that the one thing above all others which tends to a successful termination of these cases is that of early diagnosis. We must learn to read the handwriting on the wall—I might say on the abdominal wall—before it is too late to render effective assistance to our patients; and although in some cases, such as those of external strangulated hernia, this is a comparatively easy matter, other cases are surrounded by so many snares and pitfalls that even the very elect may be, and have been, deceived. As Mr. Lockwood says in a lecture of his on "The Period of Repose in Acute Appendicitis"—not, I am glad to say, without apologies to the poet whom he paraphrases—"The wisest of men have erred, and by appendicitis been deceived, and shall again, protest they ne'er so wisely."*

Now when I say "diagnosis," I mean a strictly clinical, not a pathological diagnosis—a diagnosis that can be made by the bedside of the patient, before the abdomen is opened; for although such conditions as internal strangulation into the foramen of Winslow, or through a hole in the mesentery, embolism of the superior mesenteric artery, or acute thrombosis of the portal vein, to say nothing of acute hæmorrhagic pancreatitis, are matters of considerable interest to all of us, and of great importance to the operating surgeon, seeing that it is hardly possible to diagnose these conditions, with any degree of certainty, before the abdomen is opened, they are merely of secondary importance to the non-operating general practitioner. And by the non-operating general practitioner I do not mean the man who never operates on these cases, but one who, not, of course, through any want of ability, but through want of opportunity, has been unable to acquire that experience and that degree of operative dexterity possible to those who are constantly seeing and operating on such cases, and one, therefore, who prefers that his patient, when possible, should be afforded the additional advantage of being operated upon by a skilled abdominal surgeon. And when one considers the cosmopolitan character of the knowledge necessary for the satisfactory equipment of the general practitioner as compared with what one might call, with no wish to be unkind, the provincialism of the specialist, I think you will agree with me that the less the former burdens his mind with non-essentials the better enabled will he be to concentrate his attention upon essentials, and the one essential in such cases as I have cited is that he should be able, at the earliest possible moment, to recognise the condition of acute intestinal obstruction resulting from these lesions.

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* CLINICAL JOURNAL, November 17th, 1909.

And now to come to close quarters with our acute abdominal case. Unfortunately, but I fear unavoidably for purposes of reference, in most of the clinical lectures, to say nothing of the text-books we read on this subject, the answer to the riddle is given before we have heard the question. A lecture on renal or gall-stone colic, on perforated gastric or duodenal ulcer, on acute intestinal obstruction or perforated appendicitis, tells us the diagnosis before we have heard the symptoms. Nature, however, is not so considerate; she sets us the problem and we have to work out the solution as best we can, and the way she sets us the problem is somewhat as follows:

After a hard day's work, when we have settled down to a quiet smoke, have unlaced our mental stays, and are looking forward to the enjoyment of that well-earned reward of "something attempted, something done," the night-bell rings. I do not know how it is with you, gentlemen, but in my part of the country people nearly invariably ring the night-bell after 10 o'clock, whether the house be closed or not. A message is brought—"Will you please go and see Mr. or Mrs. So-and-so at once? She has been taken suddenly ill and is in very great pain." Well, you make a remark more or less appropriate to the occasion, re-lace your mental stays, which, however, you find will not come together quite as well as they did in the morning after a good night's rest, and proceed to the address given. Here you find things in some confusion. The wife, if the patient happen to be her husband, or one of the children, appears in a more or less becoming dressing-gown, probably with her hair in Hinde's curlers. This may seem a trivial matter to mention, but when a woman, at any rate in a certain class of society, becomes indifferent to her personal appearance you may be certain that there is something serious the matter. I always feel hopeful in a case of severe illness when my patient, if a woman, begins to dress her hair carefully, or, if a man, expresses a desire to have a shave. You may say, "These are mere straws"; granted, but they help to show which way the prognostic wind is blowing.

On entering the bedroom you are greeted with a smell of turpentine, or you see a basin in which it does not require the genius of a Sherlock Holmes to detect that a linseed poultice has recently been made. Probably there is another basin or other vessel—generally the other vessel—containing some vomit.

The patient is in bed, either rolling about and groaning in agony or lying suspiciously still on his back, with his knees drawn up and scarcely daring to breathe, but his face expressive of the pain he is suffering. On approaching the bed, in a large number of cases, you perceive an odour of brandy, for it seems to me that the one rule which the laity feels impelled to follow in the presence of any medical or surgical emergency is—"When in doubt, give brandy."

Of course there are many variants of this picture, but the one thing common to all is the sudden onset of acute abdominal pain, either in a patient previously in apparently robust health or in one who has been suffering for some time from symptoms of "indigestion" or from obscure abdominal symptoms, such as flatulence, distension, griping pains, and constipation, alternating with loose actions of the bowels; or, again, in one whom we have been attending for appendicitis or typhoid fever, or, if a woman, for some affection of the sexual organs.

Now it is in some such way that Nature presents the case to us, and it is our duty to discover the *approximate*—and I emphasise that word, gentlemen—the *approximate* cause of this sudden onset of acute abdominal pain, and like the Patriarch of old, we must wrestle with that case and must not let it go until it has blessed us by giving us an answer to our inquiry, even if by so doing we should run the risk of sustaining a dislocation of the femur; and we must never allow that one cardinal fact of the sudden onset of acute abdominal pain to be pushed into our mental background by the occurrence of subsequent events, the result either of the natural course of the disease or of our well-intentioned but misguided efforts at treatment.

Given, then, such a case as I have postulated, it will be well, first of all, to define sharply the narrowest possible limits of our investigation. As I have said before, our object is to ascertain the *proximate* cause of this sudden onset of acute abdominal pain. When one reads a treatise, say, on intestinal obstruction, one's mind becomes so bewildered by all the possible and the many seemingly impossible causes of this condition—for here, indeed, truth is sometimes stranger than

fiction—that one feels it is a subject beyond the grasp of the ordinary human intelligence, and capable of being adequately dealt with only by the sublimated wisdom of the Harley Street specialist. When, however, one narrows down the inquiry into ascertaining the presence of intestinal obstruction, irrespective of the underlying cause, one approaches the subject in a more hopeful frame of mind.

Now, in my opinion, the all-important point for the general practitioner, when face to face with one of these cases of acute abdominal pain, is to ascertain whether the cause of this pain is extra-abdominal or intra-abdominal, and, if the latter, whether the case belongs to the class of so-called medical colics, medical at any rate in their earlier stages, or falls within the group of what have well been called abdominal catastrophes—that is to say, acute abdominal conditions, in which the life of the patient can probably be saved only by surgical intervention. Of course it is well, where possible, to carry our refinement of diagnosis further than this, but for purposes of treatment this is the one and all-important point.

As regards the extra-abdominal causes, taking the nervous system first, we must always bear in mind the possibility that the patient may be suffering from one of the gastric crises of tabes dorsalis, a disease which, in its earlier or pre-ataxic stage, when its recognition is not always an easy matter, is so fond of masquerading under the guise of some other complaint, so that even the most skilled observers have occasionally been deceived. The presence of Argyll-Robertson pupils, the absence of knee-jerks, or a history of lightning pains, would naturally suggest this possibility to us.

Coming next to the chest, pleuro-pneumonia or diaphragmatic pleurisy may give rise to pain referred to the hypochondrium or to the epigastric region, and sometimes extending even as low as the iliac fossa, and seeing that irritation in the course of, or at the origin of, a sensory nerve gives rise to pain referred to its peripheral distribution, this is no matter for wonder when one considers the origin, course, and distribution of the lower intercostal nerves, whether we regard the pain in these cases as a referred or a reflected pain. In like manner irritation in the course of, or at the origin of, a motor nerve will cause contraction of the muscles which it supplies, and the intercostal, being mixed nerves, it is not surprising that rigidity of the abdominal muscles should also be present in these cases. When, in addition, we get vomiting, as not infrequently happens, due, I suppose, to some irritation of the branches of the pneumo-gastric, we have before us a symptom-complex highly suggestive of intra-abdominal mischief. Within the last twelve months I myself have come across two cases, one in an old man and the other in a child, in which the initial symptoms—pyrexia, pain in the epigastric and right hypochondriac regions, and vomiting—pointed to some gastric or hepatic disturbance, but in which, the pulse-respiration ratio being greatly disturbed, I reserved my diagnosis, and although at the time there were no physical signs of lung mischief, in both unmistakable signs of lobar pneumonia ultimately developed. I am glad to say that, in spite of treatment, both cases made a satisfactory recovery.

Then, too, a blue line on the gums and the occupation of the patient may put us on the track of lead colic.

Another cause which may give rise to symptoms suggestive of intestinal obstruction, and of which I was unaware until recently, is the taking of oils, such as copaiba, for gonorrhœa. With regard to this Mr. Lockwood says:* "People taking these oils often suffer from the most violent pain in the abdomen, cease to pass any wind or fæces, and may vomit." In illustration of this he cites two cases, the second of which is so instructive, in more ways than one, that I trust you will pardon me if I quote it rather fully. The patient, an officer, on whom Mr. Lockwood had performed the radical cure, after leaving Mr. Lockwood's care went to stay at an hotel. In response to an urgent message Mr. Lockwood found, on arrival, that his patient, while straining at stool, had felt something go wrong in his inside, had suddenly been seized with the most violent pain in his abdomen, was suffering from constipation, and had not passed wind for some time, symptoms suggestive of something having gone wrong in the region of the operation. Mr. Lockwood goes on to say: "However, I had heard him casually remark to a friend that he could not partake of any alcoholic beverages, because he was suffering from inflammation of the kidneys. I thought that was curious, because I knew he had

* Clinical Journal, November 4th, 1908.

not inflammation of the kidneys. I said to him, 'What was the meaning of the inflammation of the kidneys which you talked about the other day?' He said, 'I have just recovered from gonorrhœa.' 'What are you taking for it?' and he produced capsules which he said contained copaiba. I concluded that the touch of obstruction, distension, and pain were due to the copaiba which he was taking." I do not know whether the occupation of the patient was of any assistance in helping Mr. Lockwood to arrive at such a diagnosis, but, seeing the number of our soldiers and sailors who, unfortunately, suffer from gonorrhœa and other venereal diseases, one feels almost inclined to agree with a suggestion I saw made, some time ago, that these complaints should be included among the diseases of occupation.

Such cases as these should impress upon us the important fact that *although a patient may complain of pain in the abdomen, it does not necessarily follow that the cause of that pain is intra-abdominal.*

Coming next to the intra-abdominal causes of what has graphically, but hardly euphemistically, been called the "acute belly," these, as before mentioned, may be divided into the medical colics and the true abdominal catastrophes. The principal medical colics are, of course, those due to acute gastric or intestinal dyspepsia, renal and gall-stone, and, may I add, appendicular colic, and also that due to muco-membranous colitis, for Mr. Lockwood tells us, in the paper already cited, that he has come across numerous cases of this disease which have been operated upon for appendicitis. The passage of the membranes found in these cases gives, of course, the clue to the diagnosis. We must be careful, however, not to be misled by symptoms pointing to one or other of the above-mentioned causes, for the fact that a patient is the subject of tabes dorsalis, or is suffering from lead poisoning, or has previously had an attack of renal or gall-stone colic, does not therefore render him immune to a perforation of his appendix or acute intestinal obstruction, and hence there is a danger in these cases of following up a wrong scent and making what might well be called a "red-herring diagnosis."

And now as to the genuine abdominal catastrophes; reduced to their simplest possible terms these may be considered under the two headings of acute intestinal obstruction and peritonitis, for I do not think it necessary to make a separate class for intra-peritoneal hæmorrhage, though, I suppose, I ought to guard myself by making a distinct class for fancy cases. Of course, neither acute intestinal obstruction nor peritonitis forms an absolutely distinct class, for the former, if not relieved, may give rise to the latter, and, on the other hand, peritonitis may be the cause of acute intestinal obstruction; and again, I use this expression in a strictly clinical sense as indicating the inability of the intestine to propel any of its contents, whether this be due to some mechanical obstruction, paralysis, or other cause, which can possibly be discovered only after the abdomen is opened. Fortunately, the steps necessary for ascertaining the presence of either of these conditions will, in a great many cases, help us to arrive at a recognition of its ultimate cause.

Hence, our investigation is narrowed down to ascertaining whether the sudden onset of acute abdominal pain, in any particular case, owes its origin to some extra-abdominal cause, or, this being eliminated, whether it is the result of one of the medical colics or is due to the presence of acute intestinal obstruction or of peritonitis.

I will say nothing as to the differential diagnosis of these conditions, for it would take far too long, besides which the pain, tenderness, rigidity, absence of liver-dulness and so on are all fully described and discussed in clinical lectures and text-books; but I cannot help feeling—at any rate, it is true in my own case—that it is many years after leaving hospital before one develops what might be called a "medical perspective." Of course, I am not referring to men who have held the higher appointments, but to those who, like myself, after many years of study—more or less intermittent—have managed to pass their conjoint. Certain cardinal facts are so impressed upon our minds when students that it is a long time before we realise that they are not absolute truths. Thus the fact of albuminuria is so interwoven in our minds with the idea of Bright's disease that it is some time before we fully realise that it frequently occurs in other conditions, and that its absence does not necessarily negative the presence of Bright's disease. Again, a cardiac murmur means to us heart disease, and, as before, it is some time before we realise that some of the gravest forms of heart disease are unaccompanied by any murmur, and, on the other

hand, that many patients who have a loud murmur —and that a cardiac one, too—are not suffering from any real heart disease. And so it is with these abdominal cases; the mention of an abdominal catastrophe conjures up in our minds the picture of a patient lying in bed on his back, with his knees drawn up, with sunken eyes, his face pinched and anxious, a livid yet pallid complexion, his abdomen rigid and distended nearly to bursting, and suffering from the most distressing and constant vomiting—a picture which Dr. J. Mackenzie would probably refer to as "the lurid description of an impressionist artist."* But if we wait for this condition of affairs to develop it generally means that all chance of saving that patient's life is lost. We must certainly learn to diagnose the case much earlier than this, and as showing the paucity of signs and symptoms that may be present, I do not think I can do better than refer to a case of Richter's hernia, which, unfortunately for the patient, came under my care some years ago. I am sorry to say I have not kept any notes of the case, but the chief points are as follows:

I was summoned early one morning and found the patient, a woman, in bed, complaining of severe colicky pains, which she referred to a region midway between the ensiform cartilage and the umbilicus, rather to the right side. She was also vomiting. At first I thought the symptoms were suggestive of gall-stone colic. However, as the vomiting continued and the bowels did not act, the question of acute intestinal obstruction arose. On examining for hernia I found a small swelling about the size of an almond in the right groin, which the woman said she had had for a long time, and which felt to me, and to a brother practitioner whom I called in, like an enlarged gland. Nothing abnormal was felt *per rectum*. Enemata frequently administered brought away fæcal matter on every occasion, and I therefore thought that the case was one of fæcal obstruction. There was little or no abdominal tenderness, rigidity, or distension, the pulse and temperature were normal, and the general condition of the patient such as not to give rise to any alarm. The vomiting, however, continued and ultimately became stercoraceous, and at last I realised that I was dealing with a case of Richter's hernia. At the operation the small portion of the bowel nipped in the femoral ring was found to have sloughed, and I am grieved to say that the patient died. It is difficult to avoid the conclusion that had I recognised the true condition of affairs earlier the result would have been very different. At that time I did not clearly appreciate the difference between acute intestinal obstruction and acute intestinal strangulation, or realise that strangulation may be present without there being complete intestinal obstruction. Even in the absence of that swelling in the groin, the persistent and uncontrollable vomiting, ultimately becoming fæcal, should have warned me as to the gravity of the case.

As bearing on this, Mr. Lockwood, in a lecture on "Femoral Hernia,"* refers to a case in which he mistook Cloquet's gland, which lies in the femoral canal, for a femoral rupture. He says: "The patient had been constipated, but not completely, and had vomited; she also had a tender swelling in the femoral canal. Clearly the swelling ought to have been explored." He goes on to say: "It is necessary to remember that sometimes in femoral ruptures, owing to the smallness of the femoral ring, only part of the wall of the intestine is strangulated. The symptoms are acute. The patient vomits, but has only partial obstruction. The bowels may act a little and flatus passes on. There is a small, tender swelling in the femoral canal." He adds: "I had once before seen the small tender swelling of a partial enterocele mistaken for an inflamed gland. I was therefore afraid lest I, too, might fall into error." In conclusion he says: "It is good to confess mistakes: those who have made many mistakes are more charitable in their judgment than those who think they are always right."

The case I have narrated also illustrates how important it is to regard the clinical picture as a whole, for we shall find no two exactly alike, the lights and shades being very differently distributed in different pictures, and also how unwise it is to place too much reliance on the absence of signs and symptoms which we think ought to be present, for one positive symptom is worth any number of negative symptoms.

Now supposing we have gone very carefully into our case, and yet are unable to determine whether

* 'Symptoms and their Interpretation,' p. 225.

* Clinical Journal, July 13th, 1910.

it is one for the physician or the surgeon—what next? Well, we must wait, but not too long, and, above all things, watch. Symptoms pointing one way or the other will, in the majority of cases, develop in a few hours. None of us would think of leaving a woman who was in labour for twelve or twenty-four hours without seeing her, and yet this is a physiological process which, in the majority of cases, can be brought to a successful issue by the unaided efforts of Nature. How much more, then, when the patient is the possible subject of a perforation or of acute intestinal obstruction, is it necessary to keep a constant watch on the case; and in the acute abdominal case more than in any other, I think it is advisable to make a note at each visit as to the exact condition of the patient. This is of especial value should it become necessary to call in a consultant, for we can then present a definite and accurate report rather than a general and, perhaps, somewhat hazy opinion as to the progress of the case. For it may be that the consultant sees the patient during what has been called the "period of repose," when his general condition has greatly improved, and when it is only the persistence and aggravation of the local symptoms which warn us that we are not dealing merely with one of the medical colics.

As I have said before, it would be foreign to the purpose of this paper if I were to enter into the further differential diagnosis of these cases. It is, of course, very satisfactory for the surgeon to have a definitive objective, but as far as the general practitioner is concerned, and for the matter of that, the patient, it is a question of secondary importance whether the case is one of perforated gastric or duodenal ulcer, or whether an acute intestinal obstruction is the result of a band or an intussusception, or whether the onset of acute pain in the right iliac region of a woman is due to a perforated appendix or a ruptured tubal pregnancy. No surgeon nowadays would hesitate to open the abdomen in a case of general suppurative peritonitis or of acute intestinal obstruction merely because he was unable to fix on the ultimate cause of the mischief. And this reminds me of some remarks which Mr. Moynihan is reported to have made during a discussion on duodenal ulcer,* namely, that "some surgeons seemed to think that it was a humiliation for them to open an abdomen after making a diagnosis on reasonable grounds and not to discover the particular condition which they had supposed to be present. He had himself no compunction in throwing his diagnosis aside, as soon as it was found to be wrong, and going in search of something else. And if any precedent were required it could be found in the example of Saul, who went in search of his father's asses and found a kingdom."

And now a word or two as to treatment, *i. e.* treatment before you have arrived at a definite diagnosis, or having formed the opinion that an operation is necessary while arrangements are being made for its performance. And here, it seems to me, that sins of commission are of infinitely greater importance than those of omission. It is not so much what to do as what not to do that is of moment. And first and foremost comes the question of aperients and opium. Your patient is, we will say, suffering from obstinate constipation or is in severe pain. What more natural, what more reasonable than to give aperients for the former and opium or one of its derivatives for the latter? But, roughly speaking, I think I may say—again, not without apologies—that, if it be true, as we are told, that the road to hell is paved with good intentions, it is no exaggeration to say that the road to disaster, in a large number of these abdominal catastrophes, is paved with aperients and opium *recklessly administered.*

And first, as regards aperients. Mr. Anthony Bowlby, in a lecture on "Obstruction of the Large Intestine,"* instancing the case of a patient who sends for you for the first time, complaining that for three or four days he has had trouble in getting his bowels to act, in spite of having taken castor-oil and other aperients in increasing quantities, goes on to say, "You must certainly avoid giving any more purgatives, and avoid them all the more if the patient cannot pass wind. There is no question about this; mere difficulty in getting the bowels to be satisfactorily emptied is a matter of totally different significance from being unable to pass flatus, and if your patient is unable to pass any flatus you must not give any kind of aperient whatever.

We read that "the wind bloweth where it listeth," but there are exceptions to every rule,

* Clinical Journal, March 30th, 1910.

* Clinical Journal, July 21st, 1909.

gentlemen, and unfortunately, in cases of complete intestinal obstruction, there is one direction in which the wind cannot blow, however much it listeth.

I am aware that in cases of acute peritonitis that have been operated upon, purgatives are often administered. Thus the late Mr. Harold Barnard told us, in a lecture delivered in March, 1903,* that in cases of general diffuse peritonitis due to appendicitis, after the case had been operated upon, it was the then practice at the London Hospital to administer grain doses of calomel every hour until the bowels had been opened, and then to maintain a moderate amount of diarrhœa. He mentions one case in which as much as 72 gr. were administered. He also refers to the fact that years ago Mr. Lawson Tait advocated the use of saline purges in similar cases, giving the preference to sulphate of magnesium. Seeing that not so long ago the orthodox treatment of these cases consisted in the administration of massive doses of opium, with the exception of venesection, it is hardly possible to adduce a better example of the tendency for the therapeutic pendulum to swing from one extreme to the other.

In the earlier stages of these acute abdominal cases, however, with which at present I am principally concerned, it seems to me that if in doubt in any particular case as to whether or not we ought to give an aperient, the wisest plan is to follow Punch's advice to young men about to marry—"Don't."

And before leaving the subject of aperients I cannot resist the temptation of quoting a passage which occurs at the end of a lecture by Mr. Moynihan on "The Treatment of Acute Appendicitis."† Discussing the question whether the increased frequency of appendicitis in recent years is real or only apparent, he says: "If there *be* an increase, may we not find its explanation in the greater attachment of the present generation to aperients? The columns of the daily papers, the hoardings by the side of the road or the railway are insistent in their persuasion to us to take a pill, a potion, a syrup, or a bean. So a stern critic of our times, guided by these things in his judgment, might say of us 'that we were of a generation that seeks, not for light and leading, but for laxatives.'"

Personally, I should think that the constipation for which the aperients are taken, by interfering with the due emptying and ventilation of the appendix, would be more likely to act as a predisposing cause.

As regards enemata, these, if carefully administered, can do no harm, and are frequently of great assistance in helping us to arrive at a correct diagnosis; but if after the administration of two or three strong brine or turpentine enemata, the first, maybe, having brought away some fæcal matter, there is no further result, not even the passing of wind, it seems to me only a waste of time to continue with this form of treatment. In my opinion you might as well try to wash away the Alps by playing upon them with a male glass urethral syringe.

And now as regards the administration of opium. This is a question which especially concerns the general practitioner. He knows that he has it in his power to give his patient speedy relief, temporary though it be. The pain will vanish, the vomiting be relieved, the general condition greatly improved, and at his next visit the patient will often express his gratitude by saying that he feels as though he were in Heaven—a place, by-the-bye, gentlemen, from which he seems anxious that we should keep him as long as possible.

Till recently, as the result of what I had read and heard, there had been gradually growing up in my mind the impression that opium, above all things, must be avoided in these cases. It seemed to me that some men, instead of regarding it as the gift of God, as it is called by some nations, looked upon it rather as a wile of the Devil. I will not inflict upon you such a platitude as "Abuse of a thing is no argument against its legitimate use," but it certainly seemed to me that as regards opium there was a fear of our forgetting this truism. I now know that I was mistaken in my impression, but the matter is one of such great importance that I trust you will bear with me while I attempt to place it on a rational basis.

Taking the medical colics, such as renal and gall-stone colic, first, no one, I suppose, would deny that opium or morphia, especially if combined with belladonna or atropine, is our sheet anchor in the treatment of these conditions. As regards acute intestinal obstruction, when we picture to ourselves the gut making frantic but ineffectual

* Clinical Journal, September 30th, 1903.

† Clinical Journal, April 10th, 1907.

efforts to overcome an insuperable obstruction, and thereby not only exhausting itself, but, by the agonising pain produced, drawing largely on the nervous energy of the patient, or in cases of intussusception, increasing with every contraction the extent of the mischief, surely we must feel that any means which will lessen this struggle must be for the benefit of the patient. Take, again, the case of a peritonitis—at first localised. When we consider how Nature attempts to limit this, not only by the formation of adhesions, but also by the protecting hand of what has well been called the abdominal policeman—the omentum—surely it is our duty to keep the parts as quiet as possible.

The chief grounds for objection to its use seem to be that the general condition of the patient is so much improved that the symptoms are masked, and the consulting surgeon whom we call in is apt to be misled as to the gravity of the case. But if we keep exact notes of the case, and instead of administering our opium haphazard, merely for the relief of pain, *wait until we have made a definite diagnosis*—and this is the point I missed—and then give it only in the smallest doses possible to effect our object, I maintain, subject to correction, that we not only do no harm, but, by preventing an increase of local mischief, and conserving the energy of the patient, we put him in the best possible condition for withstanding the shock of an operation ; surely we are simply bringing about artificially that period of repose which surgeons tell us is the ideal time for operating.

I am aware that statistics have been published contrasting the results of operations performed on cases in which no opium has been given, with those performed on cases in which this drug has been administered, and that the results are greatly in favour of the former; but, in the absence of any exact information as to the length of time elapsing between the onset of symptoms and the performance of the operation, and also as to the *amount* of opium given—and this, after all, is a crucial point—I think one is justified in raising the question whether the unfortunate results in the latter class are not due rather to the delay in the performance of the operation, owing to masking of symptoms, or to the large doses of opium given, than to the mere administration of the narcotic.

As regards the *feeding* of the patient, until you are certain as to your diagnosis it seems the wisest plan not to give anything by the mouth, but relieve thirst by rectal injections of normal saline. Dr. Herringham, in a lecture on gastric ulcer, not perforated,* refers to a case in which five pints of saline injection were given every day for five days without loss of weight. He also mentions other cases, in one of which, under similar treatment, the patient actually gained in weight.

In doubtful cases the best *position* for the patient seems to be with the head of the bed raised, so that any effused material may drain into Douglas's pouch, the so-called "sink of the abdomen," rather than gravitate towards the upper part of the peritoneal cavity, where absorption is so active.

The preparation of the patient for operation and the post-operative treatment—matters of such vital importance to both patient and doctor—are, of course, best treated of by the abdominal surgeon.

Before closing my paper, however, if I have not already trespassed too long on your patience and forbearance, I should like to draw attention to another point which these cases, more than any other, cannot fail to bring forcibly home to our attention, and that is the lack of any real and widespread organisation in the medical profession, so far as the fighting of disease and death is concerned; and I need hardly say that I approach this subject with the utmost diffidence.

We will suppose that you are unable to make up your mind in a particular case as to whether or not an operation is necessary, and would much like to have the opinion of a man of more knowledge and greater experience than yourself. For those of you who live in or near a big town this is, of course, a simple matter, but when you live fifty miles or more from a skilled abdominal surgeon the question assumes a very different aspect. If you send for him and it turns out that no operation is necessary, the patient and his family do not bless you for running them into what they consider an unnecessary expense of eighteen or twenty guineas. If, on the other hand, you wait until there is no doubt about the matter, by the time the surgeon arrives it is often too late to give the operation a fair chance of success, and although he says little, like the parrot, he probably thinks a good deal. In fact, gentlemen, you are between the devil and the deep sea ; which the devil and which the deep sea I will leave you to determine.

* Clinical Journal, September 16th, 1908.

In the case of poorer patients you must, of course, do the best you can, but it seems to me incredible that, at the beginning of the twentieth century, with all our philanthropic institutions, with our societies for the prevention of cruelty to everything that is in the earth beneath, or in the water under the earth, with our huge armaments for protection against foreign invasion, that the fighting of disease and death—the one common enemy—irrespective of age or sex, riches or poverty, religious or political opinions, is left to the enterprise of private individuals. I grant you that our sanitary laws, our inspection of school children, our regulation of dangerous occupations, our vaccination laws—which in my opinion are not half stringent enough—and other similar enactments have done much to reduce the amount of disease and death; but, when the enemy is once upon us, unless it be in the form of an epidemic which threatens the community at large, the fighting of it, except in our big hospitals, is left in the hands of more or less isolated individuals.

Mr. Herbert Spencer, in an essay on "Progress: Its Law and Cause," maintains that progress consists essentially of the transformation of the homogeneous into the heterogeneous; in other words, differentiation, whether it be in the development of a solar system from a nebula, of a mammal from an amœba, or of our present complicated system of government, society, science, and arts, from the primitive manners and rudimentary acquirements of a cave-dweller. If that be true, gentlemen, the medical profession has certainly made considerable progress during the last fifty years. The country general practitioner, however, is still in what might be called the amœboid stage. He has to perform the duties of a physician, a surgeon, an obstetrician, a gynæcologist, and so on and so on; he has to face and fight single-handed medical and surgical emergencies which would call into action all the special departments of a well-equipped London Hospital; and I think, on the whole, he discharges his duties remarkably well. I am a country general practitioner myself, gentlemen, and so am in a position to speak as one who knows. But when it comes to difficult and obscure cases, which require for their diagnosis and treatment special knowledge and training, it is hardly to be expected that he can bring to them the same amount of knowledge and experience as one who has devoted his whole attention to the particular class of case in question. And here it is that the use of the specialist comes in. He is the natural outcome of the progress which is taking place all around us; and just as we do not expect a liver-cell to perform the specialised functions of a brain-cell, nor, on the other hand, a brain-cell to discharge the duties of a liver-cell, so we have nothing but admiration for the intimate knowledge which the specialist possesses of that particular region of the body to which he has devoted his time and attention. But, in my opinion, what is wanting is a nervous system to connect and co-ordinate the various branches of the profession.

We have heard and read a great deal lately in the medical journals about hospital abuse and sickness and invalidity assurance, and it seems to me that the key-note struck has been how these do and will affect the medical profession financially; and that is well, for medical men have too long been the prey of the vicarious philanthropist. But the point on which I am trying to insist is, not so much how to procure that abundant supply of oil which is so essential for the satisfactory working of the machine-medical, but whether that machine is at present so constructed as to produce the largest and best possible amount of work, and I fear one must answer that question in the negative.

You say, "What, then, do you propose?" Well, of course, I am bringing this subject forward merely in what might be called the resolution stage. The complete working out of such a scheme would naturally take a great deal of time and would require the most careful consideration; but, as a first step, I would suggest an increase in the number of our cottage hospitals, that each should be provided with private wards, in which any medical man in the district should have the right to attend his own patients; that genuine, *paid*, consulting physicians and surgeons should be attached to these hospitals, so that, in case of necessity, the country general practitioner could, for a reasonable fee, obtain a special opinion in any case, the patient contributing according to his means.

With regard to the financial aspect, as far as the *so-called* working classes are concerned, this is already being freely discussed, but as to the lower middle classes, who, after all, are those who find the struggle for existence the keenest, although, in the majority of cases, they are quite unable to pay a fee of twenty or thirty guineas, with perhaps the additional expense of two trained nurses, they could often manage to squeeze out a fee of £5 or £10, and so help towards the payment of the surgeon's fees and the other expenses incurred.

I am aware, gentlemen, that I have stated my case in a very bald and incomplete manner, but, in my opinion, the question I have raised is one which must, sooner or later, come to the front, and I think it is far better that the agitation should arise, in the first instance, within the profession than be launched upon it from without.

And in bringing my paper to a close I do not think I can do better than re-echo that fine thought of Pasteur's, which I saw quoted the other day: "That where good is to be done our duty only ceases when we no longer have power to achieve more or to do better."*

* Clinical Journal, June 29th, 1910.

January 9th, 1911.

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* *Specially reported for the Clinical Journal. Revised by the Author.*

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A LECTURE

ON

GAIT AS AN AID TO DIAGNOSIS.

Delivered at the National Hospital for the Paralysed and Epileptic, Queen Square.

By H. H. TOOTH, C.M.G., M.D., F.R.C.P.,
Physician to the Hospital and to St. Bartholomew's Hospital.

GENTLEMEN,—To-day it is my intention to take as my subject a clinical demonstration of cases with peculiarities of gait, a study of which may serve as an aid to diagnosis in nervous disease. Alterations of gait are so common that you may be surprised to find how wide a range of diseases exhibit this as a symptom. In illustration, by the kindness of my colleagues, I have collected a number of patients from the service of the hospital.

Ataxia.

The first abnormality of gait to claim our attention is ataxy. It appears in a number of diseases as a symptom, but pre-eminently in tabes dorsalis, of which the patient before you now is an ordinary example. He presents a good instance of what is generally the very earliest evidence of unsteadiness of gait, namely, an involuntary widening of his base by walking with the feet apart. The loss of joint sense, and therefore of the degree of excursion made by the limb, which is included in the term "sense of position," prevents the patient from measuring accurately the length of each stride, and the result is wide variations in step unless his eyes are fixed upon his feet. This may on the one hand give rise to tremulousness and disorderly movements endangering his balance altogether, or on the other, to short tentative movements of progression which may be styled "toddling," or of the "mechanical doll" type. This patient is also an example of this sort of gait. The "toddling" gait may be seen in very old people, and in paralysis

agitans, and, as in tabes, it arises from a want of confidence in the accuracy of movement in placing of the feet in walking.

Disseminate Sclerosis.

Ataxia sometimes occurs in the course of disseminate sclerosis, of which this patient is an example.

In September, 1906, she developed a squint, and had diplopia, which lasted three weeks. In October she went to Blackpool and during that time became amblyopic. At the same time she had some nystagmus and weakness of the legs, which became worse. In 1907, after various treatments, she was able to walk with the aid of a stick. She was fairly well until the winter of 1909. Since then there has been weakness of the legs and dragging of the feet, so that she can only walk with assistance. For some months she has had weakness and characteristic tremor of the right hand. She is of an extremely cheerful disposition. The tongue is protruded slightly to the right side, and is wrinkled on that side, and tremulous, suggesting that there has been a distinct atrophy of the muscles of the right half. The discs are pale, and may be slightly atrophic. She has slight intention-tremor in the hand. She walks with a wide base, and she is distinctly ataxic. You will notice also a peculiar tremulousness of the legs in walking—a feature quite apart from ataxia. It suggests a clonus, which is probably what it is, and it is rather odd that one does not see it more often in such cases. Though she is so unsteady in her gait she can stand very well with her eyes shut.

Ataxia in Cerebellar Disease.

The little boy now before you has probably a tumour compressing the right lobe of the cerebellum. He has improved considerably after a decompression operation, and therefore the symptoms are now slight. But you will notice how carefully he walks lest he should fall. With the eyes shut he tends to sway to the right, the side of the lesion. It is rather difficult to convey in words the difference between the gait of tabes and that of cerebellar lesion, and, in fact, it might be possible to produce cases presenting almost exactly the same characteristics. But generally speaking, the gait of cerebellar disease is more of the reeling or drunken type than that of tabes, as then it is so frequently one-sided, producing the tendency to reel or lurch to that side—a condition rarely seen in tabes.

Ataxy in Friedreich's Disease.

My next case is one of Friedreich's disease. She is a girl, æt. 21 years, under the care of Dr. Ormerod. There is nothing apparently in the family history, though the condition often runs in families. At fourteen or fifteen years of age she complained of feeling tired and weak. She came as an out-patient and was treated for anæmia, so at that time there was nothing else suspected. At the age of fifteen her mother noticed that her back was "growing out." Scoliosis and distortion of the back frequently precedes the ataxy, and in fact ataxy may be an unimportant feature of the case all through. Her knee-jerks are absent, as is generally the case. In the larger number of cases of this disease there is found post-mortem lesion of the posterior columns. But the almost invariable scoliosis is very difficult to explain, so also another very common deformity, namely, a slowly developing high arch to the foot, producing a marked pes cavus. With this appears also an over-extension of the great toe, amounting in some cases to a subluxation of the joint. Still more curious is the frequent, if not invariable, appearance of the extensor response, or Babinski phenomenon. This would suggest that the lateral columns are nearly always affected in a minor degree.

Combined Postero-lateral Sclerosis.

I now show you a case of postero-lateral sclerosis, or ataxic paraplegia, a very common clinical picture. Time will not allow me to go into details, but the case will serve to illustrate the peculiarities of the gait. Here we may assume a combined lesion of two columns of the cord, each of which would, if affected, singly present opposite symptoms. Simple lateral sclerosis gives rise to rigidity and increased reflexes—a stiff man. Simple posterior sclerosis produces the picture of a limp man, flaccidity, absence of muscular tone, absence of reflexes, and ataxia. Combine the two and we find the lateral column symptoms dominate the symptom-complex, but the ataxia remains. So that the patient drags his feet owing to stiffness of muscles, presents exaggerated knee-jerks, ankle clonus, and extensor response, but in addition is unsteady in

his gait, and unable to stand with his eyes shut—symptoms which are absent in simple lateral sclerosis.

Spastic Gait; Spastic Paraplegia.

This patient twenty months ago had what appears to have been a left hemiplegia, from which he fairly recovered. Three months after he became stiff and weak in the right leg, and since then he has presented the picture of spastic paraplegia. Possibly he may have had some vascular lesion of the pyramidal tracts in the medulla or above it, with secondary descending degeneration of the crossed pyramidal tracts of the lateral columns of the cord. However that may be, he presents now a fairly typical instance of lateral sclerosis. The reflexes are increased, there is ankle-clonus, and the plantar responses are both extensor. The gait is characteristic: his legs are so stiff that he has to shuffle along, scraping the toes along the ground, so that the boots become rapidly worn out at the toes. You will notice that there is no sign of unsteadiness and he can stand with his eyes shut, walk as well in the dark as in the daylight. Such patients will shuffle about in this manner for years, their condition remaining stationary. But in some cases the adductor muscles become rigid, and the legs then tend to cross. This is a most crippling condition, and if well marked renders unaided progression impossible. One will find that two alternative methods of progression may be adopted by a person with rigid legs, either a direct forward drag of the feet as in this case, or a circumducting movement of the feet, a method chosen by some hemiplegics. The choice of these methods may be determined by the amount of adductor spasm, which would effectually prevent any circumduction even if present in the slightest degree.

These types of gait may appear in the course of a case of disseminate sclerosis.

The rigidity may be much diminished and the gait proportionately improved by massage, and especially by assiduous passive movement to the limbs. The rigidity is greatly relaxed by hot baths, which may very fitly precede the passive exercises. The patient should be encouraged to use the limbs as much as possible; the rigidity tends to increase with rest in bed, and fibrosis of the muscles may develop, adding greatly to the intractability of the case.

Peculiar Gait in Some Cases of Frontal Tumour.

The patient I now show you exhibits a symptom which I have seen in undoubted cases of frontal tumour, though I do not feel sure of the diagnosis in this particular case. He is a carman, and in February, 1909, he was struck on the vertex. He went on with his work until May, when he saw a doctor for a thrombosed vein in his leg. From that time he has had progressive weakness of both legs, with a staggering gait but no rigidity, and he has never done any work since. The weakness has progressed, so that he has not walked for a month. One month ago his weakness became suddenly more pronounced, and at the same time there has been some slight speech defect. All the time he has had occasional pains in the lower lumbar region. There is no girdle sensation and no numbness, but there are occasional sharp pains from the knees downwards, and there is some slight incontinence of urine, and of defæcation. The tongue is not tremulous, nor are the lips. The pupils were at one time unequal, the left larger than the right when he came in, but active to light. A point of great importance is that there is no alteration of the disc. In the upper extremity there is no marked tremor, his grasps are feeble, not much difference between the two. The abdominal reflex is present on the left side, absent on the right. The spine is natural. If he is sitting in a chair and I take hold of both his hands I am conscious that he is pulling against me as if to save himself from falling backward, and he walks in a toddling fashion, not with marked ataxy, but with short steps, and the feet rather wide apart. He can stand by taking hold of something, but there is a tendency to fall backwards if unsupported; this was very marked on admission. This tendency to fall backwards will remind us of the similar symptom in tumours of the cerebellum. In fact the similarity has led to operation on the cerebellum to my knowledge once or twice, in one of which cases frontal tumour was found post mortem.

I have had under observation three cases of undoubted frontal tumours,* in which the patient showed the peculiar symptom which I will now describe more fully.

As the patient sits propped up in bed there is

* One of these is reported in the 'St. Bartholomew's Hospital Reports,' 1908.

nothing noticeable, but take away the support, and though he may make a great effort to remain sitting, and may do so for a short time, he sooner or later falls backwards helplessly. If guided a little to one side he will fall to that side, and in either case he will make no effort to recover his position. This is still more marked if he is made to sit on the side of the bed with his legs hanging down. At first he may do so well, then during conversation he will gradually and slowly tilt backwards, and finally fall back with his legs in the air. This may be delayed by making the patient hold his knees with his hands, and bend his body forward, but *he has to be shown how to do it* and his attention must be kept on this point, and even then in a short time he begins to lag backwards. If one holds his hands one is soon conscious that he is pulling on them, relying entirely on them for support. If now we make the patient stand, we find that he has lost all idea of the proper position in which to place his body. He has to be dragged up by the two hands and may even require further support behind. When urged he will make short steps forward, pulling backwards hard the whole time. If the support is removed he falls without the slightest attempt at recovery. It is this complete loss of all knowledge of the attitude even, necessary for the erect posture, let alone for progression, that marks this class of case. I have never seen a cerebellar case that really resembles it, and I am inclined to think it is proper to frontal tumours, and that it is essentially a loss of memory of muscular efforts normally automatically used for the erect posture and progression, comparable to, and probably of the nature of apraxia of the upper limbs, without, be it understood, any real paralysis whatever.

A similar condition may be seen in some cases of severe, purely functional, paraplegias, so much so that the patient may have to be actually taught to walk again.

Paralysis Agitans.

The peculiarities of attitude and gait in this disease are no doubt well known to all. You may also know that these features are the characteristic ones of the disease and may exist without any of the tremor. The patient is attending under Dr. Gordon Holmes in the out-patient room. He presents a very characteristic picture, with the attitude slightly leaning forward. He presents the tremulous cigarette-rolling movements of the hands. There is a want of expression in the face. Notice his attitude and gait, namely, the tendency to festination, or the tendency to go on when started, without being able to stop, most noticeable on suddenly rising from the chair. There is never any ataxy or irregular movement in walking. Perhaps the most common gait symptom is retropulsion, or the involuntary tendency to hurry the step when started walking backwards. This, if not checked, may end in a fall backwards, but it generally is indicated only by an inability to stop exactly on the word of command. My own experience of a very large number of cases of all degrees of severity is that gait symptoms in paralysis agitans are relatively not so common as the book description would lead one to suppose. Occasionally one sees a case in which this festination is manifest on side-stepping, and as such may be called latero-pulsion.

Retropulsion when well marked is a most unpleasant symptom. I remember an old man who used to come to the out-patient room here who showed it in a marked degree, and it had been on many occasions a source of great peril to him. For instance, if after crossing the road and in the act of stepping on to the curb he, forgetting the consequences, made a sudden step backwards to avoid collision with another person, he would be started in a backward direction involuntarily and quite regardless of the traffic. He said that this had actually happened on several occasions, to his extreme danger. Happily such cases are not common. In the later stages of paralysis agitans the gait becomes almost always of the feeble "toddling" senile type, possessing no features different from those of extremely feeble old age.

The Myopathic Attitude and Gait.

The great group of the myopathies, to which belong pseudo-hypertrophic muscular paralysis, the juvenile or the scapulo-humeral form, and the infantile or the facio-scapulo-humeral form, present common and characteristic features as regards attitude and gait.

Of these forms I show you two examples. The first is a boy who has had a difficulty in walking as long as he can remember. He shows some fairly marked pseudo-hypertrophy of the calf muscles, and also of the triceps.

The other is a man who presents no pseudo-hypertrophy of any muscle, but very marked atrophy of the shoulder-girdle muscles. The pectorals are practically absent; the sterno-mastoids and scaleni are reduced to mere threads. There is marked winging of the scapulæ, owing to wasting of the lower fibres of the trapezius. There is also weakness of the anterior tibial group.

Both these cases exhibit the peculiarities of gait of the myopathies, the boy most characteristically.

These are attitudes of lordosis: arching forward of the lower spine, with sometimes a compensatory backward curve of the upper, leading to a protruding of the abdomen, and a waddling gait which, once seen, can never be forgotten. If there is much weakness of the anterior tibial group there will be a high-stepping gait, which very much increases the waddle. The patient is unable to get up from the ground without helping himself in the slighter cases by putting his hands on his knees—"climbing up legs," as the saying is. These characteristics are so constant and peculiar to the group that they constitute a very early and delicate sign of the disease.

The High-Stepping Gait.

Any condition which leads to dropping of the foot will cause the patient to raise the foot in walking in order to clear the ground. Not only this, but he will have to jerk the leg forward in order to throw the loosely dangling foot clear, and will then bring it down with a slap in a quite characteristic fashion.

Here is a case of the peroneal form of progressive muscular atrophy. There is a long history, and there are a number of these cases in the family. Notice the way in which he raises the leg and brings forward the foot with a slap on the ground. I ask you to notice the weakness and thinning of the calf muscles, the anterior tibial group and the upper thigh muscles. The knee-jerk in these cases is generally absent. Sooner or later—generally sooner rather than later—there appears a certain amount of thinning and weakness of the small hand-muscles, particularly the first dorsal interosseous. Ultimately the atrophic process spreads to involve all the muscles, but the progress is extremely chronic. I show you also a little girl with high-stepping action and foot-slapping of one leg only, due to infantile spinal paralysis, which has left her with paralysis and atrophy of the anterior tibial group only.

I may say here that this peculiar gait is not necessarily confined to organic disease. I have seen several well-marked instances of it in functional or hysterical patients, who have ultimately quite recovered.

Peculiarities of Gait in Functional or Hysterical Cases.

I suppose one may say that there are no irregularities of progression that cannot be imitated or reproduced in functional cases.

The most common affection of gait in such cases is feebleness without any other distinctive features. Such a patient walks with slow, deliberate, short steps; there may be no dragging of the feet; he appears to be afraid to walk. It is difficult to picture in words, and yet it is really characteristic.

I show you a healthy-looking little boy, æt. 12 years, intelligent, bright, and in Standard V at school. For no apparent reason he suddenly lost power in both legs. This has very much improved, and although he could not stand even when he came in he can now walk slowly. No amount of urging or threatening will induce him to walk faster.

The woman who now comes in presents the same features. She has been for a period of years apparently quite paraplegic, and is now gradually being taught and encouraged to walk again. Patients of this sort, having become convinced that they are paralysed, frequently completely, accept the position, and gravitate to the infirmaries as apparently hopeless paraplegics, and yet on examination no evidence of organic disease can be found. After a time it would appear that all knowledge of the muscular actions for the maintaining even of the erect posture seem to be lost, and the patient has to be actually taught how to stand, then how to walk. In some cases a cure is impossible owing to a hopelessly apathetic mental attitude. The patient does not seem to wish to get out of the invalid state, but appears to be satisfied with an almost vegetable existence. As years go on actual fibrotic changes may take place in the muscles, and a true organic condition may be established.

My next patient may illustrate the effect of suggestion upon attitude and gait. This man is a

shipwright and has done no work for eighteen months. He walks with his body bent grotesquely over to one side. He complains of some pain in his hip and shoulder, but Dr. Taylor can find no evidence of any organic lesion, neither is the pain sufficient to account for his extraordinary attitude. It is thought possible that an attack of lumbago may have, in a susceptible subject, suggested to him the curious position he has assumed. He has improved very much since admission, and will probably ultimately get quite well.

As our knowledge of these so-called functional cases increases one becomes more and more wary in making that diagnosis. Because at one time there is no sign of organic disease it does not follow that evidence of such may not appear subsequently, even after a period of apparently perfect health. This is almost the rule in disseminate sclerosis. "Nervous breakdown," functional paralyses of every sort may occur from which recovery takes place, and these may be repeated two or three times over a period of years, and yet eventually organic signs may make their appearance and the diagnosis of a definite lesion of brain or spinal cord becomes certain.

My last case may ultimately turn out to be of this nature.

This man was in hospital under me in 1908. Five years before his legs became weak, he had formication in them with pains behind the knees, and soon after he had a sudden exacerbation in which he was quite paralysed for a time. After careful examination in hospital we came to the opinion that the case was one of functional paresis of the legs, an opinion which has been confirmed quite lately in the out-patient room by Dr. Gordon Holmes, who can even at this time, seven years after the onset, find no evidence of organic disease. His knee-jerks are exaggerated and the right, perhaps, is in excess of the left, but that is scarcely enough to found a serious opinion upon. Yet as you see he is in a miserable condition, and quite unfit to earn his living. He crawls about, dragging his legs, which are obviously paretic, requiring two sticks to help him. There is such an amount of ataxia as to suggest tabes dorsalis, until we come to examine him. We call this a "functional" case, but is not this an expression of ignorance?

In the future may we not look for the discovery of signs comparable in delicacy with that which will always be associated with the name of Babinski?

January 16th, 1911.

THREE LECTURES

ON

CANCER OF THE RECTUM.

Delivered at the Polyclinic.

By FRED. C. WALLIS, M.B., B.C., F.R.C.S.,
Surgeon to Charing Cross Hospital.

Lecture II.

Gentlemen,—In the last lecture there were two points which I forgot to mention with regard to inoperable cases of carcinoma of the rectum, namely, the points which have been often dealt with—constipation and diarrhœa, and then constipation alternating with diarrhœa. The difficulty of getting the bowels to act was very well illustrated by a case which I saw this morning: a man had well-marked annular carcinoma, almost in the recto-sigmoidal junction, and he complained of considerable difficulty in getting his bowels to act for some months, and sometimes when he had that desire it was accompanied with a marked condition of colic. The growth in this man was an annular one, and was about three inches long. There was no marked stricture of the bowel, and there was plenty of space for the ordinary intestinal contents to pass. In these cases the malignant growth involves the *muscular* coat of the bowel. This acts as a check to peristalsis, which goes on until it gets to this point, and then, the infiltrated muscular tissue being no longer contractile, there is no continuation of the peristalsis, and the intestinal contents are not passed on. This is the reason of early constipation, and these patients have to take a certain amount of medicine to get rid of the accumulation. The diarrhœa is produced by fermentation, which occurs in the accumulation at the site of the growth.

In connection with malignant growths, and those non-malignant growths which tend to assume a malignant character, there are certain growths which are of quite an innocent type, but which, owing to their situation, have to be regarded as malignant from an operative point of view. I show you on the screen two photographs of such growths which have been operated upon. These are specimens of innocent growths which are so situated as to be difficult to remove, and show such

symptoms that they have to be operated on. The first is a specimen of multiple papillomata of the large bowel. This was removed from a man who showed indications of a malignant growth. At the operation this state of multiple papillomata was found to be present a long way up the colon. The man recovered from the operation, which was done a couple of years ago. He eventually came into the hospital again, for pneumonia, and died. At the post-mortem examination a most rare condition of multiple polypi was seen to extend all up the large and small intestine. The patient was sixty-four years of age when he came under observation. Here is another specimen of innocent growth. It is a large papilloma and was treated as if it were carcinoma. It was excised, and the upper end of the bowel was pulled down and stitched to the anus, with a very good result. It is obvious that any minor operation in dealing with a tumour high up in the bowel of this character is useless. If left alone it tends to become malignant, and there is often considerable hæmorrhage from these tumours. It is another good specimen of an innocent tumour which should be treated as if it were malignant, because of its situation, extent and symptoms.

We now come to the various operations which have been, and are still, in vogue in connection with the removal of these growths. It may perhaps strike you that as these operations are not procedures which you are called upon to do every day I ought not to spend too much time over it. I will not do so, but I am certain that though a man in general practice may not do these operations himself, he will be none the worse for knowing the details of the points in a big operation which he may have to suggest his patient should have done; in fact I think it is essential he should know them. There is first of all the perinæal operation, which was the only one for a long time. Hartman accounts for a large number of cases which he had excised through the perinæum, with good recovery as far as the immediate operative procedure goes. Another operation is the ano-coccygeal operation. Another is the ano-sacral, Kraske's is another, which you know, and then there is the abdomino-perinæal.

The perinæal is a good operation for growths around the anus involving the sphincters, and which may not have been long in existence.

The ano-coccygeal and the ano-sacral operations are much the same, in that the growth is attacked from the back, the incision is made mid-posteriorly, and in one instance the coccyx alone is removed, and in the other the coccyx and a portion of the sacrum, and the peri-rectal tissues are divided and dissected away until the bowel is arrived at. The bowel is gradually separated all round and pulled down until the reflection of the peritoneum is seen. This is opened on both sides, and then the bowel is easily pulled down, unless there is a short meso-rectum. So far, these three operations resemble one another. When Kraske's operation was invented it marked a very definite step forward in rectal surgery, but when the growth was removed the anal portion of the bowel was left, and the bowel above the growth was brought down and stitched as far as possible to the anal end. But a part of the two ends had to be left ununited, and this left a sacral opening, which often resulted in a permanent sacral anus. A sacral anus is far more troublesome than a colostomy can ever be, and causes much distress. Therefore, I made up my mind quite early in my operating days that it was useless to keep the anal part of the bowel. It can easily be dissected off the sphincters, and as easily the upper end of the bowel can be pulled down the extra two inches to the anus. By this means there is a *continuity* of the large bowel to the anus. A drain is placed in the sacral wound, which gradually heals up, and the result is a normal anus. This is the operation which I have always regarded as far the best, and I gave details in a book which I wrote two or three years ago. It must be obvious that the points are in favour of bringing the upper end of the bowel down to the anus instead of leaving a sacral opening.

The most recent development in rectal operative surgery for malignant disease is the abdomino-anal operation. Anyone who is interested in the matter should read Dr. Lusk's articles which appeared in that excellent periodical, the 'Annals of Surgery and Gynæcology,' an American monthly magazine. The operation is one of the largest in surgery, and it requires much trouble and care, and there is a good deal of anxiety attending it. But the results from it are getting better, and will, I hope, ultimately be very good. There has recently been a discussion in the medical papers as to the question of suggesting that these operations should be conducted for a growth, say, two inches from the anus. Various letters were written saying that it was an extraordinary thing to suggest it, and that obviously any surgeon would prefer to operate by the perinæal method or by one of the other methods than to do this big operation. Of course anyone who had read Lusk's paper referred to above, and also Sampson Handley's paper, and seen the

coloured print with it in one of the numbers of the 'British Medical Journal,' could no longer doubt as to the desirability of doing this operation far more frequently than has as yet been the custom, big as it is. I know I am speaking a little ahead of the times, but you will find that this will be the case. The whole reason for suggesting this operation in the case of low-lying carcinoma is that if you follow out the after-history of these cases you will find that the growth frequently returns sooner or later in the upper end of the bowel. That has been my experience in various instances, and here is a case in point, and with that I shall conclude to-day. I operated upon a lady two years ago for a small growth, which really originated in an old-standing fissure. The sphincters were not involved, and I took out three inches of the mucous membrane, which was brought down to the anus. She did very well. I saw her about eight months afterwards, and she had a small nodule of mucous membrane half an inch inside the anus, which of course I had to remove because it was suspicious. But the microscopist said, "No; it is innocent mucous membrane, and there is no growth there. Six months after that she came to me with a small nodule, *not* in the mucous membrane, but three-quarters of an inch behind the posterior anal commissure, underneath the skin. That was easily excised, and proved to be a spheroidal-celled carcinoma. I was very gloomy about this, but could not make up my mind that I ought to urge the big operation, so I thought we should give her any chance which X-ray treatment would give. She had X rays for four weeks, and I saw her after their application and she seemed to be all right. Four weeks after that I saw her again and she complained of some mucus coming away, and then I found a hard cord and a small lump, about two inches up from the anus, but *not* involving the mucous membrane. The end of it was one had to do the abdomino-anal operation, and it would have been probably far better if I had done that the first time I saw her. But one learns by experience, and it takes a great deal of courage and confidence for one to say to a person who has a small malignant growth, "You must have this enormous operation done; it is your only chance." There is much more might be said about this, but I have mentioned it that you may bear it in mind when next the question is discussed in the medical papers. Perhaps we may eventually get from the pathologists some definite statement as to what form of carcinoma *will* localise itself for a considerable time before it disseminates, and what are the characteristics of the other kind of carcinomas which will quickly involve the lymphatics. That is what we have to hope for with regard to the knowledge which we shall gain as to when to do this big operation and when not.

January 16th, 1911.

THE VALUE OF ELECTRICITY IN THE TREATMENT OF SOME ABDOMINAL DISEASES.

By H. MANDERS, F.R.C.S.Eng., M.D.Brux.

In order to use our remedies in the most intelligent manner it is only proper to make ourselves thoroughly acquainted with the anatomy and physiology of the organ affected, first in its normal position and condition, and secondly as altered in function by pathological influences or as displaced.

I think that this is more important in treatment by electricity than in treatment by medicines, for in the latter the action is indirect and not yet fully understood, whereas in the former the application is direct and the *modus operandi* fairly well known.

The stomach is the most variable and most mobile viscus in the human body, except at its cardiac or œsophageal orifice, where it is held in position by the gastro-phrenic ligament. This point is situated deeply behind the seventh left costal cartilage, about an inch or so from its junction with the sternum, and on a level behind with the tenth dorsal vertebra. The pyloric orifice, unlike the œsophageal, is very freely movable, but normally lies in the line of the right border of the sternum, about two and three quarter inches below the sterno-xiphoid articulation, and in a horizontal line drawn from the bony end of the seventh rib to the articulation between the last dorsal and first lumbar vertebræ. In its vertical position it is anterior to that of the œsophageal orifice; in its horizontal position it is on a lower plane. When empty the stomach is deeply placed, and lies somewhat flatly in its bed. When moderately distended, as it usually presents itself to our examination, the lower part rolls up towards the front, forming the greater curvature, the lower border of which generally reaches a horizontal line drawn between the tips of the tenth costal cartilages on either side—that is to say, some centimetres above the umbilicus, from which it is separated by the transverse colon.

The upper border, between the two orifices, forms the lesser curvature. The largest and most capacious portion of the stomach lies to the left of the œsophageal orifice and rises up under the dia-

phragm as high as the sixth chondro-sternal articulation. This portion forms the fundus, from the left extremity of which the somewhat funnel-shaped tube of the stomach more or less gradually narrows until it terminates at the pylorus, though, just before reaching that orifice, a small dilatation is evident, which is known as the antrum pylori.

Hence, it will be seen that the general shape of the stomach much resembles that of a Dutchman's wooden shoe (especially that kind depicted in caricature), of which the tip would represent the pylorus, and a thin ankle inserted into the opening, the œsophagus.

The stomach possesses three muscular coats, the most superficial of which consists of longitudinal fibres, which are continuous with the corresponding fibres of the œsophagus, and merge into the longitudinal fibres of the duodenum at the pyloric end. They spread themselves thinly over the whole surface, but are most pronounced over the two curvatures, especially over the lesser.

The action of these fibres would appear to be to steady the stomach and keep it rigid during the action of the other muscles. The circular fibres, which lie beneath the foregoing, form a fairly even layer over the whole organ, but concentrate at the pylorus, where they appear as a circular ring projecting into its lumen, being covered by a thick fold of mucous membrane. These together form the pyloric valve or sphincter. It is these circular fibres which, acting in succession, cause the peristaltic movements of the organ. The oblique muscular fibres are continuous with the circular fibres of the œsophagus, and are almost entirely distributed over the fundus, covering both its anterior and posterior surfaces, and wrapping themselves around its extremity. These fibres churn up the food in the cardiac segment of the fundus and help to reduce solids to a pasty consistence, after which they contract to pass it onwards to the pyloric portion.

The fundus lies deeply from the surface. It is covered in front by the lower ribs, and in its upper part is overlapped by the most dependent part of the left lung. The pylorus and the lesser curvature are overlapped by the margin of the left lobe of the liver, so we see that only a small portion of the stomach is ever in direct contact with the parietes of the abdomen. This part corresponds to a triangular space in the left half of the epigastrium, the base of which is found by a line drawn from the tip of the tenth left costal cartilage to the tip of the ninth costal cartilage on the right side as far as the margin of the liver; the sides are formed by two lines drawn from the extremity of the eighth costal cartilage on the left side to the two extremities of the base line.

As the result of disease, the stomach may alter in position very considerably, even enormously, but the œsophageal orifice always retains its normal situation. The pylorus, when the stomach is distended, may shift its position to the right as much as three inches, or it may be dislocated downwards, either with other viscera or with the stomach alone. The body of the organ may be distended downwards until the lower margin of the greater curvature is on a level with or below the umbilicus. Again, it may be so displaced that the fundus may be found as low as the left iliac fossa, and the pylorus about the level of the umbilicus.

In atonic conditions of the stomach it is not unusual to distinguish a dilatation of the fundus upwards as high as the fourth rib. I remember one case, many years ago, in which the dilatation reached right up to the lower border of the third rib. It was sent up as an unusual case of cavity of the lung with tympanic percussion note, but the correct diagnosis was given by Sir William Broadbent, who, in after years, drew attention in a monograph to this peculiar phenomenon.

I must now say a few words upon the anatomy of the stomach as revealed by the X rays after a meal containing a quantity of a pure salt of bismuth, and I may here remark that there is no danger, as has been suggested, to the tissues from any secondary rays given off from the metal, because, in order to produce such rays from a metal of so high an atomic weight as that of bismuth, a tube would be required of such excessive hardness that it would be quite useless for our usual purposes.

Mr. W. H. M. Gray has demonstrated the presence of a notch "which is very constant and very easily seen on the greater curvature where this is crossed by the continuation of the more or less vertical line of the lesser curvature of the cardiac portion of the stomach." This notch indicates the position of a central sphincter muscle which divides the cardiac portion of the stomach from the pyloric. It has been named by Gray the "sphincter aditus vestibuli." The cardiac portion

itself is divided into two segments, for "a second notch is usually apparent on the screen on the cardiac greater curvature, about three or four inches distant from the sphincter notch. It is not so well marked, nor so persistent. Occasionally, opposite to it on the lesser curvature, is seen a faint evanescent notch. These two notches indicate the division between the proximal and distal parts of the cardiac portion." It is in this distal segment of the cardiac portion that hour-glass contraction of the stomach occurs.

The mixing of the food is effected by the churning movement of the oblique fibres of the cardiac portion of the organ, in which region alone hydrochloric acid is secreted by the parietal cells. In this portion takes place the first part of digestion, and the sphincter aditus rejects and forces back such solid masses as may require further solution. When the material is reduced to a sufficiently fluid consistence it is propelled through the sphincter aditus into the pyloric portion by rhythmical contractions. This division of the stomach into segments is more clearly seen in the stomachs of infants in the later fœtal and early natal epoch.

The chief protein of milk is converted into casein in the first segment. In human milk this is coagulated into delicate flocculi, but cow's milk coagulates into large masses into which the hydrochloric acid of the child's stomach penetrates with difficulty. Hence there is a considerable quantity of it lying idle, and this produces an increased tension at the sphincter aditus. It is to these factors that the usual constant vomiting of infants fed on cow's milk is due, unless means are taken to obviate it.

The duodenum passes from the pylorus obliquely upwards and backwards to the right (grooving the under surface of the liver) to the neck of the gall-bladder, where it makes a sudden turn downwards. This portion is very freely movable. The second portion is firmly fixed by the peritoneum and pancreas, around the head of which it curls. The common bile-duct lies to the inner side and, with the pancreatic duct, pierces it obliquely just below its middle. The third or transverse portion is crossed by the superior mesenteric vessels, to which particular point I especially wish to draw your attention as important to remember in conjunction with the causes of dilatation of the stomach. The fourth portion does not concern us here.

The gall-bladder, when moderately distended, is an elongated, pear-shaped body, about four inches long by one inch broad at its fundus. It holds rather more than an ounce. It lies in a curved, approximately horizontal plane, in a sulcus beneath the liver, extending from the right of the transverse fissure to the anterior border of that organ, in a direction downwards, forwards, and to the right. The fundus may project slightly beyond the anterior border of the liver, in which case it touches the abdominal wall near the edge of the right rectus immediately below the ninth right costal cartilage, to which it is attached by a thin ligament. The best guide to its situation is the tip of the tenth rib, which is usually to be made out fairly easily, as its cartilage does not articulate with that of the ninth, but is attached to it by a ligament, which allows a certain amount of free play. The fundus cannot be normally detected by palpation.

As the gall-bladder is attached to the under surface of the liver by connective tissue and vessels it will move up and down with that organ during respiration, a point to make note of in reference to the diagnosis of tumours in that region. On its under surface the body of the gall-bladder is in relation with the first part of the duodenum, and occasionally with the pyloric end of the stomach. The neck of the gall-bladder is narrow and curled upon itself before it ends in the cystic duct; at this point there is a well-marked constriction. The cystic duct is about an inch in length, turns obliquely to the left and joins the hepatic duct at an acute angle, where it is in intimate relation with the duodenum.

Of all the maladies which may affect the various organs the anatomy of which we have been considering, I shall only deal with those which are amenable to treatment by electrical methods, either alone or in conjunction with other means, such as diet, lavage, and massage.

There is an increasing tendency in the professional mind to regard most of these diseases as only to be dealt with by the surgeon, and, no doubt, in many cases this is the fact; though, on the other hand, I am perfectly convinced that a fair number of those which are now relegated to the operating theatre may be perfectly well relieved and a cure effected by a treatment in which electricity is the chief agent employed. Most people have an innate horror of being cut about, however skil-

fully, and, although I should be the first to urge an operation in conditions which demanded it, I should also be the first to feel thankful if at any time an operation could be avoided, with equally satisfactory results to the patient, of course.

For our present purposes we may divide the disorders affecting the stomach into: (1) Functional, in which there is no appreciable alteration in structure, and (2) organic, in which the symptoms are accompanied by definite change in structure or shape, with or without actual displacement.

Functional disorders have been divided by Robin into the hypersthenic, with exaggerated secretion; hyposthenic, with diminished secretion; and those with perverted secretion, which we might well term "heterosthenic." When not caused by nerve disorders or reflex irritation these functional dyspepsias are but phases in the progress of the same malady, the gradual onset of motor insufficiency, due, as a general rule, to the villainous habit of bolting food without proper mastication, and, to a far less degree, to the quantity and quality of the food. Bits of *entrées* and lumps of animal food swallowed whole, with an insulating casing of fat or greasy sauce, cannot be properly attacked by the hydrochloric acid of the stomach; hence more of it is secreted to reinforce the normal amount. This excess begets tension and spasm of the central and pyloric sphincters, causing gastralgia, heartburn, and other well-known symptoms. This is the first stage—that of hyperchlorhydria. Owing to this constant over-stimulus the muscles become fatigued, lose the power of churning up the food, and make but feeble response to the stimulus of the hydrochloric acid, which still remains in excess. Peristalsis may be seen to occur, but it is not the steady rhythmical action of healthy digestion, but a series of spasmodic, jerky efforts, resembling the spurts made by a tired horse under the lash. This is the second stage, that of hyperchlorhydria, with motor insufficiency. The muscular relaxation and commencing atrophy are most noticeable at the cardiac end of the stomach. "Roughly speaking, the muscular tone is liable to be lost progressively from the cardiac orifice to the pyloric sphincter" (H. W. M. Gray).

After a time the secreting cells become exhausted, when we have the third stage—that of motor insufficiency with hypochlorhydria. The general tone of the stomach sinks lower and lower, and as the food is but feebly passed on it is not long before it begins to stagnate and lodge in the greater curvature. Being deprived of its natural germicidal protector, HCl, it falls an easy prey to the attack of saprophytic bacteria with its attendent train of symptoms. This condition soon passes into that of atonic dilatation.

This first stage of dyspepsia, of which I have spoken, seldom calls for other than medicinal and preventive treatment, but in the second stage, that of hyperchlorhydria with motor insufficiency, electricity is the one remedy to be used, for by it we can check the excessive secretion and restore the lost tone to the muscles. I do not think that it is advisable at first to administer a fillip to the muscles. They are tired and done up, so their first requirement is rest and nourishment by means of a fresh flux of nerve energy. This is best effected by the plan devised by Dr. George Herschell. He places a large moist pad over the epigastrium to form the anode, and a similar pad at the nape of the neck as the kathode. A continuous current of 3 to 15 m.a. is passed for from five to ten minutes. By this upward current the afferent nerves are soothed and pain is alleviated. The upper electrode is now changed to a much smaller one, which is applied with a reverse current of only 1 or 2 m.a. for one or two minutes in succession to each of the ganglia of the sympathetic in the neck. The method I adopt differs a little from that of Herschell, for I avoid placing an electrode on the nape of the neck or anywhere near the spine higher than the level of the eighth dorsal vertebra. The reason is that from the anterior roots of the fifth to the eighth dorsal spinal nerves there proceeds a branch which goes to the stomach supply and there exercises an inhibitory action over peristalsis. I place my negative electrode on the back below the eighth dorsal, and trust to the soothing effects of anodal electrotonus upon the solar plexus.

I can well believe that an undulatory current with a gradual rise and fall of quantity, as produced by the ingenious apparatus introduced to us by Dr. Hampson a few months ago, would be a decided improvement upon a steady and uniform flow of current. The excess of secretion having been reduced to the normal and the muscles refreshed, we expect the latter to resume their normal work, but they might require a little stimulus to start them: this is supplied by the

alternating or the induced current. Very little is usually required, and is given just as we might add a handful of beans to a horse's fodder after a hard day in the field.

In the third stage matters are more serious, for in addition to the motor insufficiency we have to face the cessation of secretion, showing an atonic condition of both muscles and nerves. The cure will necessarily be longer, but must also be begun by the galvanic current, as just described, and continued by applications of the induced current in the manner and technique described by Dr. Samuel Sloan, but if possible with a tidal rise and fall of current as produced by the apparatus designed for the purpose by Dr. Lewis Jones. The truly alternating or sinusoidal current may be used with advantage to increase secretion and restore tone. The method was described by Dr. Reginald Morton at the Exeter Meeting of the British Medical Association. I thought the tone of his paper rather too lugubrious, but we must not despair in these cases; they often will not yield to one form of treatment alone, nor to one technique. These obstinate cases may require intra-gastric electric treatment and the addition of massage or farado-massage.

Cases which have deteriorated so much that dilatation has ensued with fermentation require lavage of the stomach and intra-gastric electrical treatment. The first method of effecting the latter was to pass a conducting wire down the stomach-tube—not an easy matter if the wire is thin, and not free from danger if the wire is thick. In any case the wire was liable to be caught by any solid or semi-solid contents of the stomach. Einhorn constructed an intra-gastric electrode consisting of a bulb of hard rubber, perforated with holes which gave electrical connection by the fluids within the stomach to a very fine wire enclosed in a thin indiarubber tube for insulation. The mode of introduction of the bulb into the stomach is by deglutition, depending for complete success entirely upon the patient's prowess in swallowing, and often causing so much distress and revulsion that any good effect produced by the after electrical treatment was very seriously discounted. The shape of the bulb has been improved and the covering of the rheophore strengthened and made much thicker by Ewald, Marshall, Ferrannine, Boardman Reed and others, so that the operator is able to render material assistance to the patient in swallowing the electrode. All these electrodes depend for success upon the fluids already in the stomach and the water taken to aid the swallowing. I think that this method has some radical defects, one of which is that unless the stomach is previously lavaged, a current possessing electrolytic powers is let loose amidst a magma of horrible composition. What the electro-chemical effect may be I don't know. I think that even if the stomach has been previously washed out, it is a strain on the patient's fortitude to have again to submit to the introduction of another instrument. I venture to think that the method I have now practised for a good many years possesses all the advantages and none of the disadvantages of other modes. I will explain it to you, and you shall judge.

Early in the nineties Messrs. Maw, Son and Thompson made for me a lavage tube, into the silk core of which was woven a silk-covered wire of about seven to ten mils thickness of copper, one end of which issued near the mouth of the tube, and the other terminated in a thin bit of copper which formed part of the lumen of the tube near one of the fenestra at the closed end, so that no obstruction could be offered to the passage of flocculi or small masses from the stomach. For the actual preliminary washing out of the stomach I use the simplest apparatus. Nothing more is required than the lavage tube, a large glass funnel, and an india-rubber tube of large calibre connecting the two.

I have never found any real difficulty in passing the lavage tube, which I first prepare by dipping the stomach end for a little while into warm milk. This, being a natural bland food, seems to placate the pharynx and cheat it of its susceptibility to set itself against the passage of a strange body. When the tube is in the stomach the funnel is filled with warm water (100° F.) tinted to a deep pink by permanganate of potash. More of the same fluid is ready in a jug to add to that in the funnel before it has quite run down, so as to prevent any air from being carried into the stomach. When the stomach is full up I take notice of the quantity of fluid which I have used, as this gives me an idea as to the amount of the dilatation. The funnel is now alternately lowered and raised until the contents of the stomach begin to appear. The funnel

is now emptied and refilled with the pink solution. This manœuvre is repeated until the stomach becomes quite clean, as evidenced by the colour and clearness of the fluid in the funnel. The latter is emptied and, with its tube, disconnected from the lavage tube, the conducting wire of which is now connected up to the battery or coil as the case may be. I think it is always best to commence treatment with the galvanic current, the poles being arranged according to the symptoms to be alleviated. Afterwards, the induced tidal or the sinusoidal current should be used intra-gastrically. The technique is the same whichever current is used; the electrode of one pole is in place within the stomach immersed in a fluid conductor; the rheophore of the other pole is divided into a Y-shape, one limb of which is attached to a wet clay or swab electrode placed upon the epigastrium and the other to a similar electrode on the back below the ninth dorsal vertebra. Thus the cardiac and posterior portions of the organ, which, as I have mentioned, lie deeply to the back, get their fair and direct share of the current.

In galvanism the current should be from 20 to 30 m.a., maintained for about ten minutes. In faradism the duration is the same, but in quality it has been found that in cases in which there is but little secretion a low-tension current from a coarse and not over long secondary winding answers better than a high tension, which tends to diminish secretion.

The one remedy which is of the most value in restoring secretion, in improving the blood supply, in promoting absorption of redundant tissue, and in favouring the resolution of adhesions is undoubtedly massage in conjunction with faradism. I do not mean the alternate or successive use of the two, as text-books seem to imply when the two are mentioned together, and I must say that whenever electricity is mentioned as a remedy there is seldom, or never, a word about either the particular current to be used nor the best mode of application. I mean here the simultaneous use of the two remedies. I have been employing farado-massage now for a great many years, and for a long time was under the impression that the method was peculiar to myself, until (in 1889) I read in the excellent work by Stretch Dowse that he also was in the habit of using the same method of treatment.

The method which I pursue in gastric cases is as follows: One rheophore from the coil of induced current is connected to the swab electrode on the back of the patient in the situation already pointed out. The other terminal of the coil is connected by a long flexible rheophore to the person of the operator, either attached to one arm or to one ankle—of course, on the bare skin. I usually have a brassard (which serves also as an anklet) made from that ornamental, flexible, metallic ribbon that ladies wear round their waists, which is fastened by a buckle. It conforms well to the contours of the limb and gives good contact. The patient is placed on his back with the knees slightly drawn up and the mouth open. The operator stands at the right-hand of the patient and places his right hand upon the epigastrium with the butt resting over the triangular space, where the stomach comes to the front, and the fingers spread out over Traube's space. The right amount of current is adjusted, and pressure is made with the fingers, the hand being slowly drawn across to the right until the fingers clear the left costal cartilages; the pressure is now taken up by both palm and fingers, and as the direction of the movement of the hand gradually changes to an upward direction towards the pylorus, the greater curvature comes under the influence of the inner border of the palm and the little finger. By deep pressure these get well under the stomach and sweep slowly onwards, upwards, and to the right, until the region of the pylorus is reached. Whilst the right hand is going through this set of manœuvres, the left hand is made to glide along the line of the lesser curvature from right to left, and with a much more gentle pressure. In reference to that very valuable communication made by Reginald Morton as to the periodic time of muscular contraction, I should deem a setting of either Hampson's or Lewis Jones's tidal apparatus (if they will allow me to call it so), with tides or cycles attuned to the period of peristaltic action, to be the ideal adjunct to produce an optimum effect. The duration of a sitting should be of the order of ten minutes, and the strength of current may be such as not to give an uncomfortable feeling, yet strong enough to yield a good musical note as the hand passes over the skin of the patient. This note or hum is produced by the successive contractions of all the tiny little erectile muscles attached to the hairs of the skin.

Farado-massage sometimes restores secretion very promptly, so that it is as well to keep an eye upon the acidity by attention to the symptoms or by testing the contents of the stomach from time to time with a bit of congo-red paper.

In chronic dilatation we shall do much good by lavage, intra-gastric electricity, and farado-massage. It will be found that adhesions and inflammatory thickenings at and about the pylorus, the result of cholecystitis, as well as contractions from ulceration, often yield to these remedies in quite a surprising way.

Before commencing treatment it is as well to map out the actual position of the stomach. I remember a good many years ago being not a little disappointed at a patient vomiting a further supply of the horrible magma on his way home from a lavage. A dose of bicarbonate of soda, followed by one of tartaric acid, soon disclosed the presence of a pocket down in the left iliac fossa, due to a partial hour-glass contraction, probably caused by the contraction of a cicatrix after ulceration about the sphincter aditus. Into this pocket the lavage tube had failed to penetrate. This portion was treated by farado-massage and the viscera supported. The patient eventually recovered.

The best time for administering farado-massage is about two or three hours after a meal, when the chyme is ready to be passed through the pyloric orifice. There are certain contra-indications to either simple massage or Farado massage. They are—

(1) Ulcer, or any acute condition.

(2) Cancer, fatty degeneration of the heart, dilatation of the heart, advanced phthisis, aneurysm of abdomen or of thorax.

(3) During the menstrual period.

(4) Hyperchlorhydria.

Dilatation of the stomach may arise from dilatation of the duodenum without stenosis of the pylorus. This is often caused by cholecystitis, and it frequently happens in relaxed conditions of the abdominal walls that there ensues a ptosis of the intestines which causes a drag on the superior mesenteric vessels. You will remember how these vessels cross the third part of the duodenum, and you can see how any drag on them must result in a pinching of the gut between them and the structures in rear. Farado-massage is most useful in these cases, but the intestines must be supported in order to take off the strain. There is no better means of effecting this than by the special abdominal belt designed by Bradburn, and described by him in the 'Lancet,' August 26th, 1905.

There is one form of atonic dyspepsia with dilatation of the stomach which appears to be the consequence of disturbance of the nervous system, especially those due to toxæmia. The secretions are either normal or slightly under normal, but the motor insufficiency is woefully pronounced. There is no obstruction at the pylorus, yet the stomach is in a seemingly hopelessly depressed and inert condition. But it is by no means hopeless, for the symptoms yield fairly readily to treatment by the high-frequency effluve. Personally, I have met with great success by this method. I call to mind one case, that of a lady who had come over from Jamaica to try and get rid of her trouble, but did not improve under other remedies. She was then sent to me to see what electricity could do. The effluve caused the stomach to stand up at once in the abdomen, so that the whole dilated area was plainly seen. At each *séance* the organ could be watched gradually resuming its normal shape, until all symptoms had disappeared and the patient was well. Some six months afterwards (February, 1903) I received a letter from her saying that she was enjoying better health than she had done for years, and was able to walk and ride long distances without fatigue. This is the more satisfactory, as she had had a terribly rough voyage back, being confined to her cabin for three days. It is because of this fact that I quote this case.

About the same epoch Dr. Bokenham was using the same means, also with great success, as published in the 'Lancet,' October, 1902, fifteen out of seventeen cases having been completely cured, but I cannot find any other literature upon the subject.

Another form of dyspepsia is caused by the toxæmia of gout. Gouty dyspepsia is not easy to treat effectually by medicinal means. It has been said that remedies which lessen the gout add to the dyspepsia, and those which relieve the dyspepsia tend to aggravate the gout. The remedy of choice in these cases is the high-frequency current administered by auto-condensation.

Most of the symptoms of subacute and chronic gastritis may be effectually treated by electricity.

Pain and spasm call for galvanism either externally or intra-gastrically; stubborn vomiting may best be treated either by Ferrannine's plan of using the positive galvanic pole in the stomach immersed in a very dilute solution of strychnine, or, which I think is much safer, by intra-gastric faradism with as strong a current as can be borne (Borri's treatment).

Gastro-succorrhœa is most easily controlled by induced currents of high tension, especially intra-gastrically.

Amongst other currents of value in the treatment of gastric disorders the current of de Watteville must not be overlooked. In some muscular conditions it is the most potent remedy of all in restoring both tone to them and to their nerve supply.

Acute dilatation of the stomach is not infrequently seen after operations and from shock; indeed, in the latter it may be regarded as never being wholly absent, and I am sure that many less acute forms are due to mental shock, worry, and anxiety. Acute dilatation is evidenced by vomiting, which is very persistent, but has nothing to do with any anæsthetic which may have been administered. As to treatment, lavage is essential, but I should advise the static charge also, if possible, as this is the only form of electricity which can be relied upon to raise the blood-pressure.

In ulcer of the stomach we can do much good work with electricity. An ulcer may occur at any part of the stomach, but, according to Brinton, those on the greater curvature only amount to 2 per cent., and the same as regards the fundus, where they are situated in the upper part. Hence it is quite safe to pass a lavage tube into the stomach of a patient suffering from the three cardinal symptoms of pain, vomiting, and hæmatemesis. Yes! even the latter, for the sooner we cure the ulcer, the sooner the danger will cease. You will remember that in cases of dilatation I told you that I am in the habit of using a weak solution of permanganate of potash within the stomach. This has not only the effect of first disinfecting the contents of the viscus and then showing me when it is clean, but by the after-effect of the electrolytic galvanic current ions of manganic acid are conveyed to the walls. When absorbed this gives tone and quality to the blood. As ulcer is so common in the anæmic and chlorotic, this addition to one of the most important constituents of the blood is of great moment. Also the manganic acid and the oxygen set free have a direct healing action upon an ulcer. A solution of alum (with a tiny addition of sodium hydrate to take up the sulphuric acid set free) may also be used; zinc sulphate, too, is valuable both for its well-known local action and for its properties as a nervine tonic. But the finest remedy we possess, to be used in this way, is the bichromate of potash. Of course, the current will have to be run in accordance with whichever (metal or acid) we wish deposited or conveyed to the walls of the stomach. The length of time depends upon the strength of current and the amount to be deposited, according to the laws of electro-deposition.

Boardman Reed mentions "a group of symptoms which comprise especially tenderness over a region three or four inches in diameter, including the umbilicus as its centre, and a marked pulsation of the abdominal aorta in the entire epigastric region." "It may be the result of auto-infection from the gastro-intestinal tract." Also "it is benefited by kneading." I have no hesitation in saying that this condition is due to bacterial infection of the common bile duct, causing cholecystitis and sympathetic irritation in the duodenum, a condition which in many cases leads on to the formation of gall-stones and cholangitis. Relief is undoubtedly afforded by kneading, but to get the full value the kneading or *pétrissage* must be done in a particular way, which is equally valuable in the treatment of gall-stones by actual extrusion. I cannot here enter into either the ætiology nor the symptoms of gall-stones, but must limit myself to the description of a means of getting rid of them without surgical operation. Massage alone is useful, but cannot compare for a moment with farado-massage as regards effectivity.

The technique is as follows: As only one hand is used, one of the electrodes, if desired, may be held in the left hand; the other is attached to a swab in the usual place on the patient's back. The patient lies in the position as for gastric massage already described. In the case of ladies of thin habit and of short stature, the shirt-cuff of the operator must be well drawn up or folded back, otherwise it is apt to catch the anterior superior spine of the ilium and so cause keen discomfort to the patient. Our object is to squeeze the gall-

stones out of their resting-place through the cystic and common duct into the duodenum. Remember, therefore, the position of the gall-bladder and the direction in which we shall have to work. Notice also that when distended the gall-bladder comes a bit downwards and forwards. It may often be felt like a billiard ball, and I have known it as large as a good-sized pear. Lay the right hand with its palm to the surface of the abdomen and the butt below and to the right of the tenth rib, the fingers in a direction upwards and to the left. Turn on the desired strength of current, and then make firm, steady pressure with the butt of your hand deeply to the right of the gall-bladder, gradually pressing over it to the middle line, relaxing pressure as you clear the viscus. Let the fingers be well spread out over the left lower ribs and below them, transferring the bulk of the pressure to them, especially the ring and little fingers as the hand moves back again from left to right. The object of this is to produce contraction from the fundus towards the pylorus, for there is almost always a certain amount of dilatation of the stomach in cases of cholecystitis and its allied disorders. Also the passage of matter through the duodenum helps to encourage the exudation of the gall-stones through the opening into it.

In this movement almost all the pressure is in the fingers, less in the palm, and little or none in the butt. As the palm returns over the gall-bladder, turn up the inner border of the palm, and with good deep pressure squeeze the gall-bladder well up under the liver in the direction in which the viscus normally lies. By this means the fluid in the gall-bladder is made to push in front of it any inspissated bile, concretions, or stones which may be there. These massage movements are steadily repeated, gently at first, and then with increased pressure. You might think that such a proceeding would cause excruciating pain, but this is far from being the case, and it is not long before an immense relief is experienced. The reason is that the muscular fibres of the bladder have become worn out, and can only act in spasmodic jerks and twitches, whilst the cystic walls are closed around the solid matter which stretches them. This, then, is the cause of the awful agony. In my method a powerful, regular *vis à tergo* is applied, with a general electrical stimulation to all the fibres. The muscular coat is glad of this reinforcement, and responds to it harmoniously and not in painful jerks. By the steady pressure the fluid in the fundus distends the ducts evenly, in a manner somewhat similar to that of the liquor amnii. This obviates the painful spasmodic contractions and allows of the transmigration of the solid particles.

Furthermore, the stones, being usually soft and easily moulded, conform by pressure to the lumen of the tube and so get pushed through. Towards the end of the sitting, and as the swelling of the gall-bladder subsides, the tips of the fingers may be used to follow up and push in the fundus of the organ. I give from thirty to fifty cycles of massage movements.

Although a single sitting will not cure, it will give untold relief very frequently, and the patient will probably sleep comfortably afterwards. A daily or even bi-diurnal sitting for a week will effect wonders, except in those cases in which the stone is of a harder nature than usual and is firmly impacted, so that only a surgical operation can save life. The internal administration of oleate of soda, especially that preparation of it called cholelysin, during the period of massage treatment is a great help in softening the stones and so aiding their extrusion. I do not think that any case, except of extreme emergency, should be submitted to operation without a trial of this method of farado-massage with cholelysin. A little more than a year ago a lady came up from the country with only the prospect of an operation before her. She had suffered agonies of pain, and the gall-bladder was much distended. It was a bad case, but it yielded to this method, and the patient has been perfectly well ever since.

January 16th, 1911.

DR. EDEN, Obstetric Physician to Charing Cross Hospital, has been engaged in writing 'A Manual of Gynæcology' as a companion volume to his 'Manual of Midwifery.' The object of this book is to present a comprehensive account of the diseases of women from the standpoint of pathology and treatment. The date of publication is fixed for the middle of this month. Dr. J. B. Hurry, of Reading, whose investigations on 'Vicious Circles in Diseases' are already well known, has written a book on the subject. It will be ready in a few weeks. The publishers of the above books are Messrs. J. & A. Churchill.

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A LECTURE

ON

SURGICAL CASES MODIFIED BY INSURANCE.

Delivered at St. George's Hospital.

By G. R. TURNER, F.R.C.S.,
Surgeon to the Hospital.

GENTLEMEN,—I propose to say something to you to-day about surgical cases modified by insurance, and by those two recent Acts which now give so much employment to medical men, juries, arbitration and other courts, that is to say, the Workman's Compensation Act and the Employers' Liability Act. In the future, when qualified, you will many of you have to deal with workmen's claims, and the symptoms of the claimant will be modified almost certainly by the fact that these Acts are in existence. Being the surgeon to a large insurance company, I have at the present time to constantly bear them and their provisions in mind and to try, as far as I am able to, to see how things at the back of my patients' minds and interests are affecting them in their health, in their stories, and in their claims on their employers or the insurance company. First of all with reference to insurance. If a patient is insured he looks at his medical or surgical condition with a totally different eye than he would do if he were not insured. And although there may be no direct or deliberate mis-statement of fact or any intentional fraud, it makes a patient regard himself as an invalid and incapable rather longer than he otherwise would do. The medical man in attendance on the case, too, has to give the necessary certificates, and as far as he professionally can, to regard the matter more or less from the patient's point of view. There is a certain slight natural bias in the medical man's judgment in favour of the view taken by his patient as to his condition

with regard to the accident. Medical men, I am glad to say, are so constituted that they do not give false certificates, and of course I am not suggesting that for a moment; but in many cases of doubt, where there is legitimate doubt, naturally the medical man of the patient is rather favourably inclined to a view which would be of advantage to his patient. And rightly so. For example, there are many cases in which it is a little doubtful how far traumatism is to blame. Take, *exempli gratia*, a case of tubercular knee that has followed some slight injury to the joint or its neighbourhood, a wrench or trifling blow, something which would not give rise to anything like serious disease in the ordinary individual, yet in a tuberculous subject makes it the part of least resistance, and the seat after some time of tubercular deposit. Such a patient in due time will seek advice about this tubercular knee from his medical man, and almost the first question that is asked him will be, "Have you had a blow, have you had any accident or injury?" And then perhaps there may be remembered a history of accident or injury some months back, or if there is not at once this history of accident or injury, the patient racks his brain to remember if it is possible whether there has been an injury. Then he suddenly remembers, "Yes, I did fall down at the skating rink, or at this or that place, and hurt my knee." And then there comes the question of the direct or indirect effect of this possible traumatism. Let us take another instance of the same thing: say there is a case of tumour of a long bone, a sarcoma. You know how comparatively commonly sarcoma of bone is said to follow or be concomitant with injury. In fact, some years ago, there was quite a run on what was known as acute traumatic malignancy, and we were all able to show and to see cases of the kind. But there are many tubercular knees without injury, there are many cases of sarcoma of bone in connection with which there appears to have been no injury at all. These are but two illustrations of how the question of traumatism may arise and need your judgment in dealing with patients who are injured, and who may or may not be entitled to compensation. I have come across, before now, cases where the only evidence of any injury was the statement of the patient, "The doctor says I must have had a blow," and that, in the interested party's opinion, was sufficient to make a blow an antecedent fact—an instance of touching confidence in the omniscience of the profession not always shown us. There are other conditions which one meets with, all due to previously existing disease, often of long standing, which are attributed to accident. I have come across a case of profuse hæmoptysis and extensive old tubercular disease of the lung attributed to accident; a sudden attack of appendicitis with abscess attributed to accident; a perforation of gastric ulcer attributed to accident; subphrenic abscess resulting from it also attributed to accident; all these things attributed by the patient and his solicitors and advisers to accident, not to their real cause—disease. I have known a patient with renal colic, who was actually passing renal stones, attribute his condition to accident and "concussion of the spine." So also a rupture of previously existing aneurysm and an abscess following excision of rectum for malignant disease have led to serious disagreement as to causation. I have seen many such cases in the last few years, and they are all cases where there has been natural disease, sometimes going on to a fatal issue, and yet because there has been some trifling accident, some trifling strain setting up a fit of coughing, death has been set down to accident. Everything "post" is also "propter" an accident. Some of these cases have come on for arbitration or for settlement in court, and as medical men in the future you must be prepared to answer this question, which is invariably put by counsel for the assured: "Did this accident or so-called accident in any way accelerate death?" Let us suppose that you have a patient with extensive natural disease: let us suppose a woman has gastric ulcer on the point of perforation, and she has some little blow on the stomach, and that the ulcer perforates. No doubt that blow on the stomach does materially accelerate death, but death would not have happened but for the pre-existing, possibly fatal in itself, disease, the gastric ulcer, which perhaps had already leaked. And so although in one sense the patient has not perhaps died of an accident, from a legal point of view if any little trifling injury, strain or accident can be said to accelerate death, the patient is said to have been the victim of so-called accident. Now a word about that common condition, hernia, called so misleadingly, rupture. Rupture to the

ordinary layman pre-supposes that something is broken, and a large number of the laity believe that in the healthy state they may slip to the ground, receive a blow or meet with some trifling accident and suddenly wake up as it were and find themselves the subjects of rupture. Looking to the views as to the causation of rupture at present prevalent and their more than probable truth, that it is due to congenital deficiency of development, it is obvious that people who become ruptured suddenly have been for very many years the subjects of potential hernia, though not of an actual one perhaps until a sudden attack of coughing, or straining at stool, or lifting a box, or the doing one of the ordinary acts of life, makes the bowel descend into a sac that already existed. This, however, is not entirely the view of all medical men. But if there is anything in the saccular theory of hernia which is worthy of acceptation, none of us can suddenly become ruptured, any more than we can get suddenly criminal. *Nemo repentè fuit turpissimus.* We must have some tendency to rupture to become ruptured, just as everyone must have some tendency towards crime to become criminal.

Now a word about the Workmen's Compensation Act, and the bearing it has on the workman, as seen by us who are qualified, and as will be seen by you in the future when you are qualified. I take it that, however we may professionally regard the case of an unfortunate workman who has met with an accident or is ill, we all cannot help sympathising very much with the poor man whose ordinary life is, perhaps, one long toil and labour, trying to find bread for a number of youthful mouths which he has improvidently introduced into this world. I have the greatest sympathy with the workman who makes the most of that illness and sees in it an opportunity for enjoying a well-earned rest from toil. I am far from being desirous to turn the workman when ill as quickly as possible out of hospital and to certify him as fit to return to his work before he is really convalescent and well. I would rather err on the other side, and I think as a rule medical men do err on this other side. But there is no doubt that the Workmen's Compensation Act has made the period of convalescence from some of the ordinary diseases and accidents of workmen at least double what it was. When I was house-surgeon here some thirty years ago the one desire of the workman who had to come to hospital was to get back to his work: his one question seemed to be how soon could he get back to support those dependent upon him? If a man broke his collar-bone and came to the hospital, the first thing he wanted to know was how soon he could use his arm. This zeal to return to work is not noticeable at the present day: a man with a broken arm or broken leg will toddle along laboriously for fourteen months or more when there is compensation or weekly payment at the end of it. I do not think we can deny that this Act has made the convalescence of the poor very much more lengthy than it was before. And sometimes it is not the poor man who is to blame for this, because it constantly occurs that a man who has been injured is in a few weeks fit for some work, but not for all the work which he has been accustomed to do; he is fit only for light work. But many employers will not give the man light work; at least my experience is that those employers who will are the exception. They say, "We have no use for this man until he can do his full work." Sometimes it is some trade union or association that keeps him idle. So it is not always possible for the workman wishing to return to work to do so slowly and surely; he may at once be put on to ascending and descending ladders and scaffolding, and if he has had an accident to his head, with resulting, sometimes long-persisting, giddiness, as so often happens, he will not be fit at once for this work, yet may be well capable of other. In the same way a man who is partially crippled in the hand may be able to do some kind of work, but not the work which requires the full use of a good hand. I saw the other day the case of a hammerman earning £2 15*s.* a week, good wages for a working man. He had met with a slight injury to his shoulder, and had a little pain about his deltoid. The symptoms were entirely subjective; there was not anything to show that his muscles were injured in any way, no wasting, and nothing was apparently wrong. The movements were perfect, yet he had pain in his deltoid, and he would not return to work—or could not return to work, I ought to say. And although he was only receiving £1 a week while he was out of work, and was thus losing £1 15*s.* per week, he kept away from work. It appeared that he had to be certified as fit to do his *full* work as a hammer-

man or else he would be considered unfit to return to work at all, because he was not in a perfect physical condition.

I have very little to say this afternoon about the Employers' Liability Act. I think sometimes —though I am a motorist myself and insured against what is called third person risk—when I see Jehus driving about London and know how many accidents are caused by their more or less carelessness, I am rather inclined to doubt the advantages of any employer, or, indeed, anyone being able to insure against all possible risks except criminal negligence. As far as employers of workmen are concerned, if they are insured their liability terminates, and they are always anxious—and properly anxious—to do the right thing by their workers, and to get as much money for them as they can. But if the employer is not insured under the present laws it becomes increasingly urgent upon him, before he gets anyone to do his work, that such person shall be certified to be absolutely sound in every way. For instance, a man with phthisis, we will say, ought not to be employed or given any work at all, for he may rupture a blood-vessel if he has any but the lightest work to do; and, again, a man with such a simple thing as varicose veins ought not to be given any work, for if he has an accident to his leg he may get clotting in those veins and thrombosis, and may have to lie up for months and his employer be liable. Employers of labour get over the difficulty by insuring, and it ought to be for the insurance companies before issuing their policies to see that these men are sound in wind and limb. But if this were rigorously done by all the companies there would be a very remarkable increase in the number of the unemployed and unemployable.

I will now say a few words about various ordinary conditions which have to be modified, and which you will in future have to consider from the insurance point of view. I have already mentioned varicose veins; I daresay some of us here have varicose veins; they are no trouble to us, and they do not prevent us from doing anything ordinary in the pleasures or duties of life. But if you wish to insure against accidents when you have varicose veins, the insurance company will properly put a clause in the policy which will exclude the risks if from accident you injure those veins, for the reason that I have already given, the possibility of prolonged convalescence from clotting of blood in those veins, or even from the danger of the passage of some portion of clot from the veins to the lungs and heart, giving rise to that condition, which may vary from a trifling hæmorrhage to a fatal issue—pulmonary apoplexy. You have to look at these things, not from the purely surgical aspect, but to consider what difference they may legitimately make in the view of the insurance company on the prospects of the insured. *Inter alia*, a favourite subject for insurance is immunity from carbuncle. Most of us think we know a boil when we see it, and that we can distinguish it from carbuncle. I need not enumerate the differences to you. No doubt you know that boils often come, not in single spies, but in battalions, but where a big boil ends and a small carbuncle begins is often a not very well-defined matter. A man may have a policy insuring him against carbuncle. I have known very great indignation on the part of a medical man when he was not entitled, because he had six boils, to claim compensation for one carbuncle. Associated with carbuncles is the question of diabetes. This is very important from an insurance point of view. A diabetic patient may go on, under proper diet and under the advice of a wise physician, for a long time, and may live for years, but let this diabetic patient have a serious bruise or contusion, or something of the sort, and it is a very different matter. You know the liability to gangrene in diabetic patients, and so glycosuria will be found a very real obstacle to accident insurance, which you will have to consider if you have to act for an insurance company or for a person about to be insured. I said a little while ago that there is fashion in surgery—and so there is—and one of the latest fashions is the slipping of the semilunar cartilages in the knee-joint. The number of people who are averred to have slipped semilunar cartilages is very great, but I believe the number of people who have the real article is comparatively small. You must take this into serious consideration, for you may have to say whether a person shall be recommended for insurance who is alleged to have had a slipped cartilage. When, after an accident, a knee is swollen up with hæmarthrosis, or with effusion of fluid in the joint, and the medical man is told that something has slipped out from the knee, he naturally thinks of slipped

semilunar cartilage, and he may suggest that possibly the patient has that when really there is nothing of the kind. If the proposer for insurance has not had this dislocation of the semilunar cartilage he may yet have to state that he has suffered from it, and in the insurance policy there will be put a clause barring the particular accident or even all subsequent injuries to knee. And this, not because it is really necessary, but because of a mistaken diagnosis of the fashionable craze.

Another little matter upon which you may in the future be called upon to adjudicate. A patient has met with an accident to the knee: we will take the knee as a typical joint. He may have had some fluid effusion into it. Two or three months elapse and the knee does not get better; this lack of recovery is put down, naturally enough, to the accident. You may find, when you examine such a knee, that there is a very typical condition of osteo-arthritis of old standing, which has been in existence for many months, and is certain to have existed before that accident. The crackling and the various other symptoms of that condition may be most characteristic, and yet they may be thought in all good faith to be the result of the accident. You must be careful, after an accident which has laid up the patient for some little time, in accepting the statement that his present disablement is entirely due to the accident. I saw the other day a patient who had had a sprained ankle months before I saw him, and when I examined the "sprained ankle" I found extensive varicose veins on both sides of the leg, and exceedingly marked flat feet. He was not suffering from the result of a sprained ankle at all, although he was receiving pecuniary compensation on this ground. This was an illustration of how there may be perfectly legitimate errors on the part of the patient himself, especially when during convalescence he is seen but rarely by a medical man, and sometimes at hospitals by a multitude of different members of the house staff. Not long ago I had in this hospital a man, a scaffolder, who, two and a half years before he came in, had fallen on to both knees. His knees were badly bruised, but no bones were broken. He remained in bed for four months, and apparently was not able to work. And so he went on for *two and a half years* on the books of the insurance company. I was asked my opinion about him, but it was only, as it were, a paper opinion, as I did not see the patient, and therefore I did not know the condition in which he really was. But eventually he came up to London and I concluded that he had some adhesions in his knee-joint, and I asked that he might come into this hospital to have them broken down under an anæsthetic. He came in and I broke down his adhesions under an anæsthetic. And in passing I would say that if you give an anæsthetic for breaking down adhesions, give it with both hands, as it were, and see that the patient has as much as he can take and lies like a log for an hour or so after the operation, because the joint pain after breaking down adhesions is very great indeed. *Experto crede*, I have myself had adhesions in both my shoulder-joints broken down, and I know what the pain is after they have been broken down under gas alone. I took this man, then, into hospital, and he was given gas and a full quantity of ether, and I broke down the adhesions in his knee-joint and put him upon the domestic treadmill which I presented to the hospital, a sort of stationary bicycle called a "home trainer," on which the patient can pedal away as if he were on a treadmill, it is a first-rate machine for the movement of leg-joints around or in which there have been adhesions, and where you do not want much weight to be placed on the joint, but where you want movement. He came in here in July, and a month later he walked out of the hospital cured. Yet for two and a half years this man had been drawing invalid's pay, and had been doing no work at all, and all the time all that he wanted was movement of the joint under an anæsthetic. I have seen many similar cases, and often, as was pointed out long ago by Sir James Paget in a lecture on "Cases that Bone-setters Cure," the bone-setter rushes in where the surgeon fears to tread. A surgeon with better knowledge of the risks does sometimes look with apprehension on a case which the bone-setter will fearlessly and ignorantly tackle, for he knows what harm may be done to a tubercular joint, for instance, by too active measures. So that the surgeon is apt sometimes to delay the necessary treatment of breaking down adhesions, and mistakes are made in diagnosis between tubercular knee and adhesions. You may think that it is an easy matter, but I can tell you it is not; I have seen cases of tubercular knees diagnosed as adhesions, and *vice-versâ*.

Now a word as to how you should look upon age. Age is supposed to be a bar to insurance of

people against accident, and there is no doubt that aged people are more liable to die after serious injuries than are the young, because naturally they have not so much resisting power to shock and hæmorrhage. I have here a letter from Mr. Vian, the Secretary of the Railway Passengers' Assurance Company, who has made some investigations on this subject. He says: "Our policy-holders over seventy years of age form 8·14 per cent. of the whole number of policy-holders from eighteen years old and upwards. We should expect to find among the fatal claims upon us 8·14 per cent. over seventy years of age if the incidence of accidental death were equal at all ages. It is found, however, that the fatal claims in which the deceased are over seventy years of age constitute 11·44 per cent., a difference between anticipated and actual of 40 per cent. But figures are invariably fallacious, and although I see no mistake in ours, I have doubts whether a wider experience would show the same result." Old people, then, when injured, are more liable to death. But you must remember that old people over seventy as a rule are taken much greater care of than are younger, and they also take better care of themselves: they do not play at polo, or hunt, or steeplechase, or shoot so much; and therefore, from the insurance point of view, although the deaths may be slightly higher, yet they are not bad subjects for insurance. In one particular, however, they are bad subjects—*e.g.*, an old man has an apoplectic stroke, he falls, hits, and perhaps bruises his head, and then dies. His relatives attribute his death to accident—bring evidence of his fall. A sympathetic coroner's jury return a verdict of accidental death, and the insurance company usually has to pay, for it is really difficult to say whether the rupture of the diseased cerebral blood-vessel was the consequence or the cause of the fall and blow on the head. When I was a student I remember one used to read a great deal about spinal concussion, and there was a very eminent surgeon (Mr. Erichsen) who had written on the subject, and one was at the trouble, like every other student, to get up the very complicated details of spinal "concussion." Then Mr. Herbert Page, consulting surgeon to St. Mary's Hospital, appeared on the scene and wrote a book dealing with these cases of spinal injury, and showed that the cases of spinal concussion so-called were exceedingly rare, even if they existed at all. And injuries of the spine, unless they are very gross injuries, like fracture dislocation of the spine, you will find in your future life when you are medical men are the rarest possible things. Many a patient complains of his back because so much has been made of the back in the past in regard to injuries, and because it is so common a seat of renal, rheumatic, or gouty trouble, but as a matter of fact very few patients really have trouble in connection with lesions in the spinal cord other than those I have already mentioned—fracture, dislocation, or blood extravasation. And that is only what you would expect if you think how admirably the spinal cord is protected from all possible dangers. It is enclosed in a bony canal which is not in immediate contact with it; it is suspended in fluid, and around it are strong muscles. Consequently, although injury to the back may be followed by stiffness and immobility, this is more constantly due to muscular or ligamentous injury than injury directly affecting the spinal cord or the bones of the spinal column. With regard to muscular injuries and other injuries which are followed by this disability, especially in the back, you will find it is common for people after injury to the back to suffer from what is termed lumbago. I have had lumbago a hundred times myself, and I know what the real article is. It often comes like a "bolt from the blue." You suddenly feel a pain in your back and you cannot stand upright. There is a form of lumbago which follows upon injury, and I cannot help thinking that a very large number of cripples are crippled not so much because of their immediate injury, but because of the gout or the rheumatism, or whatever you like to call it, which they get on the top of the injury. I saw some years ago the son of a medical man who had had the amplest opportunity of getting the best professional advice in the Kingdom, and availed himself of it, but he was still a cripple, and I found he had done everything which cripples usually do. He had been to Bath and other spas and had had wet and dry local treatment, mud-baths, electric light, and any amount of massage, but still he was just as bad as before. I gave him alkalies and took the view that he had gout of the injured parts. A week afterwards he walked into my consulting-room, whereas previously he came on two sticks. That is an illustration of what you frequently see: gout or

rheumatism on top of injury, and it is well to remember that. If the muscles are stiff and do not resume their work; always be on the look-out for some such condition as that. Massage by all means, but don't neglect the general treatment.

With reference to the immediate treatment of injuries, one sees even now many cases where medical men and their patients have made too much of an invalid of the part affected. I do not know if any of you have read the excellent book by Mr. Wharton Hood, who himself had a very large experience of injuries in olden days. He points out that minor injuries, sprains and injuries to muscles should not be treated by rest, but by early movement, and that the elasticity of the skin should be counteracted by the use of a plaster bandage, which, being put over the skin, does away with its elasticity, and allows the patient with a partially torn muscle or torn ligaments to get about in spite of the injury. I am sure this is very good surgical treatment. And it is one of the difficulties in dealing with people who are injured and insured, or workmen who are liable to receive compensation, to make them, when getting well, get about if there is pain. You have to explain to them that pain is a necessity on resuming movement if they wish to get well quickly. Let us take such a thing as contusion of the shoulder with a tendency to adhesions in a gouty or rheumatic subject. If that patient is to get well he must move the shoulder, in spite of the pain, and not rely solely on the arm of the masseur or masseuse as the case may be, but do it himself. Voluntary effort, although painful, is necessary, and it is difficult to get workmen to realise and do this, especially if they know they will get compensation of half their wages from their employer, and perhaps the other half from a sympathetic corporation. The point of the whole matter is that it is necessary pain. And that leads me to speak of one more matter in conclusion, namely, about operations in connection with the Employers' Liability and Workmen's Compensation Act. When people are disabled from accident or from some disease, we will say hernia or a painful scar, with possible implication of nerve, a badly set fracture or some injury to the head, where operation would do good, you as medical men will have to give your patients advice as to what they should do, and you will find this a difficult matter. You may sometimes well wonder what you shall suggest to the insurance company, or how you should advise an unwilling patient. Let us take the case of a man with a broken leg. You are all familiar perhaps with Mr. Arbuthnot Lane's views as to how much a broken leg affects the capacity of a workman, how much per cent. is taken off his usefulness by a fractured leg, and you may remember that he recommends that when there is any bad fracture, although simple, it should be submitted to operation, and the displaced bony fragment brought together into apposition and wired or screwed. It may be the right advice at the time, or even afterwards. But your insured patient may say, "I do not want to have my fracture wired; I do not want to undergo the risk." And the Courts have decided that a patient is not bound to undergo a risk unless there is a reasonable certainty he will be cured, and that the risk is not a real risk to life. But even in these aseptic days there is a possible risk—even in a trifling operation. We know that in the case of an injury to a finger, requiring operation, amputation is as an operation a mere bagatelle, yet so long as we are human it will be possible for some of us to err, and the finger may go septic after operation, although it practically never does. Here is a case which illustrates the point. I have a patient now in the hospital with old head injury and painful scar, who I believe might be put right by operation; he refuses to submit to the risk that the collective wisdom of our surgical staff at last week's consultations declared to be non-existent, and he is master of the situation. He will not have an operation. The position of the assured workman is very often that of one who is granting a favour to the operating surgeon by permitting him to operate, forgetting that the operating surgeon suggests the operation, and is prepared to carry it out to cure the patient or to do him good. It is almost usual now that the patient condescends to allow himself to be operated upon, and it is a very difficult matter sometimes to make up your mind that the benefit likely to come is so certain that you can insist on operation in the way allowed by the law.

And I should like to say here that I hope the time will come when the decisions in cases where expert medical evidence is given will rest with a board of medical men, by whom, of course, the niceties and technicalities of their evidence and terminology can be adequately appreciated and apprehended; or anyhow, that there shall always be a medical assessor in courts of arbitration.

I have been able to touch on but few of the insurance problems which may confront you in your future work, and as my time has practically expired I must leave off without touching upon malingering, the estimate of times of partial or complete disablement from disease or injury, and other allied topics, all the more necessary for you to be familiar with if you happen in the future to be appointed medical or surgical referees to an insurance company, or have to deal as medical officer with large bodies of working men.

January 23rd, 1911.

VERTIGO.*

By SYDNEY SCOTT, M.S., F.R.C.S.

LADIES AND GENTLEMEN, — I need make no apology for choosing the subject of vertigo as our study for to-day, when I remind you that Sir William Gowers, in his text-book, 'Diseases of the Nervous System,' published in 1888, stated that some affection of the labyrinth was the commonest of all the causes of vertigo, and that out of 106 consecutive patients who consulted him for vertigo, no fewer than 94 owed their condition to the disordered labyrinth. You will therefore realise the importance of the subject from our point of view. Sir William Gowers' reason for attributing the predominant cause to the labyrinth was principally the association with the vertigo of ear symptoms of which the patients complained on inquiry, or which were found by examination, namely, deafness or tinnitus. He informs us that upon examining these patients he found that all showed some loss of bone-conduction to tuning-fork sounds. The test known as Schwabach's was applied by means of one of the lower middle octave forks, *e.g.* one of the C's (256 d.v. per sec.), which would be placed on the vertex of the head or on the mastoid process. If the patient cannot hear the sound for as long as a normal person treated in exactly the same manner, there is said to be so much loss of bone-conduction.

Nomenclature.—Dizziness, giddiness, and vertigo are used almost synonymously. The term "dizziness," according to Skeat, is derived from a word which means dull or stupid, and that expresses the sensation of some patients who experience the slighter degrees as well as the severer forms of vertigo. "Giddiness" comes apparently from "gyd," a song, hence "gyddian," to sing, to dance, and then to whirl. Continental writers seem to employ only the one word to describe the condition. We may reserve the term "dizziness" for mild forms of vertigo, and "giddiness" for the more protracted forms, although there is no essential difference in meaning.

The state of consciousness. — Dr. Hughlings Jackson and Sir William Gowers regard vertigo as a state of consciousness of the effect on locomotor co-ordination of discordance between the afferent impressions, upon which co-ordination depends.

Essential features of vertigo.—Vertigo may take the form (1) of a sense of disturbance of subjective orientation (Bonnier), that is, of our sense of our own position in relation to space; (2) a sense of disturbed objective orientation, that is, of the position of our surroundings in relation to ourselves in space; (3) the third feature appears to have received very little attention in our text-books, namely, the occurrence of forced movements.

Forced movements.—In connection with forced movements, let us consider for a moment that when we, as normal persons, assume an unstable position and feel ourselves about to fall headlong, we may recover our equilibrium by muscular exertion. This effort is the outcome of reflex processes, and is not a purely voluntary act. The forced movements in vertigo—for instance, the staggering gait, the reeling, the falling forwards or backwards, or to one side, the movements of the eye-balls—are all the results of similar reflex processes.

Topographical perceptions.—We shall understand the character of the impressions which guide our topographical perceptions if we consider a semi-hypothetical case, and imagine an individual with a fracture-dislocation in the cervical region, or a lesion which no impulses can pass, either upwards or downwards. Although rendered unconscious of the topography of the limbs and trunk, the patient will still retain consciousness of the position of the head, even when the eyes are closed. If the head be inverted (a dangerous experiment in the case of a traumatic lesion) there is no doubt the patient would be aware of the fact. Even if totally deaf so that no auditory impressions could furnish information by which it might be possible to judge the direction of sounds, he would assuredly still know the position of the head. The question is suggested, would this position be known if the semi-circular canal-apparatus were destroyed? I certainly think it would, for in resting postures of the head pressure of gravity would affect the fifth nerve, and the other cutaneous nerves of the scalp and sensory nerves of the skull, and, moreover, muscular impressions from cervical muscles would still be retained. We must not, therefore, assume

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that the semi-circular canals are indispensable, although normally, no doubt, we depend upon these organs very largely in estimating the position of the head.

Vertigo due to discordant impressions.—Discrepancies between the afferent impressions received simultaneously along any path concerned with orientation, especially the vestibular nerves, the muscular sense-tracks in the cord and in the ocular-motor and visual paths may bring about vertigo.

These three sets of impressions are intimately related. Hyperstimulation of one or both labyrinths, or unequal stimulation of both labyrinths, will set up forced movements in the eyes, or in the neck, trunk, or limbs. Simultaneous impressions are derived from the muscles thus set into reflex action as well as from those paths concerned with the inhibition of muscular action.

Vestibular impressions.—Clinically, vertigo is far and away most commonly caused by some disturbance of impressions derived from the semi-circular canals. Sir William Gowers did not indicate all the cases of vertigo as having this common cause. He left nearly 10 per cent. of his cases to be accounted for by tumour cysts and vascular lesions of the medulla and cerebellum or of the central connections of the vestibular nerve, also by certain frontal tumours, and by lesions involving disturbances of the ocular-motor muscles, which alone are represented by a relatively small group. We should bear in mind, too, that Gowers no doubt saw an unusually large proportion of such cases. In a small number of instances vertigo is sometimes associated with certain general diseases, such as malaria, a number of examples of which have been recently contributed to Italian literature. There is also Gerlier's disease, known as paralysing vertigo. Vertigo is also said to be a common symptom in the early stages of pellagra, a disease which, as you know, appears to be caused by one of the parasites (see Dr. Sambon's 'Journal of Tropical Medicine,' 1910). We do not know which particular groups of impressions are affected by the parasites or virus in these diseases, but it is at least as likely that labyrinthine impressions are as much involved as any other.

Vertigo in middle-ear affections.—There is a form of vertigo not included in Gowers' statistics associated with relatively increased bone-conduction, which is a recognised character of middle-ear, in contrast with internal ear affections. A classical case was that of a patient under the care of Dr. McBride, many years ago. Vertigo which was associated with a strongly retracted drum was completely relieved by inflating the Eustachian tube. As a matter of fact the condition is not so rare as it was formerly supposed to be, and otologists of long experience refer to many cases of vertigo with signs of middle-ear deafness, and without any other evidence of internal ear affections (see Dr. Dundas Grant's "Vertigo," published in the CLINICAL JOURNAL several years ago).

No doubt the effect is produced by some physiological disturbance of the labyrinth, in these cases not necessarily due to any gross pathological lesion in the internal ear itself.

Effect of ablation of labyrinths.—What other evidence have we that the semi-circular canals are important factors in the production of vertigo? While numerous examples of one-sided destruction of the labyrinth are met with, it is only occasionally that we meet with instances in which both sets of semi-circular canals have been removed by operation. A case was published by Mr. Lake and Dr. Gibson in the 'Proceedings of the Royal Society of Medicine,' 1908. Mr. Lake afterwards kindly allowed me to examine the patient, not only by slow rotation, as he had done in order to see whether the patient was sensitive to movement, but by fast rotation, and then a sudden stop. As you are well aware, most individuals experience definite symptoms of giddiness after being turned round even comparatively slowly when the motion is suddenly arrested. The experiment is quite an old one. It is most probable that the fluid in the canals flows by its own inertia after the sudden stoppage of the head; at all events we may regard this as the stimulus which excites the vestibular nerve-endings. Usually three revolutions in fifteen seconds are sufficient to cause giddiness and nystagmus. But Mr. Lake's patient was subjected to a severer test after milder measures had failed, and was rotated twenty times, and at a rate more than twice as fast as usual, but on sudden stopping no sensation or appearance of giddiness whatever was produced. The same procedure was carried out with the head in different positions, all with a similar negative effect.

Bilateral auditory nerve tumours.—After the ves-

tibular nerve on its way to the medulla has been destroyed by disease, we find that galvanism fails to produce giddiness. This was observed in a patient who was under the care of Dr. Gardner (Shrewsbury). A man had complete bilateral facial palsy, with total deafness on both sides. His symptoms began eight months previously, with giddiness, then deafness, followed by facial paralysis. The giddiness had passed off and the patient could walk unaided when I saw him, although his movements were very deliberate and cautious.

A full examination showed that the patient evidently had some lesion of the seventh and eighth nerves on both sides. As in Mr. Lake's case, no giddiness could be provoked by rotation, and the galvanic current passed through the head from one ear to the other produced no sensation of dizziness and no nystagmus or other reflex movements. The patient bore a current of nearly 20 milliampères, which (as regards the strength of the current) is almost the limit of toleration for any ordinary person.

The state of equilibrium controlled by muscular sense.—It may be asked how individuals who have no labyrinth to guide their movements manage to walk and turn and so forth.

There is no doubt muscular impressions now become paramount in importance. It was almost amazing to find the state of efficiency acquired by muscle-sense in the case of the young woman whose internal ears had been destroyed, although it took many months of practice. She could turn herself rapidly round and round, stand on one foot or the other without losing balance with eyes open or shut. In fact, she seemed preternaturally steady. The man with a bilateral lesion had not had time to attain such proficiency when I saw him.

Vomiting associated with vertigo; its importance in diagnosis.—There is one symptom which is frequently associated with vertigo upon which I should like to dwell for a moment or two. I refer to vomiting. Patients are seized with vertigo, which is followed by vomiting, pallor, feelings of faintness. Sometimes these attacks are called "biliousness." I have seen several treated as "influenza." The idea that "biliousness" should cause vertigo is, as Gowers pointed out, an erroneous one. Raymond, of Paris, has recently emphasised the same statement, and advises particular examination of the ears of "giddy dyspeptics." There are, no doubt, patients who are wrongly treated for "biliousness," when the ear is really at fault, or "gastric influenza" when the disease was infective labyrinthitis, which occurred during an "influenza" epidemic. We should regard this as a serious fallacy when we recollect that vertigo may be a timely symptom for surgical intervention, by which an infection in the labyrinth may be prevented from spreading to the meninges.

Gastric symptoms in vertigo due to quiescent disease of the ear.—The following was an instructive case. A young woman was sent to this hospital by Dr. Gray Duncanson for vertigo. For two years before being seen by Dr. Duncanson the patient had had repeated attacks of "biliousness" and giddiness, which had been attributed to gastric catarrh and possible gastric ulcer. It was difficult to get the patient to realise that the ear would account for the symptoms, although on one side the hearing was defective and there was occasionally a little discharge. Through the speculum some small granulations and much epithelial *débris* were visible. I also observed rotatory nystagmus to the right side, *i. e.* to the unaffected or opposite side. When trying to walk unaided the patient deviated toward the right side, and when Romberg's test was applied she fell obliquely backwards and towards the right.

All the tuning-forks were audible with increased bone-conduction on the left side, but there was some loss of the highest and lowest tones in the left ear by air-conduction. An exact record of the hearing-tests showed the deafness to be of the middle-ear type. Attempts to stimulate the left superior and horizontal canals by irrigation and rotation resulted in a negative response, and no effect of giddiness was produced by increasing the intra-meatal pressure. I concluded that there was antral disease, with invasion of the semi-circular canal apparatus, but without complete destruction of the labyrinth. At the operation on the mastoid which I performed, a fistula was discovered in the external semi-circular canal beneath a large cholesteatoma, which was pressing upon it. After the operation the patient said she felt quite a different person. The sense of constant giddiness had disappeared and the attacks of "biliousness" ceased, and in a short time the nystagmus was difficult to detect.

Every day in aural departments one meets with patients who suffer from giddiness, but these are not all cases of labyrinthine *disease*. The occurrence of vertigo among patients in aural clinics is so frequent that one regards the sign as one of the common manifestations of ear disorder, just as important as discharge from the ears, noise in the head, or deafness.

Ménière—a retrospect.—In 1861 Ménière described nine cases of giddiness associated with deafness and tinnitus. At that time the work of Flourens was being much discussed, and other experimental physiologists were entering the field. It was a new idea that the semi circular canals had to do with subjective orientation before Flourens' observations were made. Even as late as the sixties a text-book on physiology (John Marshall) described the functions of the semi-circular canals as that of determining the direction from which sound-waves originated. This hypothesis was the one which held sway for perhaps a hundred years or more. Flourens' statements were at first received with considerable scepticism, and his critics said he must have injured the cerebellum to get the inco-ordination effects which he ascribed to the semi-circular canals. As a matter of fact, the posterior canal in pigeons is very close to the cerebellum, and it is difficult, if not almost impossible, to divide this canal without opening the cranial cavity, but the other canals can be readily cut without disturbing the brain.

What Ménière did was to apply Flourens' data to a consideration of clinical cases of paroxysmal vertigo. He noticed the following symptoms: tinnitus, giddiness, vomiting, and deafness, all of which symptoms tended to recur simultaneously, or in rapid succession, and which formerly had been considered due to cerebral hæmorrhage.

In the nine cases he reported the middle ear was thought to be normal, and Ménière could not suggest any pathological cause until he had an opportunity of making a post-mortem examination in a tenth patient who had this complex of symptoms.

Ménière's tenth case; an autopsy.—The patient was a girl, æt. 19 years, who, riding outside a *diligence* one winter's night in Paris, had caught a severe cold, became deaf, and was so ill with giddiness, vomiting and headache that she was put to bed. The least movement of her head increased the giddiness and brought on vomiting afresh. The headache increased, the patient became drowsy, and finally comatose, and died on the fifth day of the illness.

Post-mortem.—Ménière says the brain, cerebellum and spinal cord appeared absolutely normal. He opened the superior semi-circular canal and found it contained a little blood-stained plastic exudate. There was a similar but paler exudate in the vestibule, and the fluid in the cochlea looked more normal.

I can find no description of the middle ear in this case nor a statement of the side affected. But Ménière established his point that there was disease of the semi-circular canals to account for the vertigo.

We should not expect to be able to establish an accurate pathology on a case like this at the present time, but it has generally been supposed that Ménière's case was one of labyrinthine hæmorrhage. Indeed, Gustav Alexander, in his researches into leukæmic deafness and vertigo, seems disposed to regard Ménière's case as one of leukæmic labyrinthine hæmorrhage, a view which I do not see sufficient grounds to share.

A parallel—acute internal hydrocephaly.—It so happens that among my cases one presented very similar features to Ménière's; it was described in the 'Archives of Otology' a few years ago as an example of streptococcal labyrinthitis with acute internal hydrocephalus; it was really a fulminating form of serous meningitis, although after the removal of the brain from the skull it was almost impossible to detect the evidence of disease in the brain or meninges.

In my patient, a boy, the symptoms came on after an operation for middle-ear disease. He desired to be left quite undisturbed. The slightest movement of the head brought on attacks of vomiting. In a few days headache began, and increased in severity until the eighth day, when he died somewhat suddenly. At the post-mortem the brain seemed curiously firm and tense all over the surface, and it was remarked that it had a resemblance to a brain which had been hardened in formalin. However, on cutting horizontally through the ventricles, as the brain remained *in sitû*, out gushed a quantity of cerebro-spinal fluid, and the brain became flaccid. You will note that the brain was not removed in the ordinary way,

but was divided *in sitû*. No organisms could be discovered in the films of the fluid or in culture media. When one separated the tentorium, and divided the brain, layer after layer, one found that the seventh and eighth nerves on the left side were surrounded by a collection of fluid which was enclosed in the pia-arachnoid sheath which envelops these nerves. The fluid was not quite so clear as that in the ventricles, but no organisms were discovered in it by bacteriological tests. No other nerve-sheath was involved. After the ventricular fluid had drained away from the brain no sign of abnormality was discoverable, either in the brain or in the cranial cavity, with the naked eye.

The superior semi-circular canal was chipped open on both sides. The right side was normal, but the left canal contained a blood-stained exudate, and cultures of this exudate taken from the vestibule of the left labyrinth yielded cultures of almost pure colonies of *Streptococcus pyogenes* (Gordon). Moreover, it was found that the infection had extended to the labyrinth from the tympanum. The presence of these organisms in the labyrinth would also account for the meningeal effusion which had determined the death of the patient.

I feel justified in submitting this case as similar, so far as the autopsy was concerned, to Ménière's tenth case, although we are not furnished with sufficient details in that classical case to make the parallelism complete.

Supposing I had removed the skull-cap in the usual way, and cut through the olfactory tracks, optic nerves, stalk of the pituitary body, and infundibulum of the third ventricle, and then the other nerves, pair by pair, beneath the base of the brain. All we should have seen after the brain was removed would have been a certain amount of cerebro-spinal fluid, which had collected in the cerebellar fossæ (assuming the head to be resting on the occiput). This fluid would have escaped from the ventricles when the infundibulum was divided, but we should have failed to find anything abnormal in the brain after it had been removed, just as happened in Ménière's case.

Bárány's new tests (1910).—I must pass on to speak quite briefly of certain cases of unilateral cerebellar disease as they have come to my notice, in connection with the vestibular apparatus. In a paper read before the Neurological Section of the British Medical Association in London last summer, Dr. Bárány, of Vienna, described some new tests, which depend upon the functional activity of the labyrinth, in investigating cerebellar disease. According to Bárány, whose facts I can myself vouch for, a patient with a unilateral lesion of one cerebellar hemisphere reacts to labyrinthine stimulation in some respects differently from either a normal subject or a patient with unilateral labyrinthine disease. If a normal subject is seated erect in a chair which is rotated uniformly a certain number of times and then suddenly stopped, an attempt to point to a fixed object in front of him with eyes shut will result in a deviation of the finger several inches to one side of the object. This deviation is the result of a normal forced movement, similar to those to which we have already referred, and we are to regard it as a reflex produced by hyper-stimulation of the horizontal semi-circular canals. Either hand will behave in this way, the direction of deviation being controlled by the direction of rotation.

In four cases of unilateral cerebellar disease which I have had the opportunity of testing (three with Dr. Bárány last summer and one more recently), the deviation reaction in the "pointing" test was absent in the ipsolateral limb after the chair was turned to the contra-lateral side. As both labyrinths responded separately to other forms of stimulation, as evidenced by the nystagmus tests, it appears as if the cerebellar lesion interferes with the deviation reaction which Bárány described. I am not prepared to say more regarding these cases at the present time, and I refer to them merely to draw your attention to the possibilities of normal labyrinthine reactions being an aid in the investigation of cerebellar disease.

The principles of treatment for vertigo.—When a patient complains of giddiness, with or without noises in the head, the first thing to do is to carefully examine the ear. Defective hearing is not necessarily observable, for reasons which I must leave you to see. Vertigo may be due to impacted cerumen pressing upon the ossicular chain; if so the symptoms will disappear on removing the cerumen. Giddiness may be associated with an affection of the middle ear, and may be due to an indrawn drum. If so it is amenable to treatment by inflation with the Eustachian catheter or Politzer's bag. *When vertigo is accompanied by acute otitis media* we need to be circumspect. The tension of an effusion into the tympanum should

be relieved immediately by incision of the tympanic membrane. Sometimes such effusions are not accompanied by so much actual pain as by discomfort. We must not always expect to find tenderness to pressure around the ear in such cases. Sometimes such effusions are absorbed without resorting to puncture, but the latter is a wiser course as a rule.

When vertigo occurs as a complication of chronic disease of the middle ear the only treatment is operative. The condition may be due to a cholesteatoma, as in the case I recited to you, which represents a frequent cause; the radical mastoid operation may be sufficient, particularly when the defective hearing is of the middle-ear type. But if the vertigo is accompanied by such loss of hearing as betokens a widespread extension of the disease into the labyrinth, then it is not wise to be content with an operation on the middle ear; we must then follow the infection into the labyrinth without delay, for fear of the infection spreading to the meninges.

Vertigo unaccompanied by gross middle-ear disease.—According to the doctrine of chances, at least 90 per cent. of such cases are labyrinthine (Gowers). The precise cause of the vertigo, even in these cases, may at first be a matter of conjecture. The subject represents a wide field for fruitful research. Evidence is accumulating to show that the complex is not invariably due to hæmorrhage into the labyrinth, or into the auditory nerve (see Bruce and Fraser's case), or to thrombosis, or embolism of the labyrinthine arteries. Primary degenerative processes have been found in the ganglia of the vestibular nerve, and this may be due to toxins which we have yet to seek. The principles of treatment in these cases will be governed by the associated conditions or signs of disease in other parts of the body, the vascular system, the digestive tract, and so forth. Repeated observations must be made in doubtful cases, the eyes carefully tested, Wassermann's test applied, and appropriate treatment carried out should syphilis prove to be present. We must be on the look-out, too, for signs diagnostic of an intra-cranial lesion, with a view to the possibility of a cerebellar tumour or cyst which could be relieved only by an *intra-cranial operation*, or the case may prove to be one of vascular cerebellar disease.

Bromides.—For the symptomatic relief of vertigo the bromides are the most useful drugs, but their administration may be interfered with by vomiting, and some patients display an idiosyncrasy for the drug in whatever form. Too small doses, in some, act as excitants. In others a contra-indication arises from the cutaneous eruption a few doses produce.

In spite of these drawbacks the bromides represent the most reliable standby. It may be necessary to increase the doses from 10 to 40 gr. three times a day (or *pro rata*), and this should be done if the drug is well borne, while smaller doses fail to produce the desired effect. To patients intolerant of bromides, we must select some other sedative such as phenacetin or aspirin, although the larger doses of these preparations are precluded. Small doses of quinine are sometimes efficacious. Iodides are useful in others.

During paroxysms it is of course necessary to keep the patient at rest, and particularly to immobilise the head. When patients remain giddy for days together, in spite of rest—"*status Ménière*," "*subintrant crises*" (Charcot)—*lumbar puncture* is a rational procedure, as Babinski pointed out some years ago, when he described a number of successful cases treated by this means. Dr. Stewart also has adopted this measure, I believe, with success in suitable cases. I need only mention its diagnostic value.

Conclusion.—Vertigo and Meningitis.—I conclude by warning you not to overlook a removable cause in attempting to relieve the symptoms of vertigo, and I would remind you once more of Ménière's tenth case, and to remember that a patient ill and in bed may not tell you of the onset of giddiness. The disturbance may be mistaken for weakness, or peevishness because he dislikes to be moved. Vomiting and pallor caused by change of position should make us think of the labyrinth. It is by no means easy to diagnose labyrinthitis when these symptoms are common to so many other affections, and death from meningitis may occur as a complication of unsuspected internal ear infections: the significance of the earlier symptoms was not appreciated and their cause not ascertained by suitable tests. Let us ever remember that otitic meningitis is a complication the danger of which is heralded in many cases by the occurrence of vertigo, and is preventable by a timely operation if we interpret the warning sign correctly, and while the infection is yet limited to the labyrinth.

January 23rd, 1911.

THREE LECTURES
ON
CANCER OF THE RECTUM.

Delivered at the Polyclinic.

By FRED. C. WALLIS, M.B., B.C., F.R.C.S.
Surgeon to Charing Cross Hospital.

Lecture III.

Ladies and Gentlemen,—My last lecture on this subject has to do with post-operative treatment. In the after-treatment of this formidable complaint there is much to be known, and those who are in the habit of treating these cases should be able to give you some useful hints so as to assist you, and at the same time help the patient. With regard to all rectal work, you may take it as a matter of course that nothing pays you so much as really good nursing. To my mind the nursing depends first of all upon the man who is looking after the case: he directs his nurse. There may be all sorts of nurses, but you should have a nurse who will carry out your instructions properly. That is all they are there for, though some of them have their own ideas, some bad, some of them no doubt good. A good knowledge of the post-operative treatment of rectal trouble is one of the best forms of knowledge you can have, for your own comfort, because the case is less trouble to you, and the after-results are as much more satisfactory. Whatever the form of operation among those which we discussed yesterday, where the peritoneum has been opened and the operation has been a long one, it is generally desirable to put some saline into the patient by transfusing saline into the sub-pectoral fascia. One and a half pints of it can be used certainly with much advantage to the patient. One practical point is that this may have to be repeated three or four times in bad cases where there is much shock, in which case do *not* leave the needle in. Every time I have seen a needle left in a painful abscess has been the result, although every care has been taken. I need not bother you with a statement of why this is, but I may say I took a culture of some pus in a case which recently occurred in which there was abscess following the leaving in of a needle for twelve hours. The result was a staphylococcus, though whether it was the white or the yellow form I do not know, and there were some curious micro-organisms which looked diphtheritic. If it is necessary to reintroduce the needle, do so; it saves troublesome conditions afterwards in the shape of abscess.

In all rectal cases it is best to use plenty of packing, for in these big cases there is much oozing afterwards for eight or ten hours. So put on plenty of packing, gauze, wood-wool, etc. This should be removed on the same night as the operation, the operation being presumed to have been done in the morning, which is when most of them are done. If this is not done the dressings get hard and caked, and are uncomfortable to the patient. The patient is turned on his side, the T-bandage is undone, and the superficial bulk of the dressings removed and taken away and fresh ones applied, but not so tightly as immediately after the operation. Most of the cases are all the better for a little morphia, but very little is wanted—one-eighth of a grain, which may, however, be repeated, and then resort to aspirin. As a rule these patients do very well with morphia, followed by aspirin. With regard to the diet: after the operation, hot water or any liquid which the patient can take without being sick is desirable; sickness is sometimes stopped by the drinking of bicarbonate of soda and water. Above all things avoid giving ice or cold water. Next day the dressings are again removed, and the whole of the area outside is irrigated with saline, and round the wound and also around the anus irrigate with peroxide of hydrogen twenty volumes strength, with an equal quantity of water. It is an excellent lotion, and has no irritative effect on the tissues. The dressings may be left in the sacral wound until the bowels act, but these dressings get foul in a few hours. Twelve, or even eight hours after the operation you will find the dressing offensive and affected with *Bacillus coli*. A sufficient aperient is given on the fourth or fifth day, and that is assisted by an injection of olive oil by the rectum. It is well to have handy some iodine water, a drachm to the pint in strength, into which the foul dressings are put, and if the wound should be well irrigated with saline whilst the dressing is taken out, it comes out without any great discomfort. Irrigate the cavity afterwards, and syringe with peroxide of hydrogen. Then very lightly re-

pack for the purposes of drainage. It is surprising how rapidly the cavity closes up, and after the dead tissue has come away the wound granulates very rapidly. You must not be worried because, in the early days, when you look at the sacral wound, it seems to be of a grey, ashy appearance, as if it were all going to slough. This is only superficial. When you are dressing the wound avoid digging your fingers or thumb into the buttock to lift the patient's thighs up, because the buttock is very sensitive. But get a *thorough* view of the cavity; do not be content with *thinking* it is all right—a sin too often committed in rectal work. I lay particular emphasis on irrigation because that is the key-note of the after-treatment in rectal surgery. On the fifth day when possible get the patient up and put him into a boracic bath, in addition to the irrigation which will have been carried out, and keep him in the bath fifteen to twenty minutes twice a day—that is to say, morning and night. That is the very best "tip" I can give you in the after-treatment of these rectal cases. It is of the greatest comfort to the patient and cleans the wounds in the most marvellous way.

I now want to say a few words about radium, because there is a good deal of talk about it just now in regard to the treatment of cancer. Let me briefly tell you my own experience with radium, and from that you will draw your own conclusions. The first case I treated with radium was that of a young officer who was sent to me with a large tumour just above the prostate; it was as big as a small-sized orange, and was fixed. He had no symptoms from it, but it was found by the physician who saw him, and who made a point of examining the rectum, and having found the tumour he sent the patient on to me. I took a grave view of it; it was hard and fixed, and seemed, clinically, an inoperable malignant tumour. I examined it with another surgeon under an anæsthetic, and the bladder was cystoscoped, and the same conclusion was arrived at. We thought it would be bad to let him go without further aid, so Dr. Finzi very kindly treated him with radium, and the result was that the man got absolutely well; the tumour disappearing entirely after three months. It was interesting to feel how, three weeks after the first application of the radium, it had altered in shape and shrank up and gradually vanished. Two interesting sequelæ occurred. After the first application of the radium he had double facial paralysis—I do not know whether it was *post hoc* or *propter hoc*, or whether it was a coincidence. He also had a considerable amount of sugar in his urine, but this disappeared, as did the facial paralysis. This man has returned to duty and gone abroad. There was no microscopical examination possible, and probably this was a chronic inflammatory swelling which clinically appeared malignant. I suggest this because since then I have had eight cases of cancer of the rectum in all stages treated with radium, and three or four breast cases, and I regret to say that not any single case has it done one bit of good. I had a typical case for which one would have thought it should have done good, and that was the case of a woman whom I operated upon for carcinoma of the rectum, and who kept well eighteen months. At the end of that time, however, she came back with a small nodule just at the anal margin. If there was any case where radium could do good it surely was this, because the radium could be applied directly on the recurrence. This was done, and the radium was left for fifteen hours. It is uncomfortable to the patient, but morphia is given hypodermically, which makes it bearable. A pit appeared in the growth after the application where the radium had rested, and here there was an obvious diminution in size, but the circumference had increased, and when I saw it the second or third time after the radium had been applied it was a large and inoperable growth.

I had another case in a young woman who had a growth which I could not remove. She had a colotomy done, and there again the radium could be placed right on the growth, but it had no effect at all. So the direct application in these cases is not, in my experience, very hopeful. But I think radium *has* some material effect in after-treatment in rectal carcinoma if the growth is removed, which may be infiltrating the walls slightly beyond the bowel—and you must remember that the outside part of the growth is the active portion—and then after about ten days or so the radium is inserted into the bowel and fixed where the growth *was*. This I did in a young woman from whom I removed a growth, and later put radium in the region from which the growth had been removed. She was a comparatively young woman, and I

should say that in the ordinary course the growth would have returned soon in the peri-rectal tissues. It is over two years since the operation and there is no indication yet of any return, and I hear that the patient is very well. The growth was situated, not in the supra-peritoneal, but in the intra-peritoneal part of the bowel, where it is more likely to invade the surrounding tissues. I believe it is in this way that radium is of use rather than in applying it directly on a growth.

When a case is inoperable with regard to the cancer the best thing to be done is a colostomy. And let me suggest to you at once that colostomy is an operation which will give patients far more benefit than most of you know of, unless you have had one or two cases where you have seen the results. I say that because it used to be stated that colostomy does no good. But that is not the case. The operation has been so often described that I shall not bother you with it; it is a procedure which anybody may be called upon to do. In my own cases I pull the bowel out and open it at once, tying in a Paul's tube. There is no danger in doing this, and it prevents a great deal of discomfort from distension, etc.

After-treatment.—An aperient is given on the fourth day, at which time the Paul's tube will have begun to separate from the bowel; in any case the tube can quite safely be removed by this time. The suture tying the mesentery and skin together should not be removed until the cæcum has thoroughly emptied itself and the bowel is quiet. If the aperient has not been sufficient it must be followed by an enema. If the suture is removed before this emptying has occurred there is a possibility that the bowel might fall back into the abdomen, owing to straining having caused separation of the newly-formed adhesions which occur between the parietal and visceral peritoneum and which help to make the permanent spur.

There is sometimes a considerable amount of œdema present in the exposed bowel, and this is best treated by boracic fomentations for a few days; in fact it may be said that this is a good procedure in all cases of colostomy after the bowels have acted. After about forty-eight to sixty hours of fomentation the bowel should be surrounded with dry, plain, sterilized gauze, any stitches which are left being removed. If there is any pus at either end of the wound this is best treated with peroxide of hydrogen. On no account should iodoform be used or an irritating dressing of any sort; it does no good to the wound, and too often causes considerable dermatitis.

For thoroughly emptying the large bowel and so enabling the patient to get about in comfort and cleanliness for the rest of the day the following routine method must be carried out:

For emptying the colon.—A long rectal tube is passed along the upper opening of the colon towards the cæcum, and the outer end is attached to a Higginson's syringe. Between one and two pints of warm water are slowly injected into the bowel, the edges of the colostomy being held together to prevent any back-flow from the bowel. This enema is kept in the colon as long as the patient can comfortably bear it (generally about twenty minutes).

The patient is then turned over (face downwards) on to a "slipper," and the water is returned with all the contents of the colon, and firm pressure over the cæcum ensures the complete emptying of the whole of the colon. This should be done with the morning dressing, and the patient is then quite comfortable and clean for the rest of the day.

This simple procedure, if done at a regular time each day, and especially if proper regard is paid to diet, will ensure the patient's comfort provided that there is no intestinal irritation or diarrhœa.

It will be obvious to the practical mind that this is far more effective and reliable for keeping the patient clean than any complicated operation for the production of a spurious sphincter.

I could give many instances of cases of colotomy in which this method is carried out daily. And I am emphatic in saying colostomy is a great boon to these people, because not only do you get the bowel more freely emptied than in the ordinary way, but you can largely prevent any septic absorption from the growth by irrigation with suitable fluids from the lower opening down through the rectum.

This is a big subject, and I have only dwelt on those points which are, perhaps, new to some, and they are essentially practical.

January 23rd, 1911.

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ON DILATATION OF THE HEART AND HEART-STRAIN.

A Discussion before the Medical Society of London.

BY

Dr. F. J. GOODHART.
Sir RICHARD DOUGLAS POWELL.
Sir LAUDER BRUNTON, F.R.S.
Dr. HENRY DAVY.
Dr. W. COLLIER.
Dr. SAMUEL WEST.
Dr. WALTER BROADBENT.
Dr. ALEXANDER MORISON.
Sir JOHN BROADBENT.
Dr. CHARLES CHAPMAN.
Dr. LEONARD GUTHRIE.
Dr. PARKES WEBER.

Mr. CHARTERS SYMONDS in the Chair.

Dr. F. J. GOODHART: In opening this discussion to-night will you allow me first to say in justice to myself that I am not here at my own instance, but by the kindly pressure exercised by you, Sir, upon an old and unwilling friend. I say this because it might otherwise be thought, appearing as one who is not a Fellow of this Society, that in what follows I have something striking or new to communicate. This is not the case; I am only going to say to-night what most of us here know well. Nevertheless, I do not think it waste of your time to ask you to discuss, and if you can, to define, what limitations to attach to such terms of menace as "strained heart" and "dilatation of the heart." The question is not an easy one to handle, because, notwithstanding what I shall subsequently have to say, I think it must be admitted that dilatation is sometimes present without giving positive signs of being so.

Now, as I wish to make this discussion entirely clinical, if I may so speak when we are not at the bedside, I propose to confine myself to my own point of view, leaving it to others to improve the picture by amplification, justification, or otherwise.

Now strained hearts are constantly in evidence. I surely do not exaggerate in speaking thus. With what body do they come? They have usually been where some muscular exertion has been

undertaken by male or female, mostly young, and often of apparently a very insignificant character. The symptoms appear to be unnatural sensations of illness—perhaps nausea, perhaps fainting, the breath sighing, irregular action of the heart, collapse, sensation of impending death, perhaps, in some few cases. Then there come pains of various degrees and kinds over the region of the heart, and other sensations of general decrepitude, such as extreme lassitude on the smallest exertion. This, I think, is one of the most common of all symptoms. The heart is then examined, and the impulse is found a little outside the normal, the beat wants in decision and varies in force, not infrequently it intermits. There may be no murmurs of any kind, or if there be any, there is a systolic bruit at the base, usually over the pulmonary valves, or, a systolic mitral murmur—murmur towards the apex I should, perhaps, rather have said—which alters considerably in area and intensity as the subject changes from the erect to the recumbent posture, when it is likely to spread all over the region of a line drawn from the impulse to the pulmonary valves. I think this is a fair summary of the symptoms upon which the heart is said to be *strained*, and for which rest of a prolonged and more or less rigid kind is ordered. It hardly seems necessary to give any actual cases, but I will shortly outline one or two, one that occurred to me some years ago; the others I take at random from my notes of the last few weeks.

A boy, æt. 18 years, at one of our public schools. Two and a half years before I saw him he had taken part in a steeplechase and sports, and came home with a big heart, a systolic murmur, and a questionable friction-sound. He was kept in bed and got well. He was kept a little quiet afterwards, but he still had pain in the heart region if he ran much, and also under such circumstances would feel faint.

His nerves were upset; he ought to have been in the eleven, but he got too nervous. He was a fine, healthy fellow with phosphatic urine and a trace of albumen, but otherwise without discoverable fault.

Eighteen months later he came to know if he might row at Cambridge. His medical adviser there had said he might, but Dr. Bowditch Hale, his doctor at home, who knew him better, had said some time before that he might not. He was a most splendid fellow in physique, and I really could not say he must not, but I said, take it easy. Well, he did row, and went to pieces, and a localised systolic murmur was heard. A consultation was held, and he was ordered to do nothing for a long time—my note says for three years—and I understood that this was because of the condi- of his heart. Some four years later I saw this young man again. He had been overworked at a mission, and had suffered from dyspnœa and pain in the cardiac region and faintness. Llangammarch being near his home, he was given there a modified Nauheim course. When I examined him afterwards his heart appeared to me to be perfectly sound.

There is one other point in the case that seems to me of great importance: that some few years before I had seen this patient's father suddenly taken with ptomaine poisoning and what was supposed to be a most alarming cardiac failure. He had been tremendously overworked and was seized suddenly with a bad faint, but I had no doubt in my own mind that the nature of the attack was essentially nervous and not cardiac.

Another case is that of a healthy man, certainly coming of gouty, and probably of nervous stock, who some years ago suffered from nervous dyspepsia. He is a man of intellectual pursuits, working anxiously, and giving little heed to his physical development. He takes a holiday and commences it with a bicycle ride of excessive length, and then not long after, impelled, I suppose, by the invigorating air of the Swiss mountains, he overwalked himself. Without apparently feeling much amiss he suddenly faints, and after a few days he has another faint, and he subsequently comes under notice, with some pain about one shoulder perhaps suggestive of angina, but more probably of gouty character, a systolic murmur at the base of the heart, and a feeling of the necessity of deep inspiration which comes on especially at night, which made him feel, he said, that if he did go to sleep he would never wake up again. But what was the more noticeable to his friends was an excessive decrepitude in walking, and the impression that he gave of being so very weak and really ill.

The case comes under the category of strained heart. So it was termed by his mother, but there was no increase of dulness, a little only of varying force in the several beats, such as I at any rate

should think of no importance at all, an occasional systolic murmur, and I had no doubt that the heart was sound.

Of dilatation of the heart it may perhaps be said that there are two sorts of cases met with, or, indeed, three, like the three Johns of 'The Autocrat of the Breakfast Table.' But of one of them (the real John) I do not propose to speak otherwise than incidentally to-night. Of the first class there are two briefly stated. Both are women; both are very stout; both are nervous. The one of about middle age lost her husband a year ago, and has felt "mouldy" ever since, and not unnaturally wants to be better. She has been told that she has a dilated heart, but that there is no organic disease. Her symptoms of this are entirely general. She is generally tired. She is so stout that it is impossible to obtain any reliable information as regards her heart by percussion, but the sounds are perfectly natural; they are not unduly conducted into the axilla; the action is quite regular, and the pulse tension is under rather than over standard, 110–120 R. R.

The second case is a woman, æt. 39 years, who has a great deal of out-of-healthness in her life. Her nerves went all to pieces after influenza, and again after a course of thyroid extract to relieve her extreme obesity. Some years ago she went to Nauheim for baths, but they were too much for her, and she only had a few. She came for degeneration of her heart and vessels and to know if it were worth her while to undergo another Nauheim course. That she is an unsafe life is clear, and an unhappy one also, for she is always in misery, but so far as her heart was concerned to my ear it was perfectly sound. There was no murmur, no palpable impulse; although I examined her in every possible position the heart's action was quite regular and the pulse-tension only 120–130.

Of the other class I must add a little more, for although much the same forerunning conditions are at work, added thereto are illnesses of many trivial sorts, more especially ephemeral fevers in young people, but all sorts of temporary disturbances, leading to feelings of general malaise, which in sensitive people lead them to consult a doctor. When so doing the heart's rhythm is found to be disturbed in some way or another, the impulse of the heart is found to be outside its ideal limit; quite possibly a murmur is heard such as I have already shortly described, and according to some there is an increase of the præcordial dulness elicited by what is called auscultatory percussion.

For all these cases rest and generally prolonged rest is ordered, combined with one form or other of treatment directed to remedying the dilatation, and in my experience by far the most important gauge of improvement or recovery is judged to be the movement inwards of the visible or palpable impulse of the heart, which is anxiously looked for day by day, as it provokingly varies—now in, now out.

Now what do I suppose to have been the condition of these hearts, or, at any rate, of some of them? When I try to picture them I see first of all a presumably healthy heart, strong for a life of sixty or seventy years of wear and tear. Will it dilate under the conditions of temporary stress that have been laid upon it? Can we allow that it frequently does? Take the case of the young man after a well-contested race. His congestion is suggestive enough, it may be admitted. But what is the actual state of the organ? The man is played out, and his nerve current has for the moment ceased to run. The heart is, therefore, as all his other viscera, running on its own, the engine is shut off, and if, then, it could be, or were, forced at once to fight its battle over again it might certainly become irreparably damaged. But is the muscle there and then over-stretched, that is, dilated? It cannot be. That absolute state of powerlessness in life is the sign of its health, and we know that in a few hours, a few days, it will be working as well as ever. In some few there may remain symptoms of doubtful meaning, of which I must speak immediately.

But one of my cases said, "My heart is dilated, but not diseased." Is this a legitimate use of words? Are there not many cases to-day that are called "dilatation" with but an imperfect appreciation of what that term consists, and are not numberless cases being treated for their heart rather than for some trivial disturbance of its action, which *quâ* heart is of no importance at all?

At any rate the fact remains that these cases are treated as if they were cases of disease, and the diagnosis is essentially based upon the increase of the præcordial dulness and a displacement of the impact of the apex upon the surface wall of the

chest, associated with certain subjective symptoms, and as a matter of experience I have repeatedly found that the mere position of the impulse is held to be sufficient to determine whether there be dilatation or not.

When the history of medicine for our period comes to be written I incline to think that the Recording Angel will have some criticisms to make upon the teaching of those who, upon the strength of what is a most unreliable method of examination, have insisted so much upon an as-it-was and as-it-is demonstration of the size of the heart. Untrustworthy as I think it, it seems, nevertheless, to have captured the lay mind to such an extent as to have compelled its reflection in medical practice, and that many have grown hyper-sensitive to the discovery of dilatation, and diagnose its existence upon quite inadequate grounds.

In saying this I believe I but express the opinion of most of those who are entitled to speak authoritatively, but of this we shall, I hope, hear more presently. I am here to-night only to say what *I* think, and I shall rely upon certain common observations from which it hardly seems possible that anyone can dissent.

And, first, what do we know of conditions bearing upon it that are practically within the range of health? Well! Take, in the first place, any ordinary day of the examination of patients in the out-patient room. One after another the nervous man or woman will come before you, and you find the heart's action rapid and thumping, occasionally intermitting, and the transmitted impulse carried far beyond the nipple. Take such cases as they stand, and there is a complete picture of dilatation of the heart as it is often diagnosed at the present time. I do not dwell upon increased dulness, you may notice, because I hold that except by radiography it is impossible to obtain any reliable evidence of the real size of the heart and its cavities when it comes to attempting to estimate variations, —oscillations they might not incorrectly be called —of relatively small size taking place within short intervals of time. I don't believe even radiography can do it when you come to consider the rapidly changing conditions of the environment of the heart, viz. the lungs and the thoracic walls and diaphragm. Carry your minds back to the earlier days of your career and the difficulties that you then experienced in deciding between health and disease. I count myself fortunate that years ago it fell to my lot on one day in every year to examine one hundred or so of young men, who were required to be healthy, to enable them to enter a certain college to be trained as teachers. I learned then that many a heart of which I was at first doubtful was a good, sound organ, and I do not remember that in the many years that have elapsed since I ever had a heart case returned upon our hands. Yet I am quite sure of this, that many of those cases then would well represent the cases that are referred to me from other sources as cases of dilatation now. And you all know very well that these out-patient aberrations are purely physiological and temporary. I admit, of course, that the personal equation constitutes a real difficulty, and although I am often quite sure in my own mind, over and over again have I said that I wish—and it would not be difficult to do—some one would devise a simple ovum and attachable stethoscope by which to examine the heart at a distance as the patient sleeps. I am ready to admit that there is many a man, woman, or child in whom it is impossible to be quite sure whether there be disease or not where the point could be quite readily determined when the patient was asleep. Whereas now one needs to act upon a probability as gauged by the examiner's past experience.

There seems to be an idea abroad that the impulse in relation to the chest-wall is a constant one, and no doubt in general terms it is a fairly constant anatomical landmark, all things considered.

But look at the several conditions that tend to alter it when we come to try to precisely fix its limit and to judge of its value as an evidence of dilatation. We are dealing with an organ suspended between the lungs and resting upon the diaphragm. Surely it can hardly do otherwise than vary in position constantly, with the varying substance, distension and position of these surroundings.

I would not venture to intrude such ordinary details upon your attention were it not that seldom a week passes where they must have been overlooked or ignored.

Again, we are dealing with a muscle, and therefore the position of the impulse upon the surface will vary with the tone and precision of the muscular contraction. The ventricular muscle is described as twisting up into a tight spiral, and we think of it in general terms quite correctly as doing so.

But does this constitute the complete history of any living muscle? Examine your own heart after dinner and in bed at night when lying on your left side, and I shall be surprised if it does not tell many of you quite a different tale. I cannot imagine any living fibre acting hour in and hour out through days and weeks and years with any rigid mechanical machine-like precision such as this. Could it last if it did? Should we not all be dead, even in our infancy? A very considerable latitude must be allowed to contraction and contraction in the healthy physiological play of any muscle. A general result is obtained truly, but not always with a vigour of equal mathematical value. Fibres will rest; fibres will work half time; fibres will lag and work out of harmony; and unless this vitality, so to speak, be allowed for, I cannot imagine how all the varieties of sound and murmur that we hear are to be explained. How is one to explain, for instance, some of those very common murmurs at the apex that are produced in a moment on suddenly shifting the position of the body? Muscular distension or relaxation has a mean, but healthy excursion on either side must by no means be neglected, and, in a series of cavities linked together thus, the shape of the organ will vary somewhat in accord, and beat by beat. This is another reason why percussion can be no adequate or reliable gauge of what I will venture to call the physiological play of the healthy heart. Of the existence of such play there can surely be no doubt. To what extent this variation reaches I don't think that at present we have any accurate knowledge. It is easy to see that information is very hard to obtain. I do not myself think that the extreme, or at any rate considerable alteration of size supposed to be revealed by auscultatory percussion, and by it alone, has any real existence, but if it had, it would be less surprising to me to explain it on some such hypothesis as that I have suggested, than to allow that a distension that was reduced so immediately as is maintained to be the case after a single bath could rightly deserve so serious a term as "dilatation of the left ventricle."

Again, I constantly find that the *shape of the chest* must have been ignored in judging of the value of the position of the impulse, and a heart is said to be enlarged or dilated solely because the impulse is then further out than it should be; very likely, too, in such a chest the sounds are of a turbulent quality. But what else would one expect when the chest is so obviously narrow and contracted? In such a conformation—and, of course, you can see such by the score any day you like to look for them—the natural expectation would be a diffused beat of the heart, on the surface, a loudness and impurity, maybe a murmur, of sound, and these signs are in consequence of doubtful value until all other possible explanations have been eliminated.

When one comes to speak of murmurs as evidence of dilatation I am then, perhaps, upon a more debatable ground, for there are many cases in which anyone must own to considerable doubt. Starting from the fact, long well recognised, that there are many cardiac murmurs that are not valvular, and are of little importance, I myself would go further, and say that many murmurs that seem to be regarded as evidence of valvular, or perhaps, still worse, of muscular disease, are, from the absence of symptoms of ill-health, of some other source of production not to be explained with certainty, but are functional, if we may use that term to exclude organic changes in the valves or muscle. I say this in face of the admitted fact that many of such murmurs are of great persistency. It is usually supposed that functional murmurs indicate some deterioration of blood or tissue, the which, as soon as it is removed or repaired by tonic or rest, will disappear. This has not been my usual experience, however. I should say some follow the rule, but many will continue for long, sometimes for many years. I have been too often disappointed in this way not to be certain of what I say. Yet will there be no indications of heart failure. These things are common in boys at school, and in young men who will take as much exertion as others, and who have no distress or knowledge of any incompetence in their daily undertakings. I have known a large number of such cases first discovered when examined for life insurance, and refused or postponed in consequence. Many of them are in nervous subjects, who suffer from "the heart in the mouth" of the embryo orator, or cricketer, or performer in public of any kind, and again I have come to the conclusion that a large number of these murmurs are the result of the natural physiological play that I have alluded to. Long years ago we were shown and taught—Oh! Physiologist, think leniently of me

when in my mellow years I call up the shade of Wilkinson King—that on the right side of the heart there is a safety-valve action, that allows of regurgitation, and by so allowing it fulfils a necessary function. Surely, also, under appropriate conditions regurgitation is to be expected, and looked for upon the left side of the heart, for it seems to me unreasonable to suppose, as I have already said, a nervous muscular organ with a throbbing vitality such as the heart possesses to be always alert. And to this purely physiological up and down—I will not call it either failure or imperfection—may be attributed many of the muscular rumble murmurs; many of the systolic souffles, many of the purely postural murmurs. Many of these last must, I think, be true mitral leaks, although not due to disease; one is entitled quite consistently thus to speak of *valvular* leakage without disease.

I willingly admit, however, that in many cases of this sort it is quite permissible to remain in doubt, all I maintain being that it is very necessary to keep the various alternatives in mind, and that one must not jump to dilatation without very careful consideration. Take a very common case. A healthy child or young adult, possibly one who might be called nervous, is suddenly taken with a sharp febrile attack, owning no obvious cause, and of a few days' duration. In the course of it the heart's action is rapid, and both at the apex and base there is a systolic murmur to be heard that is not known to have existed before, although this is all uncertainty because the patient has not been ill before. The murmur has certain features about it already shortly alluded to.

The febrile storm expends itself; the patient is well; but the abnormal sounds remain much as before, probably somewhat mitigated. Has the case been one of endocarditis or myocarditis, or not? How often have I not been asked this question? I believe *not*! It seems to me more probable that the febrile storm has spent itself upon the innervation centres of the heart, and by so doing has altered, but, maybe, not materially so, the rhythm of some of the muscular bundles. The condition may, perhaps, not inaptly be compared to the strabismus of infancy brought on by a convulsion, which remains permanent afterwards, and where no doubt some equipoise has been upset which is not recovered.

Now these are cases of the very commonest, and over and over again I have known them to entail long periods of rest in bed, and carrying chairs, and all the paraphernalia of serious muscular failure, because the impulse has been displaced, the murmur has persisted, or the pulse remained unduly quick—all signs which, when weighed in the balances, are found to be wanting in the peculiar significance that is attributed to them, unless they be most closely checked by every other available means of coming to a conclusion.

One more argument may still be adduced against this too prodigal use of the term "dilatation," which is often neglected: it is that the symptoms for which men and women apply for treatment are not such as are found in cases where the dilatation is beyond dispute.

Fainting is perhaps the very commonest of all in these cases of spurious dilatation. It is a common belief amongst the public that fainting means jeopardy because of "weakness" of the heart. There is no doubt that the same idea is pretty deeply cut into the medical mind also. Why not? It seems so reasonable. Yet there is no idea more absolutely contradicted by experience. Do people with well-marked disease of the heart associated with dilatation faint? Surely they of all cases ought to if fainting be a sign of heart disease. What does your experience teach you? I will hazard a guess that it tells you this: that fainting as a symptom of pronounced heart disease is so rare that probably few here to-night have ever seen it happen. And there is good reason why. The vascular mechanism prevents it, and a dropped beat in the pump is taken no instant account of in the capillaries or in the nutrition of the centres upon which function depends. Faints are peripheral phenomena, and due to peripheral spasm, or nerve control, and so on, and with them the pump has nothing to do. Thus, by the way, it is that an intermission of the heart-beat, if unassociated with any obvious disease, is one of the most unimportant of cardiac aberrations, and a weak pulse, made so much of too often, has but seldom any sinister meaning *quâ* heart.

Another very common complaint of these people is that they are dreadfully tired: the least fatigue is too much for them. Is this a symptom of pronounced disease of the heart? By no means!

And what of all the other signs—the sighing

respiration, the collapse, the experience of dreadful illness, and so on? Is it unwarrantable to say, as I often do as an encouragement to one who thus suffers (I don't dispute the suffering, only the cardiac origin, and the danger to life), "If you are able to appreciate so acutely your sensations you may be fairly sure your nervous centres and your heart are still much too vigorous to be anywhere near the point of death." Heart disease (within the confines of which I emphatically include dilatation of its muscular wall) produces the very opposite of this; it leads to congestive stagnations, and therefore to somnolence and mental hebetude; to that crossing of the bar of which but the one in a thousand, thank God, is able to recognise the lights when actually passing over it.

It will be said quite justly that this is all very well, but that I am here discussing advanced disease, and the use of the term that I am attempting to combat deals with the beginnings, and with the attempt to prevent the pathological state from coming into being. I am quite alive to the intention, and would by no means belittle it, but I object to dilatation without disease, because it is just there that over-anxiety is likely to step in and do harm, and where the true measure of the physiological capacity of the heart for labour needs to be correctly and calmly appraised. Errors must, of course, be made on either side. I do not doubt that any of us may under-estimate the risks in a weakling's heart. Still, on the whole one cannot but come to the conclusion that the heart-muscle has a magnificent power of resisting strain, and that dilatation is seldom, indeed, other than a *result*, and *late stage* of *disease*. Primary dilatation is surely very infrequent, except, perhaps, as senility advances, and degeneration has taken place.

Thus far I have attempted to show that these so-called dilatations of the heart are no dilatation at all. Then if not, what are they? I have already shown my hand, and you will have seen what I think of them.

They are, in my opinion, both the strained heart and spurious dilatation, mostly due to aberrant innervation, played-out, exhausted, intermitting, or otherwise misdirected in its transmission—batteries with intermitting currents, engines devoid of power.

Now someone may say at once, "Granted all you say, it does not seem to help us much." But it is obvious that it raises the vital question of the treatment of these cases and of the necessity of long periods of rest. If in all these cases the heart-muscle is sound there is no necessity for it; they want watching and care to ensure no mistake is made, but prolonged rest is the very worst thing that can be ordered them. Take the cases of heart-strain. I am glad to say Dr. Collier is here to-night, who has had much experience at Oxford, and has given, moreover, much attention to the subject; I know not what he may be going to say; for all I know he may be prepared to scatter my arguments to the oblivion of the four winds, but my own belief is that bad as young men look after hard athletics, and curious, and one would think anxious, as is the condition of the heart on coming out of them, the musculature of the organ speedily recovers itself, and within a few hours or days is as well as ever it was, and the symptoms of heart-strain that we are hearing of so constantly, and that go on month after month and year after year, natural healthy exercise being tabooed all the time, are conditions of neurasthenia, and their hearts have been sound all the time. To take an extreme case: only a short time ago a nine years' invalidism more or less was dated from a heart-strain all that while ago. I am not saying that there are not cases where, as the result of heavy strains, the heart may give out and dilate. Of course it may, and perhaps more often than I may seem to allow. And I certainly hold that athletics should be carefully selected—in boys, graded—and watchfully inured by training; but it is patent that even in the weakling the heart-muscle has long had training in this, its life's work, and when it fails grievously—and let me say again, I hold the occurrence of dilatation to be *grievous*—there will come signs of serious illness. The case of a sudden rupture of a valve is a case in point. The symptoms in such cases are generally immediate and severe, and it does not take long to find out that something desperate has come to pass. In the common class of cases of which I am speaking to-night, if the muscle were damaged by the strain, one ought to expect and require definite signs of illness. This they seldom or never show, and I have, therefore, come to the conclusion that we are far more sensitive lest the heart-muscle should fail than our experience, if we think back upon it, will justify.

One concluding word upon a group of cases which come, I think, fitly within the purview of the remarks that I have made, and where the heart becomes irregular in the declining years of life. It is a large group, and you all know the cases. The heart is tumbling about in a most erratic and apparently critical fashion, yet there is no murmur, and there are really no signs of cardiac failure. Now in these I am frequently told that the heart is dilated, or the muscle is fatty. These cases sometimes faint, they are often giddy, but there is good reason to think that the heart and brain are both giddy together as the result of some central nerve disturbance, toxic or otherwise.

Moreover, these cases will suddenly change their rhythm and become temporarily quite regular and then will again resume their old habit. I can recall two cases at the moment—one in an old man, whose irregular heart, under the influence of an anæsthetic, beat with perfect regularity. In another case, that of a very old lady, whose heart had for years taken on this irregular rhythm, she became very ill with influenza, and one naturally augured the worst with a heart of that kind, whereas the action of the muscle became regular and remained so all through the attack, and resumed its natural irregularity after she got better. One can see a similar sort of thing in certain cases of tachycardia, where the rapid action will cease in a moment, and be replaced by a period of an exactly opposite type, where the heart becomes unduly slow. Often have I wondered, as you must have done, how the circulation has been carried on under such circumstances. There is no doubt that there may be no breathlessness, cough, œdema, congestions; none of the signs of cardiac failure. The suggestion such a condition gives to me is that the heart muscle must be of excellent quality thus to carry on its duties so completely in this higgledy-piggledy fashion, but also that it must be greatly assisted by the arterial circulation, which must call out all its reserve power in steadying the blood-stream.

Thus questioning, I fancy that there may be a still deeper root to the matter, and that these cases are some of them indicative of a nervous centre's growing old, and that the faintness, giddiness and irregularity of the heart may be not cause and effect one way or the other, but special sense centres and heart both giddy together as the result of some deep-seated central degeneration or nerve-storm.

Many a time have I seen this condition come on in men and women who have lived a hard life of nervous stress. Often, too, I have seen it associated with the vertigo and loss of consciousness that has sometimes been called senile epilepsy. I have several times seen it come and go with an attack of gout and I therefore incline to link it with some cases of Stokes-Adams' disease, which in like manner I have seen come and go apparently in obedience to a similar toxic state, or in obedience to the dictates of an epileptic storm.

But if this malady be not one of degenerate muscle there seems little reason for insisting upon absolute rest, and it is my conviction that a case of this kind will often show better results if allowed to take moderate exercise, and live a life in conformity with his or her time of life.

Looking the matter all round, then, my conclusion is that in disease in which the heart shares in the vicious process there may be too much tendency to consider it from the local standpoint, when we should do better to think less of the heart and more of the driving power. From this point of view I would put in a good word in properly selected cases for the Nauheim bath and exercises. And if you like to have it so my selected cases would be the neurasthenic heart. But in doing so I should like to drop the heart entirely into the background, and ignore the premises upon which, I think, unfortunately, it has been especially recommended to our notice.

Sir RICHARD DOUGLAS POWELL said he had listened with great care and attention to Dr. Goodhart's paper. Dr. Goodhart he had always regarded as a physician with a particularly robust view of medicine. He gathered that in Dr. Goodhart's opinion a large number of the cases of dilated heart were present mainly in the imagination of the observer; in this he (Sir Richard) was inclined to agree. There were several kinds of heart-strain. There was first the strain of continued high blood-pressure upon the normal heart, which in time would overtax and dilate it. Then there was the strain of normal blood-pressure upon the weak and anæmic heart, under which sometimes the heart yielded. There was also the more recognised strain of excessive over-effort upon a normal

heart, overtaxing and dilating it. He thought the great point after all was in the diagnosis. The people who consulted them were ill from some cause, and they came to the physician for a diagnosis. Many of the cases of what the reader referred to he would call functional relaxation of the heart associated with neurasthenic symptoms, and they were highly analogous to those cases of displaced uterus and dropped kidney, etc., the symptoms of which were generally of a neurasthenic kind. The late Dr. Matthews Duncan used to have such cases brought before him in connection with uterine affections, and he would dismiss many of them with the observation that they had nothing the matter with them, and the patients went away more or less comforted and tried to lead their normal lives. A friend of his and a late President of this Society told him the story of an American lady who consulted him. He examined her with his usual care and thoroughness from head to foot, investigating every organ, and finally said, "I am very pleased to say, madam, that I find you to suffer from no organic disease whatever." "Well, then, doctor," she replied, "all I can say is I guess I don't function well." He (Sir Richard) thought the lady had the better of the physician on that occasion. The various forms of so-called "heart-strain" overlapped one another somewhat. It was his experience, for instance, that many of those cases in which illness was brought about by excessive athletic exercises, and which sometimes were attributed—and he thought to a great extent rightly—to the heart, were cases in which there was a very great amount of anæmia present. He had observed that people who had overtaxed their muscular sysytem over a long period were very apt to become profoundly anæmic, and he thought there was probably good cause for the anæmia in a certain amount of auto-toxic poisoning produced by the rapid waste and change in the muscular system affecting the quality of the blood. The diagnosis in many cases of relaxed or dilated heart was extremely difficult, and yet it was extremely important, because very grave mistakes were made on both sides in border-line cases. Some cases of importance had been recorded as having nothing the matter with them; other cases had been regarded as having disease when they had none. Many of those present must have known, if they had not seen, cases which had been referred from the public services on account of "diseased heart"; a bruit and some signs of dilatation had been found, yet when subsequently examined with infinite care one failed to discover any sign of organic disease of the heart whatever. Extended impulse and even a cardiac bruit under greatly excited action were not incompatible with a perfectly healthy organ. Cases were undoubtedly often referred or had their premiums increased in insurance offices on the same grounds. He was aware, many years ago, when he was physician to the Evelina Hospital and Dr. Goodhart belonged to the staff, that not a few children between the ages of seven and twelve presented relatively large hearts with the apex-beat outside the normal position, and yet they had no disease of the heart, and as they developed in general bodily vigour their hearts became perfectly normal. The heart in such cases was relatively large in proportion to the development of the respiratory organs. In making a diagnosis of dilatation of the heart he agreed with Dr. Goodhart that he would not be satisfied by the evidence from percussing from the left to the right, making out the margin of dulness; one must look at the case from several standpoints. But with care and perseverance, by auscultation, percussion, and palpation, one could satisfy oneself whether the dimensions of the heart were increased or not, and especially so if allowances were made for any excitement of the heart's action, which invariably caused the organ to pulsate over a larger area than in quietude. In estimating the value of any evidence of slighter degrees of enlargement, one must consider all the collateral circumstances and recognise the heart as part of an extensive vascular system, to be taken in relation with all the other factors. Probably he would be told that seeing was believing, and that there was a very easy criterion whether a heart was enlarged beyond its normal dimensions or not, namely, by taking an X-ray photograph. He had the utmost respect for the importance of X-ray photography in clinical medicine and its especial value in certain conditions of heart and aortic disease, but to estimate the minor shades of difference in cardiac dilatation he could not think that it was of much value, and certainly not in making comparative measurements from day to day, for the slightest variation in the plane of shadow would make an enormous difference. Cases of cardiac trouble of the kind referred to by Dr. Goodhart were sometimes of considerable

importance from a medico-legal point of view. Of three cases which had come before him in that respect in the last eighteen months, two were in railway-men, and he was consulted about them in connection with the Workmen's Compensation Act. He would not dwell upon those cases; there was no doubt about the existence of an enlarged heart, probably from long-continued heavy work, and in neither of them could the diagnosis of acute strain be sustained. The third case was that of a middle-aged man who had suffered attacks of pseudo- or vaso-motor angina for twelve months, following upon a history of acute gastro-intestinal illness from water poisoning. The attacks consisted of præcordial pain coming on when he exerted himself or by walking to his business. The pain was dull in character and ingravescent, obliging him to stop for a minute or two until it passed off, and then he could resume his walking. The man exhibited very few cardiac signs, but there was a slight displacement of the margin of the heart three quarters of an inch to the left, and he undoubtedly was a dyspeptic man. He (Sir Richard) took it that the man suffered from a form of pseudo-angina, probably connected with dyspeptic disturbance affecting the heart, the texture of which had in some way been slightly damaged. He mentioned the case more particularly in association with some of the cases which he thought Dr. Goodhart alluded to, in which slight damage to cardiac muscle had resulted from the action of microbic toxines, such as the slight forms of cardiac change which resulted sometimes from enteric fever, from influenza very commonly, and occasionally from ptomaine poisoning and malaria; slight cardiac alterations attended with very decided cardiac symptoms. The pathology of the condition he would find it very difficult to explain, and he was not aware that it had ever been definitely explained, and such patients did not, as a rule, die. In a few cases of extreme anæmia that had been examined post-mortem the cardiac muscle had been found to be decidedly fatty. In cases of toxæmia the hearts had been found to present what was called hyaline change or cloudy swelling, sometimes associated with granules which disappeared on the application of acetic acid and sometimes granules of the fatty kind. It was ascertained by some experiments of Dr. Pearce, of New York, a few years ago, that the increase of blood-pressure caused by subcutaneous or intravenous injection of adrenalin would produce definite and distinct changes in the cardiac muscle, either of the hyaline variety or positive fatty change in the muscular fibres. With regard to the treatment of the conditions of heart he had been alluding to, he thought the essential point was to make a diagnosis. If the physician could be sure himself and could convince the patient that he had nothing organically the matter with his heart at all, that his discomforts arose from some functional disturbance of a reflected kind which could be remedied by a little care and appropriate treatment, perfect recovery often rapidly took place. In other cases, where strain had produced some definite enlargement of the heart and disordered function, and where that strain was of the nature of increased blood-pressure which could be accurately ascertained and measured, it was necessary to overhaul the man's life and find out what it was due to. Usually by regulating the diet and attention to excretory functions the undue blood-pressure could be lightened. It was of great importance in these cases to inquire into the patient's daily life, average doings, and methods of work, and often by cutting off extraneous duties which these energetic people as a rule went in for, one could do a great deal to restore them. In other cases, in which there was a definite dilatation of the heart, one had to enjoin absolute rest for a time, followed with relative rest for a further time. But there were border-line cases, in which he agreed with Dr. Goodhart the neurasthenic element was decided, and in which modified rest was better than absolute rest. It was desirable not to encourage those people to "nurse their sorrow," but to direct them to take three or four hours' rest a day, preferably before and after meals, and allowing them in the interim to follow their household duties or their normal interests. It was to be borne in mind that in this class of patient the neurasthenic element was largely concerned; such people became "pressed" and agitated in any work they did, and might often accomplish all that it was necessary for them to do in a better regulated and more leisurely manner. By regulating rest and work much more could often be done for them than by more stringent rest-cures. The only stipulation upon which he would lay emphasis was that the rest should be carried out under such circumstances that they were breathing the fresh air with properly warm coverings. One

other important point in these cases was the need of stimulating the heart's function through the respiration. A valuable aid in these cases was to insist upon their taking from time to time, several times a day, a succession of slow, deep inspirations and expirations, contracting the limb muscles during inspiration and slackening them during expiration, in this way hastening the venous return to the heart and stimulating the coronary circulation of good aërated blood. The exercises were best taken whilst quietly walking in the fresh air. This was an important principle in the so-called Nauheim treatment, and a sojourn at Nauheim or Kissingen was, at the proper season, of value in these neurasthenic heart cases. In other cases where the blood-pressure ranged high the additional effervescent bath treatment was also valuable. The remarks he had made were, he feared, very fragmentary; he had only touched upon a few of the points raised in Dr. Goodhart's interesting paper.

Sir LAUDER BRUNTON, F.R.S., said he was sure that all had listened with very great pleasure and interest to Dr. Goodhart. He would begin by speaking of the last part of the paper because with that he agreed. In elderly people the disturbance of the heart was not usually due so much to change in muscular structure as to a nervous change. And the tachycardia and other symptoms of nervous disturbance were often better treated by attending to the digestion than to the heart. A patient of his treated himself; he suffered from spasmodic tachycardia, which he found he could cut short at once by an emetic, and after he found that out he did not trouble the doctors much more. With regard to the dilated heart occurring in young people, he held rather strong views. It was true that imagination was rather apt to bias one's judgment. He, as a rule, depended in making his diagnosis of dilated heart or cardiac strain in young people partly upon the symptoms so well enumerated by Dr. Goodhart, but chiefly upon a combination of these with an estimation of the size of the heart as ascertained by percussion. He believed he used a different method from that usually adopted. He percussed very gently indeed, and his reason for that was as follows: Weber's law was that in order to differentiate between two sensory impressions the difference between the two requisite for perception must be greater according to the strength of the impression. Anyone could tell the difference between one ounce and two ounces, but probably no one could tell without assistance the difference between 1 lb. 1 oz. and 1 lb. only. So by percussing gently in the way described one was able to appreciate very slight differences. But even here one's imagination might run away with one. To avoid this he turned away his head or closed his eyes, and percussed once. Having ascertained blindfold the position of the cardiac dulness he marked that with a blue pencil. He did that again, and afterwards made the observation with his eyes open. If those three marks coincided he concluded he was not far out in his estimation. He did not think one obtained such good results by X rays as from simple percussion, and that, he supposed, was due to the fact that the heart was constantly on the move, and one chiefly got from X rays the size of the heart, except during systole only. The sounds of the heart might be ascertained all through the day if it were wished, as Mr. Sidney Brown had invented a stethoscope by which one could hear the heart as long as he wished and almost as far away as one liked. He had heard from his house a heart beating in Ladbroke Grove, two miles away. By fixing one of these instruments on a patient's chest the heart could be listened to for the whole twenty-four hours. The question of murmurs was of great interest, because there were some murmurs which did persist, and yet apparently were of no clinical importance. He was very much struck with that once when auscultating the heart of a friend who was specially notable for his extraordinary muscular strength and for his phenomenal endurance. He was a Canadian, and although he was a thin man, he (Sir Lauder) had seen him take up two dumb-bells, each weighing 35 lb., in one hand, putting them over his shoulder comfortably, and draw them back again. He could sit with Indians most of the night over the camp fires and yet could wear almost all his men out snow-shoeing in the day. Yet he had a remarkable systolic murmur, rather loud and harsh, which was chiefly audible at the lower end of the sternum, and was not propagated much either to left or right. The safety valve idea he regarded as true for both ventricles. Although it was usually considered only in regard to the right ventricle it was just as important in the left.

A man was suffering from gouty kidney, with enlarged heart and high tension; he thought the mitral leakage, indicated by the development of a mitral murmur, was likely to add several years to his life. With regard to cardiac strain, it seemed to him that cardiac strain was like strain anywhere else. A joint might be strained or sprained, and he did not think there was any difference of kind between these conditions, but only one of degree. He once strained his wrist, and he spoke of his own experience because he was able to estimate the sensory condition as well as the obvious condition. After straining the wrist there was nothing to be seen, no swelling or any other change, nothing was to be felt, and there was no tenderness on palpating the wrist. But when he tried to lift a portmanteau pain was felt and continued for some minutes afterwards. Otherwise he would not have known that there was anything wrong with the wrist. He once sprained his ankle, and there the condition was different; there was swelling and ecchymosis, some pain, even when he did not move, and intense pain when he attempted to do so. Similarly, in cardiac strain there might be nothing obvious on examination, and yet the patient complained of a dull ache in the chest, very often only on exertion, and when the patient was quiet neither he nor the physician could make out anything wrong with the heart. But there were other conditions of cardiac strain when the patient might feel pain even when he was not exerting himself, and when one probably, on percussing him, could make out a certain amount of increase in the cardiac dulness. Those, he believed, were of the same character, but differing in degree. For the slight strain, either of joints or heart, all that one wanted was avoidance of further strain. If one took Nature's warning and avoided pain the condition got well of itself, and gentle exercises did good. But for severe sprain or strain what was needed at first was complete rest, and after that gradual exercise. When he had patients come to him and he diagnosed cardiac strain they asked what they should do. His answer was that they had strained their hearts by doing so and so, and all they had to do was to treat their heart like a sprained ankle and to avoid doing anything which brought on pain, and let all exercise be such that it did not bring on pain. The use of the so-called Nauheim exercise did an immense deal of good in such cases, especially when combined with the baths at Nauheim. It was certain that the healthy heart had very great power of withstanding strain, just as healthy joints had, but this was greatly weakened by acute disease, and more especially by influenza. Some of the worst cases of cardiac strain he had seen had come on after influenza, and it would almost seem as if the slight cases of influenza were more dangerous than the severe ones, because if the patient was severely ill, with a high temperature, he was obliged to be in bed, and when he got up he took care of himself. But if it were a slight attack he said it was only a feverish cold, he did all his work as usual, or tried to, and brought on cardiac strain. He was much struck by the remark of a patient who had a nephew in Moscow. That nephew was not inclined to do what the Moscow people did when they had influenza, namely, to go to bed for five or six days, but he said he would show them what an Englishman could do—he would try to do his work as usual. He did, and the result of it was that he broke down and was in bed five weeks, for the heart was too much weakened to bear the strain.

Dr. Henry Davy (Exeter) said that when his friend, the President, asked him to take part in the discussion which was to be initiated by his old teacher and friend, Dr. Goodhart, he felt he could not refuse, and he was very pleased to be able to take part in such a discussion, particularly so because Dr. Goodhart had expressed very much the views which he held himself, and which he probably derived from him when Dr. Goodhart taught him. It was gratifying to him to find someone in authority protesting against these cases of so-called heart-strain, and which were diagnosed by the great authorities and leaders in London, the result of whose verdict was sometimes to sacrifice the lives of young people at school, when really their trouble was not heart-strain at all. He would illustrate that by quoting a case—and he could quote several—that of Mr. —A, a tall, lanky, badly nourished boy, æt. 17 years, who was brought to him some years ago. He had twice previously been sent to Nauheim on account of dilatation of his heart. He saw the boy because it was proposed to send him again. He found that the lad was badly fed and anæmic, and his muscles, including the heart muscle, were feeble and might yield to

undue pressure. When he ran upstairs once or twice there was a slight bruit. He recommended breathing and other physical exercises at a gymnasium and an out-of-door life, and the boy did very well indeed. He saw him last week, a healthy, married man with no trace of heart disease. The interesting point was that there was a great dispute about the boy, and on two consecutive days he (Dr. Davy) had letters from two well-known physicians who had seen him the week previously in London. The first of these gentlemen said that "the right side of the heart was relatively weaker than the left; the patient had dilated heart, and was developing adolescent rickets." The second said "he was unable to look on Dr. X—'s view as anything but grossly inaccurate, that he did not understand his attitude of mind, and he was sure it was wrong." It seemed to him (Dr. Davy) that this might be said of a very large number of cases of so-called heart-strain with equal truth. Personally he had never seen a case of dilated heart, or one which he would call dilated, in which he could not trace some poison of the muscle or some other degeneration of muscle. He was a profound disbeliever in the fact of muscular exercise being harmful. He had never seen a case of athleticism in his life, and yet he had been on the look-out for such all his life. He thought he saw one some years ago, that of a gentleman, æt. 63 years, who came under his care. He was a teetotaller and had never had syphilis or any other disease. Yet there was manifest atheroma of his arteries, a hypertrophied heart, with the apex-beat well outside the nipple line, a feeble, unsatisfactory pulse, with an occasional soft mitral systolic bruit after much exertion. He had always been athletic, and captained the cricket team at Rugby. He was an old Oxford blue in cricket and football, and was said to have suffered heart-strain at Oxford. It was a case in which it was thought over-athleticism was possible. But that was not the case. Two months afterwards his sister came under his care with the same condition as her brother, but, if anything, the atheroma and the feebleness of the heart were worse. She had never done much muscular exertion in her life, and had always led a sedentary life. And the chief point in which she agreed with the condition of her brother was in having very diseased teeth; in fact neither the dentist nor himself had ever seen teeth in a worse state of decay. It was therefore clear that her brother's condition was not altogether due to his former athletic life, and he would have something more to say about diseased teeth. The position he (Dr. Davy) would take up in the matter was that assumed by Dr. Stokes, of Dublin, in the classical book which he wrote on heart disease many years ago, and everything he had read since had not altered his opinion. Dr. Stokes said, "The occurrence of simple uncomplicated dilatation of the heart considered without reference to any muscular degeneration on the one hand, or valvular obstruction on the other, is so rare an affection that while we do not deny the possibility of its occurrence we must admit that there is very little clinical observation which will establish its diagnosis." Before we can accept it as satisfactorily proved that dilatation may occur in a healthy heart we must have cases recorded in which dilatation has occurred in a heart which was found to be healthy a short time previous to the occurrence of the dilatation, and I have been unable to find a case recorded in which this has occurred. He had seen plenty of cases where degeneration, or want of nutrition of heart muscle, had caused dilatation. One heard a good deal about patients with high tension going on to dilatation. Everybody in the room must have seen dozens of cases of chronic Bright's disease which had gone on year after year, and the arteries becoming more and more thickened, the heart more and more hypertrophied. One would see cases of atheroma going on to such an extent that all sorts of mental troubles came on because the arteries were so atheromatous, and yet from first to last the hearts did not dilate or cause much trouble. But if in such a case the nutrition of the heart was altered, that heart would often become dilated. One of the most marked cases he had seen was that of Mr. A. B—, æt. 69 years, who consulted him in 1886, and was under his close observation for twelve years. He had been told by Addison in 1858 that he had Bright's disease of the kidneys, so one must be certain he had kidney disease forty years before death from apoplexy in 1898. During the whole time he (Dr. Davy) knew that gentleman the urine was albuminous, the heart hypertrophied and the arteries thickened, but he had no trouble with his heart except on two occasions, December 3rd, 1888, and March, 1889, when he came with all the

symptoms of dilated heart; there was a bruit and increased dulness, and the apex-beat was much increased outside the nipple line, which quickly passed off after rest and appropriate treatment. The point to which he wished to call attention was that in 1888 he began to lose weight. A note in October recorded his weight as 12 st. 7 lb., and in April of the following year it had gone down to 10 st. 12 lb., showing a loss of 23 lb. in six months. Neither his doctor nor Dr. Davy discovered why he lost weight at that time. From April 1st it remained stationary until the following November, and then he began to pick up again, and he recovered his original weight in the course of a year or so. It was significant that twice during the period in which he was losing weight his heart became dilated, and he thought the nutrition of the heart was affected by the same condition as affected the general condition of the heart and body generally. It was scarcely realised in how many cases there were badly nourished atheromatous hearts which were dilated with very little exertion. He could mention a group of five cases in elderly single ladies of ages varying from 60 to 65, belonging to the middle upper class. All had very marked family histories of gout, all were teetotallers, and denied themselves every luxury to give to the poor. All except one had trouble with decayed teeth. None of them took much muscular exercise and all showed arterial degeneration. In one of the cases a post-mortem examination was made, death having supervened after a long illness during which she had permanent dilatation of the heart. The medical man who made the post-mortem examination wrote to say he found very marked atheroma in all her arterial system, and especially in the aorta and in the arteries in the splenic area. The heart was greatly dilated, the auricles being no thicker than brown paper, while the ventricles were almost double that thickness. In those badly nourished old ladies dilatation of the heart was constantly occurring. One lady came to him every Lent with dilated heart and mitral bruit caused by weakening of the heart-muscle by fasting. There were several notes about it in his case-book like the following: April 20th, 1901, "Starved again, horribly thin and weak, bruit very marked." April 24th, 1897, "Looks starved, heart bruit very marked." So that that lady every Lent pulled down her strength and general nutrition, and the heart dilated. She never walked more than a mile or two at a time in her life. He had another case of the same kind in a lady who was practically always an invalid. He knew all about her life-history as she was a distant relative of his. She used to have twenty-one stairs in the house she lived in. She was about the house for two or three weeks, and her doctor would lay her up for three or four weeks. He went down to see her, and there were symptoms of dilated heart. She rested, and after she was up the same thing would begin again. It was inconvenient for him to see her every little while, as she was forty miles away. Therefore a house was procured for her nearer, with three rooms on the ground floor, so that she never had to go upstairs. She had been in that house eight years, and had never had her heart once dilated in that time. In the cases where the heart was badly nourished almost the slightest exertion would show those people that they could not stand exercise; their hearts were in such a condition that they yielded to the pressure produced by the slightest exertion. Another point to which he wished to call attention he did not believe was sufficiently recognised as a cause of deterioration of heart-muscle. Probably the most potent causes of deterioration of heart-muscle were the various fevers which produced toxins. Sir Lauder Brunton had already spoken of it coming on after influenza. It had been known to come on either slowly or acutely after typhoid fever and after diphtheria, and he wanted to call attention to the fact that it might come on in cases where toxins were produced from decayed teeth. Everyone knew that Dr. William Hunter traced pernicious anæmia to organisms which were discovered in bad teeth, and since that discovery he (Dr. Davy) had been on the look-out for a case of pernicious anæmia from that cause. Although anæmia was of frequent occurrence in Exeter, he had not been able to find a case of pernicious anæmia due to decayed teeth, although he had observed many cases of anæmia which were associated with bad teeth, and which differed from chlorosis and ordinary anæmia in showing a recurring temperature of the remittent type and very much longer duration than would be found in ordinary chlorosis. It would also be found that whatever treatment was adopted for such cases, they did not improve at all until the diseased

stumps had been taken out. He could mention many such cases. In one such case there occurred something which, on looking back, made him regard it as very significant. One of these anæmic girls, with bad teeth and a remittent type of temperature, went under the care of one of his colleagues at the dispensary. One day she hurried upstairs and fell down dead on the top, just as one had known patients do who were the sufferers from diphtheria. There was no post-mortem examination on that case, but his colleague who saw her at the dispensary said he felt sure there was no heart disease; he had examined her several times. A year or two ago he was very much struck, as he always had been, with anæmia and dyspeptic symptoms, and loss of flesh, such as one constantly found in association with bad teeth. One case was very instructive. He was a well-developed, fairly athletic young man, who had passed through Cambridge, and entered one of the London hospitals. He came to see me with dyspepsia, loss of flesh, and profound anæmia. I noted that his heart-sounds were unsatisfactory, but I could not make out any bruit or organic disease. His teeth were in a very bad state, many were decayed, and he was in the hands of a London dentist when I saw him. I prescribed iron and strychnine in a bismuth mixture, and advised him to consult one of the physicians at his hospital. He could not get on with his work in London. He returned home in April last, and seemed in such an unsatisfactory condition with anæmia, dyspepsia, and unsatisfactory heart-sounds, that he advised him to leave work and spend the summer in the country. That he did, going to Newquay, and playing golf a good deal, living in the open air. He (Dr. Davy) received several letters from that gentleman saying he was getting on well. He did not see him again until July, when he returned from a tour in the Lake district, walking and motor cycling among the hills. He was surprised to see him looking thin, anæmic and ill. He found the apex-beat outside the nipple line, and he had an apical systolic bruit which he had not noticed before. He sent the patient to bed and called to see him next morning, and found his heart more or less normal in every way. Two days afterwards the patient walked to his (Dr. Davy's) house, half a mile, with a slight hill between, and hurried, as he was a little late for his appointment, and on examination the heart was found to be in the same condition as regards bruit and apex-beat as when he saw him in the house a few days previously. A few days after that there was no bruit. He considered that the heart muscle was in a degenerated condition, and that it yielded to extra pressure produced by the slightest muscular exertion. He thereupon advised him to go for a long voyage. He went to New Zealand in consequence of that advice, and he had heard from him that he was much better. He regarded it as a case of toxic poisoning produced by the bacilli from decayed teeth. One other case was that of a Sister at a naval hospital, where she had been for a number of years. She consulted him two months ago on account of her heart. He could not find evidence of disease of heart, lungs or kidneys, or other organs, and although anæmic, she did not seem anæmic enough for that to account for her breathlessness. She also complained of a symptom which he would like others to watch for, to find whether it was significant of anything in particular. She had attacks of great breathlessness when sitting down quietly and doing nothing. He had had several other cases of anæmic women when sitting and doing nothing who were seized with breathlessness, and he believed that it was often a sign of that toxic condition. The pulse was very weak and the heart's action feeble. She had been invalided out of the Navy because of her heart, though her health had been usually good. Still, she had always been more or less anæmic. She was in Germany in 1879, and had since had very much trouble with her teeth, which had been extracted because of decay, until now she had very few left. But she had two very bad diseased molars and several diseased teeth in her mouth. He had those extracted aseptically, and had them examined by a bacteriologist. The report was that from one molar a Gram-negative micrococcus of the catarrhal group, a streptococcus occurring in short chains, and a large Gram-positive coccus were present, all of which would be sufficient to produce toxæmia. From the end of another molar tooth which was sent to Mr. Goadby pure cultures of *Saccharomyces neoformans* were obtained, a micro-organism which Mr. Goadby said frequently produced toxæmia, and probably quite enough to account for a sufficiently coarse toxæmia to produce myocarditis. The third case was even

more significant. In September, 1908, he was asked by a medical man to look after his wife. She had some abdominal pain connected with old adhesions caused by pelvic peritonitis, and she suffered much with sick headaches. At that date he made a note that she was anæmic and her heart and lungs seemed normal. In the autumn of 1909 her heart began to trouble her. She could not lie on her left side or sit up in bed without feeling faint. She complained of palpitation and shortness of breath; on two occasions the apex-beat was outside its normal position, and a low apical mitral systolic murmur was heard which he thought was due to dilatation of the heart. About that time or a little later she was losing flesh in a marked manner. He was summoned down in a great hurry on two occasions and had to inject strychnine because she was so ill. In the spring of this year she came to London and her teeth began to trouble her, although no actual disease was visible in them. The gums receded a good deal and showed the fangs. Nothing else was visible. The pain and tenderness caused so much annoyance that the worst offending molar was removed. In July she was taken to Mr. Goadby, who wrote to say he had extracted a root abscess from Mrs. B—'s mouth and found she was suffering from sclerosing osteitis, the chief organisms being two different varieties of bacilli. One of them was the *Strepto-bacillus necrodentalis* and the other closely related to the *Bacillus pyocyaneus*. Mr. Goadby prepared a vaccine for her from the mixture of the two organisms, and some time afterwards there was considerable improvement in the osteitis in the bone. He (Dr. Davy) saw her after those vaccines had been used for some time and he was struck with the improvement in her heart, which, when he saw her in October, was described as normal. He heard that Mr. Goadby had tried to obtain a culture of the bacilli by passing a probe up into the sockets of the teeth, but since the use of the vaccines he could no longer get a culture. That was interesting as showing not only that in diseased teeth one had a condition which produced grave anæmia, such as everybody recognised, but that there might be growths from the bacilli of such a nature that the toxins from them affected the heart, and in that way caused the disease which more frequently than not was the cause of the trouble.

January 30th 1911.

(*To be concluded.*)

A Difficult Case.—Much sympathy has been extended, and rightly so, to Mr. Charles Ryall, the well-known surgeon to the Cancer Hospital, for the trouble and inconvenience he has experienced in connection with the case that was unsuccessfully brought against him in the Courts of Law for alleged negligence in the performance of an operation. In our opinion Mr. Ryall still further demands the sympathy of the medical world, because in some most extraordinary manner the idea has got abroad that he left a swab in the peritoneal cavity at the operation, and this in spite of the fact that the learned judge, in summing up, explained aptly and clearly what Mr. Ryall had done with the swab by describing the procedure as putting a swab in the intestine to form a cushion for the support of a torn bowel in the process of repairing a tear. From the evidence given at the trial it transpired that the tear occurred in a part of the large bowel, which was bound down deep in the pelvis by dense adhesions, and that the swab was inserted into the intestine to prevent leakage of fæcal matter into the peritoneal cavity. When the diseased organs were all removed it was plain that to remove the swab from the intestine when suturing would have subjected the patient to the risk of fatal sepsis from leakage of the contents of the bowel. It followed therefore as a matter of course that the surgeon, in leaving the swab in the intestine and suturing over it, did the best possible for his patient.

Dangers of Iodine Medication.—Römheld remarks that few realise that iodine and all preparations of iodine, including thyroid tablets, are liable to induce exophthalmic goitre in a patient with a personal or family tendency to goitre and obesity, even when the individual presents no signs of goitre. Every year he encounters cases of incipient or feared arterio-sclerosis in which iodine has been given, and serious symptoms suggesting exophthalmic goitre have developed. He reports in detail eight cases to sustain his assertion. In six of the cases a latent exophthalmic goitre was aroused and aggravated, and in the others patients with a family history of goitre developed the exophthalmic syndrome under the iodine. The severity of the symptoms does not seem to stand in any proportion to the amount of iodine taken.—*Journ. A. M. A.*, vol. lvi, No. 2.

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A LECTURE ON DERMATOLOGICAL CASES.

Delivered at the West London Hospital.

By PHINEAS S. ABRAHAM, M.A., M.D., B.Sc., F.R.C.S.I.,

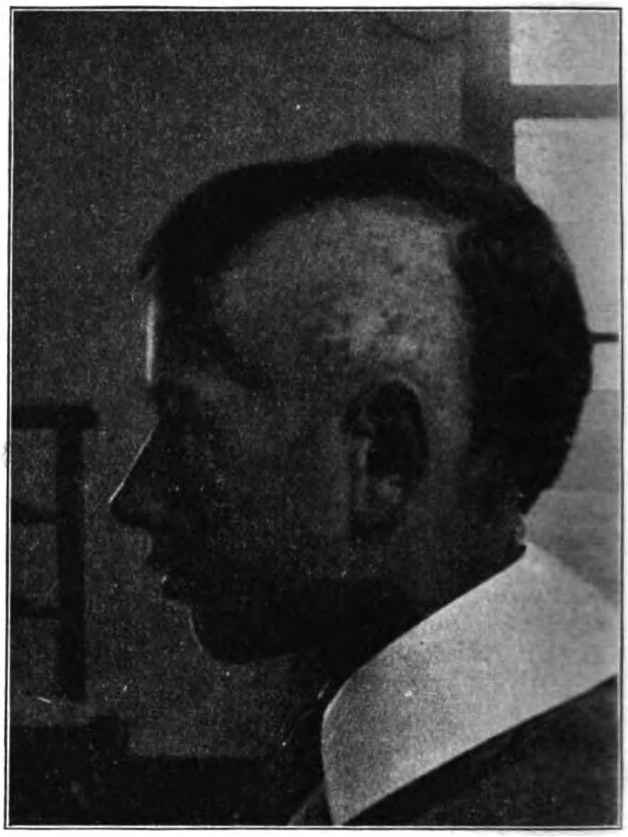
Dermatologist to the Hospital, and Lecturer on Diseases of the Skin, West London Post-Graduate College.

GENTLEMEN,—I generally think on these occasions that it is better to show you cases than to deliver a set lecture. You can read about a skin disease much better in books and in the journals, and I think it is more profitable for practical men to see the patients, to observe how their diseases are diagnosed, and to hear what, in the experience of this hospital, is the best treatment for them. I have made a selection of cases to-day which I hope will be interesting.

The first cases I shall show you are a series of patients with ringworm. This little girl is under treatment, and you will observe that there is a considerable amount of alopecia, which I like to see. You will also observe the remains of one of the original patches, showing a number of good hairs, and at the same time some thick, short, crooked hairs, reminding one of broken fingers, amongst them. This child will be cured of her ringworm very soon, probably in another month or two. In the majority of cases of ringworm, if you get them early enough you can cure them within two to four months by the old-fashioned methods of applying germicides of various kinds, preferably in the form of ointments. In order to get medicaments into the scalp, you want something which will mix with the natural sebaceous secretions. Ointment mixes with the greasy sebaceous matter in the scalp much more effectually than watery solutions. Many people have tried to substitute liquid applications for ointments, but I do not think they are so satisfactory. Chloroform and

ether, although they penetrate, seem to irritate the scalp, and I rarely now use them as vehicles. This child has been treated with a 5 per cent. pyrogallic acid ointment, which can be increased to 10 per cent. in strength. Another ointment which I use very frequently here with success contains cuprate of copper and salicylic acid and carbolic acid, about a drachm of each of these latter, and two to four drachms of cuprate of copper, incorporated with an ounce of lanoline and vaseline, in equal parts, or with some other ointment base. These ointments should be well rubbed into the patches night and morning, and a little kept smeared over the whole scalp as well, with the hair all over being kept short, or even occasionally shaved. To still further obviate the risk of the fungus spreading, the child should also wear some cheap form of cap, which can be destroyed every few days by burning. Most cases seen early enough should soon be cured by means of this kind. I say this in full knowledge of what you will have read elsewhere in the medical papers, that the only way of treating ringworm successfully is by X rays. I admit that, however experienced you are in treating ringworm, you will come across a certain number of cases which hang fire, and in which the disease persists, and the patient may be under treatment for months or even years. The best and most experienced dermatologists have cases of this kind. Dr. Alder Smith, who has had an enormous experience of ringworm, and is a well-recognised authority on the subject, concluded that the treatment of ringworm in many cases is hopeless; that one had to wait until the disease wore itself out and died a natural death when the child was sixteen or seventeen years of age. When the treatment by X rays was introduced a few years ago we found that we had a method of removing the diseased hairs in a very efficacious manner. I think everyone admits now that the rays do not destroy the fungus; they only destroy the nutrition of the hair, and it is shed. The diseased hair comes out, and if you are lucky, and you do no damage, the hair grows again. I shall show you some children soon who have had X-ray treatment, and in whom no harm was done. But there are cases in which those rays have done damage, although you might not expect anything of the kind from what one has read in the medical papers lately. I showed a short time ago, at the West London Medico-Chirurgical Society, a little girl who was X-rayed at one of the largest hospitals in London by one of the best men, under all precautions, such as using Sabouraud's pastilles to measure the strength of the rays, and this child had severe X-ray dermatitis immediately afterwards. The exposure was made in June of last year, that is to say, eight months ago. She now has a very ugly scar all over the side of her head, and but very little hair growing there. One part is still ulcerated from the X-ray dermatitis;

Fig. 1.



the other side of her head is also slightly scarred, and there is some permanent alopecia there. I have seen five or six cases as bad as that; two of them are illustrated in these photographs (Figs. 1 and 2). One case was that of a boy, who was treated four years ago by the rays by a first-rate man in London, before Sabouraud pastilles were introduced. He has a keloidal scarring on the back of his neck, the back and both sides of the head. He will never have hair upon these places and is marked for life (see Fig. 1). The second case figured is that of a girl who was X-rayed at a

hospital, and will also be permanently disfigured. This photograph was taken nine months after the treatment.

Since the possible dangers of X rays have been more fully known, great precautions to prevent bad results have been taken; but in the earlier days of the use of the rays there were, I believe, many cases of permanent alopecia and some severe ulcerations in the scalp. Still, the case I showed recently illustrates the fact that even now such cases may occur. That being so, I maintain that

FIG. 2.

one is not justified in using X rays in every simple case of ringworm which comes before one, especially as everyone of experience knows that the majority of cases of ringworm in former times were cured by simple and safer measures. Dr. Alder Smith pointed out what a good thing it was in treating ringworm if there appeared some alopecia, as in the case before you, and he tried to produce that alopecia. That is the underlying principle of the treatment by X rays, viz. that you produce a bald patch, upon which subsequently new healthy hairs will grow.

The children now coming in were sent by me to our X-ray Department under Dr. Morton and were all long-standing cases and very extensively affected; I felt justified in these patients in making a clean sweep of the disease early in the treatment. One point I want to show you particularly is, that if these children are left alone now, when the hair grows again they will probably have ringworm again. I have seen such recurrence in several instances. The fact is, that in some parts of this child's scalp there are ringworm hairs still persisting. In this case I expect Dr. Morton was over-cautious for fear of doing damage, so that there is not quite enough of the hair removed. In showing you these cases which have been treated by X rays without harm, you will see that I am not really prejudiced against that treatment in certain cases, but that I reserve it for the very obstinate ones which have been extensive as well as long-standing before I saw them. I may say I have never seen any case which bore out the statement which was made some time ago that the brain is sometimes damaged by the application of X rays to the head; but as I have said, I have seen several of very ugly scarring and permanent alopecia.

This little boy presents a typical case of ringworm or tinea tonsurans. Here you see many short hairs, each hair being opaque and thickened, and one of the hairs of this boy is under the microscope now. These short, broken hairs everyone ought to be able to recognise when they see a case of the kind. There are old-standing cases of ringworm in which the disease exists all over the scalp, which is then very scurvy, and in consequence you may not be able to see so easily the broken hairs. I have known several such cases which have been treated for years for supposed eczema, the doctor never having taken the trouble to find out whether there were diseased hairs or any fungus on the scalp. Sometimes there are abundant crusts, which also mask the short hairs. You must scrape away the scurf and crusts and examine these for fragments of hairs, which you will find full of fungus. In one instance at this hospital, a girl, æt. 15 years, really suffering from ringworm, had been under treatment for supposed eczema of the scalp ever since infancy. In this boy, as you will see under the microscope, the fungus is of the small spore or "microsporon" variety. I am ordering his head to be shaved,

because there may be small patches elsewhere on the scalp which his mother does not know of, and because the applications can be more thoroughly and more widely rubbed in. The rapidity of the cure in these cases depends upon how far the fungus has got into the hair-follicles. If the spores or mycelium are at the top or near the mouth of the follicle, any germicide will be efficacious, but if the fungus once gets deep down the disease is much more difficult to cure. I have known many cases of early ringworm cured by parents with common black ink, common salt, verdigris, or common turpentine. Almost any germicide, indeed, will cure some cases of ringworm, *i. e.* when the condition is comparatively superficial. Some time ago I had three children brought to me with ringworm; two of them were cured in three weeks, but the third took seven months. Ringworm which occurs anywhere else in the body where there are no deep hair-follicles can of course be easily and rapidly cured by tr. of iodine or any germicide in solution or ointment.

Here is an interesting case of lupus erythematosus in a young woman. She has had it four years. She had typical erythematous patches on both cheeks and on the side of the nose. She only came here six weeks ago, and under the treatment which we recommend in these cases there has been a remarkable improvement. I have had similar good results by the same treatment, so I thought it worth while to show this patient. There is now left only a small patch each side, and you can barely see where it has been on the cheeks. On the nose very little is now to be observed. The treatment consisted in a very mild antiseptic ointment, containing a little resorcin, a little ichthyol and a little salicylic acid, 10 gr. of each to the ounce, and I think there is also some zinc oxide in it. This has been freely applied, and I have been giving her rather large doses of quinine, five grains three times a day. It is of little use administering quinine for these skin cases unless you give fairly large doses. In some cases I have increased the quinine up to as much as 15 gr. three times a day, that is to say 45 gr. per day. She has the typical slight affection of the concha of the ear which Sir Jonathan Hutchinson pointed out many years ago, reminding one of orange-peel.

Here is another case, that of a woman, with another form of erythematous lupus, more of the hypertrophic and sebaceous type. The skin is thickened, and when she came under observation six years ago it was a very bad case. At one time it was so bad and the thickening so great, and the tendency towards ulceration was so marked, that one very good authority who saw her thought it must be lupus vulgaris. I remember many years ago that a very high authority lectured on a case at the Blackfriars Skin Hospital as one of "lupus vulgaris," and a year or two later, having forgotten the case, showed it again as one of "lupus erythematosus." Sometimes, indeed, these conditions apparently pass into each other, and it is difficult to make an exact diagnosis. A casual observer might now, indeed, mistake the case for one of chronic eczema. This woman has not been very regular in her attendance, but nevertheless she has improved immensely. She has been having the same large doses of quinine off and on for a very long time, that is to say since she came under my notice six years ago; and she is an illustration of what I have noticed in many cases, that they can take quinine in large doses over a long time without the hearing being damaged. In this instance I used very vigorous treatment besides, ethylate of sodium painted on, and sometimes crystalline carbolic acid and camphor in equal parts. The latter is efficacious and does no damage, and you can paint it on with impunity; but where there is induration ethylate of sodium is more useful as a strong cautery destroying the hardened skin and new growth. Some of my cases of erythematous lupus have done well with carbonic acid snow, which gives a comparatively quick result. But where there is no induration, the treatment I have mentioned is sufficient. Neither of these patients has patches on the scalp; such I generally find particularly obstinate.

Here is a man, middle-aged, a labourer, with acute dermatitis of the face. He has to do with lime and mortar, and has been a great deal exposed to wind and weather. He has not the usual trade dermatitis on the hands. He will be all right soon. I am giving him an antiseptic lotion containing a little tar and calamine to use to his face, and the unguentum metallorum co. of this hospital, half-strength. I have, of course, knocked him off his alcoholic drinks and everything of that sort, and told him to look after his diet carefully. The indications for treatment are,

first of all, protection, and the use of mild antiseptics—no strong applications. One of the reasons that many medical men are not successful in treating cases of eczema is that they use the ointments of the British Pharmacopœia, the majority of which are too strong for most skin cases. The elder Startin at the Blackfriars Skin Hospital was, I believe, the first to show that you get better results in dermatology by using very weak applications; many of the ointments he advised contained only 5 to 10 gr. to the ounce instead of 1 drm. to the ounce, as is the usual proportion in the British Pharmacopœia preparations. In using the latter you may well dilute them to 1 per cent. or 2 per cent., that is to say, to about 5 or 10 gr. to the ounce. The ointment that this man is using contains 5 gr. of carbonate or acetate of lead, 10 gr. oxide of zinc, and 10 gr. of the ointment of nitrate of mercury to the ounce of vaseline.

This next man presents a typical syphilitic ulceration of the nose. There is a history of a primary sore in 1895, when he was in the Army, and in those days the treatment was not as thorough as it now is under our most excellent and "up-to-date" R.A.M.C. He tells me he was treated with mercury for the disease only for a few weeks. Of course that is not sufficient for this disease, and it is not surprising that late symptoms frequently appear in old soldiers who served many years ago. This is a typical case of syphilitic ulceration, and it ought not to be mistaken for anything else. The only thing which you would be at all likely to mistake it for is lupus, but it has not outlying nodules with the yellow apple-jelly appearance on pressure. The ulceration, moreover, is more extensive, and it is more rapid in its course, and there is not the same crusting as in lupus vulgaris, and it is not so soft in consistence. It might possibly be mistaken for a malignant ulceration, say rodent ulcer, but in that case the edge would be hard, and it would not be so much inflamed around as this is. It has improved in appearance in the course of a week, and is also closing in, which neither of the above would do in so short a time. He is taking mist. hydr. biniod., which contains a drachm of liquor hydrarg. perchloridi and 5 gr. of potassium iodide, and he is using unguentum rubrum, containing only 5 gr. of red oxide of mercury and 5 gr. of red sulphide, with a little creasote to the ounce. He has only been under treatment since November 4th, and there has really been very great improvement.

The next patient is a boy with alopecia areata. He has not been X rayed. It is *sui generis*, and has nothing to do with ringworm. His hair first started coming out at the age of five, and he is now ten. When he came, the hair was practically all off, but it is now coming back everywhere, and I think ultimately his scalp will be quite covered. In my view, alopecia areata is an effect which may be caused in various ways. The majority of cases I believe to be due to microbic action; but in this instance there is no evidence of the boy having had any other fungus or microbic disease, or seborrhœa; there is no history of anything of that sort; but he is an extremely nervous boy, and when he went to school his hair came off very quickly.

I believe that only a small minority of cases of alopecia are due to the nutrition of the scalp being interfered with through some functional affection of the nerves. You may get cases of that sort in people who have had headaches or extreme neuralgia; and in some few instances the patient has had a fall or some severe general shock to the nervous system. But in a large number of the cases I believe the condition follows seborrhœa capitis, or something of that kind, and that it is really a microbic disease. The treatment in these cases is, as a rule, very successful by the use of stimulant and antiseptic applications. Generally I stimulate the scalp freely with a solution containing ammonia, tincture of cantharides, and turpentine, or something of that sort, being of an antiseptic as well as stimulating nature, and at the same time I have rubbed in an hour or two afterwards some antiseptic ointment, which may contain some perchloride of mercury as well as salicylic and carbolic acids. This boy has been so treated, and he has been taking phosphate of iron and a small amount of strychnine. Iron is very often useful as a tonic treatment in these cases, especially where there is no distinct microbic possibility. When X rays first came in I used them in several cases of alopecia areata, but I could not find any distinct benefit from them. The first bad case of ulceration of the scalp which I saw from X rays was in a case of my own, which had exten-

sive alopecia areata. I thought it would be a good thing to try to remove the bad hairs, and I had read that X rays sometimes promoted the growth of hair. An expert in X-ray work gave my patient seven or eight mild applications of X rays, and then stopped applying them for about a fortnight. There was no appreciable reaction, and after a further series of exposures there was still no reaction; but two or three months afterwards ulcerations started, and I saw the unfortunate patient six months later with her scalp then badly ulcerated. The treatment did not stop the alopecia, and the hair never came back. It is only fair to say that this was long before M. Sabouraud introduced the pastille method of measuring X rays.

Here is a young girl who illustrates the treatment of acne vulgaris. There is not time now to demonstrate fully the method. She had very bad acne some time ago; her face was a mass of pustules and "blackheads" or comedones. She has improved very much under treatment. We nearly always find digestive or circulatory disturbances in these cases; there may be constipation or the reverse. A very useful medicine is the common M.F.A. of the hospitals—iron sulphate and magnesia with some bitter and a little sulphuric acid. The proportions vary according to circumstances. In cases where there is flushing after meals it is best to begin, especially if there is no constipation, with bismuth mixtures before meals, and to try to get the mucous membrane of the stomach, etc., into good order. In addition to the internal treatment, the external treatment is most important. What does excellently in most cases is the unguentum sulph. co. of this place, a modification of a very old one of the Blackfriars Skin Hospital, containing sulph. precip., ʒss; hyd. sulphureti, gr. v; hyd. ammoniati, gr. x; vaselin, ʒj. This is applied after bathing the face with some soap and hot water and pressing out the blackheads. Practically anything with a small hole in the centre which can be pressed on can be used for pressing out the blackheads. Many people use an ordinary watchkey, but we have this special instrument for the purpose, which is far more serviceable and not painful to use. After pressing out we generally apply a little carbolic acid in strong solution, or equal parts of crystalline carbolic acid and camphor rubbed together; but any antiseptic will do. If there is a pustule this should be opened and cleared out, and if there are subcutaneous abscesses, as there may be in bad cases, these should be opened and washed out. For this I use a hypodermic syringe with a blunt-pointed needle. The little purulent collections, indeed, should be treated as one does a big abscess. If this treatment is thoroughly carried out, however bad a case of acne may be, you should be able to deal with it and cure it. Most men, it seems, have not the time, or will not take the trouble, to thus treat each little lesion separately.

We know that the microbes which produce pus, and probably the acne microbes too, exist on all skins. They increase and multiply and do their work, produce comedones and pustules if the soil is favourable to them, but they do not do so if it is not. I am certain that most of these cases have something unhealthy about their tissues, and that it is most important to treat the cases internally as well as externally. The results which you get by the method I have mentioned are very good. Those who attend this clinic are sometimes astonished to see a person who has come with a face a mass of blackheads and pustules come again in a fortnight with most of them gone. There is no doubt that some cases of acne recently have cleared up like a charm under the new vaccine treatment. But, on the other hand, there are cases which have been made infinitely worse by it. I have no hesitation in saying that in the last year and a half six of the worst cases of acne I have seen in private have been cases which have been made bad by the new vaccine treatment—and in the very best hands. I say this because otherwise you may have thought these bad results were caused by inexperienced operators or amateurs. I look upon the vaccine treatment of acne as at the present time experimental, and I should only order it in very bad cases which had resisted all other treatment. To use it in slight cases is, in my opinion, absurd, especially as I know that we can cure these cases easily in the way I have mentioned, or perhaps by other methods, without fear of damage. Only this morning I saw one patient in private and another yesterday—young women whose faces have been made terribly bad by the injection of a vaccine. One of these patients will be disfigured for life, as though she had had very bad confluent smallpox, and she and her parents declare that before commencing the treatment she

had only a few acne pustules on her face, but nothing special to speak of. Her doctor had been very enthusiastic about the new method, and he insisted on employing it.

This little girl came here two years ago with a large number of horny protuberances or keratosic masses sticking out. She had some on the toes, a very big one on the dorsum of one hand, and others on her abdomen and thighs. Some of them were veritable "cornua cutanea." On account of those on the feet she walked very badly. We treated her vigorously by scraping them away and applying acid nitrate of mercury to the base, and using salicylic acid ointment to soften the smaller ones. She turned up again the other day with a similar condition on the sole of the foot. We are treating this with solid CO_2; she had one application last week and another one to-day. We have scraped or pared some of the growth, and Dr. Morton is applying the frozen gas to the base. It hurts her a little afterwards. These cases of cutaneous horns are very rare, but the growths may come anywhere on the limbs or body. It is sometimes difficult to draw the line between them and callosities. I have a boy now in hospital who came in June in a practically helpless condition, not being able to use his hands or feet, which were the seat of several large callosities which were evidently most exquisitely painful. We had to put him under a general anæsthetic, and then treat them as in the last patient's case. They are very much better now, but not quite cured.

I wish all of you had seen this next patient, a woman, six weeks ago. She then had an obstinate eruption, large papules in groups, all very dark red in colour, on her face, on her back, and several other places. She had been under the usual specific treatment with hydrarg. and iodide of potassium for about eighteen months, and there had been certainly some improvement; but the eruption would not go, and it was really one of the most obstinate syphilides that I have seen. She had one dose of "606" six weeks ago, and the whole thing has cleared up. She now feels better than she has ever felt before. The "606" seems to have been an excellent success in her case. Mr. McDonagh, who is one of the clinical assistants here, has had a supply of the Ehrlich-Hata preparation, and this is one of the cases that he has injected for me. Three or four others have also done remarkably well in this hospital under the same treatment.

February 6th, 1911.

ON DILATATION OF THE HEART AND HEART-STRAIN.

A Discussion before the Medical Society of London.

BY

Dr. F. J. Goodhart.
Sir Richard Douglas Powell.
Sir Lauder Brunton, F.R.S.
Dr. Henry Davy.
Dr. W. Collier.
Dr. Samuel West.
Dr. Walter Broadbent.
Dr. Alexander Morison.
Sir John Broadbent.
Dr. Charles Chapman.
Dr. Leonard Guthrie.
Dr. Parkes Weber.

Mr. Charters Symonds in the Chair.

(*Concluded from p.* 272.)

Dr. Collier (Oxford) said that his interest in the question of heart-strain had for many years centred around the question of athletic heart-strain, and he would confine himself to that question. He was conscious of the fact that during the past few years the subject had, unfortunately, been discussed in the lay press on at least three occasions. He said "unfortunately," because he believed the general public, owing to the correspondence in the lay press, had formed an erroneous opinion of the effects of athletic strain. Any layman who read some of the letters which had appeared could only come to the conclusion that athletic competitions, as carried on at our public schools and ancient universities, must be a very potent source of life-long injury to many schoolboys and undergraduates.

But what were the facts? Less than two years ago the medical officers of the public schools took the trouble to send a circular letter to all their officers inquiring into the subject, and asking if the medical officers were aware of any cases which had occurred at their schools of injury to the heart from athletics. A similar circular was sent to about twenty medical practitioners at the Universities of Cambridge and Oxford. He had had the opportunity of going carefully through the answers returned, and the conclusion he arrived at was that the medical officers of the public schools were almost unanimously of the opinion that there was very little injury done; and he knew the medical men at the universities were very much of the same opinion in regard to the universities. He had himself been in practice at Oxford for twenty-five years, and he lived nearer the river than any other medical man. But he could not remember more than two or three occasions on which he had been

called to either the barges or the running-ground to deal with a condition of severe collapse from athletic competitions; nor had he found it necessary, except on very few occasions, to keep a man in bed for more than a day or two at the most owing to the effects of athletic competition. But he admitted that cases of heart-strain did occasionally occur among athletes, and his opinion was that it usually took the form of a definite alteration in one or other cavities of the heart, and was not in any way connected with neurasthenia. He was frequently called upon to examine men as to their fitness for either rowing or running, and he now seldom began by asking many questions. He requested the man to strip, and then he examined him, first standing, then in the horizontal position. Often he found absolutely nothing abnormal until he made the man walk round the room, or run up and down stairs; a definite murmur appeared which could be traced up the left side of the sternum, systolic in time. This murmur did not come and go. If one saw the man the next day or the next week, and made him do the same things, the murmur would appear. The murmur ran up towards the pulmonary area, and he believed it was due to a dilated condition of the right ventricle; that as the right ventricle contracted, its base was pressed against the unyielding sternum, an alteration took place in the shape of the pulmonary valve, and a murmur was produced by an eddy in the blood-current. He practically invariably found that the murmur was connected with a very definite symptom, namely, abnormal breathlessness. The murmur occurred in two quite different types of athletes. One was the narrow-chested, badly developed, muscularly flabby man, who had for the first time taken up hard athletic exercises. The second type was the muscular man, who had probably been overdoing it. A man of the first type was often able to say quite definitely that he knew that his wind was not as good as the other members of his crew. The man of the second type, who knew what he could do, found that he was below form, but did not know the reason. If he were a runner he felt fagged earlier than formerly, and if in a boat the exertion seemed greater than formerly. Men had come to him, not because they wished themselves to see a doctor, but had been sent by friends, who had noticed their change in colour and early fatigue when called upon to exert themselves. When such a murmur occurred after only a few seconds' exertion, one naturally asked oneself what must be the condition of such a heart at the end of, say, three minutes' very strenuous exertion. That was particularly the case in rowing, for rowing, on the whole, threw a greater strain on the individual than other sports. For example, when a man was running on the track he could stop when he felt exhausted, or at football he could ease up, but when in a boat a man felt that he must keep on somehow and could not give up until the end of the course was reached. He thought he was justified in telling a man, such as he had described, that he should stop all strenuous exercise. With regard to treatment, he thought the most important treatment of all was to reassure the patient. He tried by every means to make the patient realise that he was not to consider himself an invalid; he advised him that, as long as he gave up hard athletic competitions and lived what might be called the physiological life, going to bed in good time, smoking only moderately, and avoiding heavy meals, he might go in for golf, or lawn-tennis foursomes, might row, or punt, or swim, or cycle, always carrying on these exercises in a leisurely way. One question occurred to him. Was it necessary for a patient suffering with this form of heart trouble ever to be sent to Nauheim? He thought not. It might induce these patients to think too much about their hearts—a state of affairs which was not at all desirable. One occasionally saw cases of what he took to be definite dilatation of the left ventricle, as the result of too great exertion, with a definite systolic mitral murmur, the result of the dilatation. One such case he had recently seen, and he believed that such cases required much more prolonged treatment by rest, etc. With regard to the question of the value of mapping out the area of heart dulness, Sir Lauder Brunton had given only last week very definite instructions as to the best means of bringing out the dulness, but he (Dr. Collier) was obliged to confess that in those cases in which it was probable that the area was not much increased, he was quite unable to demonstrate the dulness to himself, his ear was not good enough, and he had found on inquiry that many of his friends had to make the same confession. With reference to the question of fainting, it occasionally happened that a strong, athletic man came to him, because,

perhaps, in an overheated, stuffy room, he had fainted, and was very much frightened by the incident. Dr. Collier had learned by experience that in such cases fainting meant very little, and that such patients could run and row just as well and as safely as ever. Among other points, Dr. Collier would like to hear discussed the question as to whether the marked hypertrophy of the left ventricle, which attended oft-repeated competitions, was followed by any bad effects in later life. It was often stated that the fine athletic man of thirty had a habit of becoming a hopeless invalid between fifty and sixty. He had not seen any striking examples of this change among his own athletic friends, and would like to learn the experience of others.

Dr. SAMUEL WEST said that the subject was so extensive that it was difficult to select the part to deal with. As to what was meant by strain, it was common to hear people who were overworked in mind or body to be said to be working under great pressure or great strain, and if they broke down, as they often did, either from the nervous or the muscular side, they were said to be suffering from nervous or muscular breakdown, in other words, from over-strain. Strain meant overwork. In the case of the heart it was very important to distinguish between the two groups. And it was possible to do so, although the symptoms were, in many respects, much the same: palpitation, some discomfort or pain, and shortness of breath. At the same time, those who complained most of palpitation were often those who had least organically wrong with them; one might even go further, and say that if cardiac lesions developed the nervous side might diminish, exactly as happened in cases of asthma. The diagnosis between the two groups was the difficulty, and mistakes were frequent enough to teach caution. A case in point came under his observation some years ago, the patient being a young woman, æt. 30 years, a little pale, but otherwise healthy-looking, who complained of severe attacks of pain in the cardiac region. He could not fit the symptoms in with anything organic. He therefore regarded them as nervous, and advised her not to attach much importance to them. A few days later her husband reported that she had died suddenly in an attack at night. The difficulties of diagnosis were further increased by the fact that the two classes cut into one another. It was easy for nervous symptoms to be added to those of organic heart disease or *vice versâ*. There was no organ more liable to nervous influences than the heart. All were familiar with the distress which nervous people suffered from when suddenly called upon to make a speech without preparation—those feelings of tension, palpitation, and choking. Every rowing man was familiar with the awful sensation of palpitation and tension, as if the heart would burst, while waiting for the gun to fire to start a race, colloquially called "the needles." This was a good illustration of the effect of work on these symptoms, for directly the race began and the heart had something to do it quieted down. In training there was a nervous as well as a physical side, and a training of the motor nerve-centres as well as of the muscles. He looked upon the "needles" as due to the nervous symptoms getting somewhat ahead and starting off first, for as soon as the heart got to work those symptoms adjusted themselves and the distress passed away. He would now turn to the cardiac or muscular side, on which there seemed to be much to say. As the heart was a muscle it must be subject to the same affections that occurred in other muscles; for instance, if a muscle were over-fatigued it would be the seat of pain, which might even end in violent cramp. This was a common experience in the calf muscles during a walking race. Cramp of the heart would be immediately fatal. But a better analogy of the conditions which might actually arise in the heart was given by a hollow muscular organ, like the bladder, when acting against obstruction. It did its best to overcome the obstruction, and if it failed it then dilated and produced pain, which might end in colic. In the heart this would be angina. It was very difficult to strain a healthy heart. The heart seemed able to deal with any ordinary demand upon it, for it had enormous reserves. In long-continued strain, *i. e.* extra work, for instance, in training, the heart hypertrophied because of the increased work. But there was this difference between training and health: the hypertrophy was attended with a certain amount of absorption, so to speak, of the reserves of the heart, and therefore one understood the paradox that the hypertrophied heart was more closely allied to a weak heart than to a strong one, for there were smaller reserves. When it gave way the power of the heart became

less than the work which it had to do. The result was dilatation, and symptoms of embarrassment and distress appeared. He gathered from Dr. Goodhart's remarks that many of the cases did not present the physical signs of dilatation, but he (Dr. West) thought dilatation must have been present in every case at the time, and probably to an amount which could have been estimated if examined then. But in a healthy heart the dilatation quickly passed off. There was another difficulty in demonstrating dilatation, viz. that increase in the size of the heart might be masked by the lungs, for, like the heart, the lungs hypertrophied in training, and, being enlarged, encroached upon the area of cardiac dulness just as in emphysema. The condition was often called emphysema, but it was really hypertrophy, for the respiratory capacity of the chest was not diminished, but largely increased. In the young and healthy heart those dilatations were quickly recovered from, but not so in elderly hearts or diseased hearts, and in them might not be recovered from at all. Transient dilatation was easy to demonstrate where the heart was weak and sometimes where it was diseased; for instance, in chlorosis, and in convalescence, especially from a disease like rheumatic fever in patients who had been some time in bed and got up too soon. That dilatation quickly passed off. It was a strain, but not of any great intensity. One of the best instances of muscular strain and the effect it produced on the heart was what was met with in the case of a ruptured valve, but the embarrassment which followed immediately on the rupture and the dilatation which occurred with it might even then pass off in time. Where the heart was greatly hypertrophied, as in aortic disease, sudden failure was one of the common symptoms, even a slight strain producing sudden and complete failure. Patients might even date their disease from such a slight strain, although known to the doctor to have existed a long time before. Cardiac failure was still more likely to follow great exertion, as in the following case: A schoolmaster, who was a great athlete and swimmer, was known to have had double aortic disease for ten or twelve years, and had been warned about it, but not feeling any distress did not take the warning, but continued to run and swim as usual. One day he was with his boys bathing. A boy got carried away by the stream and was drowning; the schoolmaster plunged in, swam out, and brought him back. He was very exhausted, and when he reached the bank had an attack of angina. From that time he was never well, and died in a couple of years of gradually increasing heart failure. There was another form of hypertrophied heart, in which the sudden onset of symptoms was often surprising, namely, those which occurred in the course of granular kidney. The first evidence suggesting grave disease either to the patient or those about him might then be sudden, acute failure of the heart, from which recovery never took place. A sharp distinction must be drawn between the cases in which the hypertrophy was the result of disease, and those in which it was the normal physiological result of muscular work. He did not remember to have seen a case of serious heart affection or strain lasting for any time in a healthy young adult. Nor did he remember at all to have seen anyone who had been a strong and athletic man in his youth breaking down in middle life with heart symptoms which could be fairly attributed to athleticism and which could not be explained in any other way. It was very difficult to bring this question forward in such a way as to convince those who were sceptical, but his opinion was in agreement with that of many who had had very special experience. This was explained by the fact, already referred to, that when the heart hypertrophies, it does so at the expense of its normal reserves. In a healthy heart, as soon as the extra work ceases the hypertrophy passes off, and the reserves are proportionately increased until they again reach the normal. A normal heart might, of course, suffer from strain when very great effort was undertaken, especially in those who were not in training, and the effects were then much more likely to be serious, as might be expected, for the general muscles tired or the breathing went before the heart. The principles of treatment in all those cases were the same, viz. to diminish the work the heart had to do and to increase its power to do it. A muscle not exercised became feeble and wasted, and therefore it must be kept exercised if it was to do its work properly. How much it could do was a matter for observation. Therefore, in speaking of work and of exercise in the treatment of heart weakness it was obvious that supervision was necessary. That was one of the principles of Nauheim treatment, but he did not think people need go to

Nauheim for the purpose if they would only do at home what they were asked to do at Nauheim. With regard to the neurasthenic group, where cardiac symptoms predominated, rest was necessary for the nerve disease from which the patient was suffering, though how far it was necessary for the heart was another matter. Cardiac neurotics were often not well and suffered from their hearts because their nutrition was poor, but there was a distinct danger in laying stress on the cardiac symptom, viz. the risk of manufacturing cardiac neurotics. For cardiac neurotics regulated exercise was essential, and under stricter supervision exercise was beneficial, even with organic disease. The extraordinary anæmia which develops in great athletes had already been referred to; they often had enormous muscles and yet looked very anæmic and ill. The same thing was seen in over-trained men, but not to the same degree—over-training seemed to produce anæmia.

Dr. WALTER BROADBENT said all must agree with Dr. Goodhart that there was a certain number of cases of so-called heart-strain which were neurasthenic, and some others in which there was great complaint of heart symptoms, but which were due to dyspepsia. His father used to say that if a person came complaining of his heart one should examine the stomach first. That gave one much help in some of these cases. But there were many genuine cases of heart-strain, and there was one interesting method of examining them which had not yet been mentioned, namely, by taking a tracing of the jugular pulse by means of Mackenzie's polygram. It was not difficult to do, and most interesting results came from it as far as his own experience went. He had found that in the cases which were functional and those due to dyspepsia there was a normal jugular tracing. But in the other cases there were two variations from the normal, one in which the auricular wave was very much bigger than the ventricular—it was very big—and the other class was that which he had mostly found in people who had passed thirty-five years of age, in which the ventricular wave was much bigger than the auricular. When the auricular wave was big, so far as he had seen it was in rather young people. And he had found that if they were told to leave off their very active games for a time and they were given a tonic and perhaps a purge, in quite a number of cases it was not necessary to make them rest. Often they would be found to have a little dilatation of the right side of the heart, and that could sometimes be easily ascertained by percussion. Those young people could return to their games with only a little moderation after four or five weeks. In the other class of cases where the ventricular wave had been big, he had had good results from making them rest and after a time getting them to take graduated exercises, and treating them with cardiac tonics. By that means the ventricular wave usually became smaller than the auricular. But every time such people went back to their former exercises they broke down. This method was therefore useful in finding out what the prognosis in those cases was. It was especially useful in athletic people who had emphysematous chests, which point Dr. Collier referred to. He (Dr. Broadbent) was not capable of percussing out accurate deep cardiac dulness in an emphysematous chest, and he was sorry to say he did not quite believe people who said they could. There was another class of case which one saw, with definite dilatation. Whenever he found a patient with a definite dilatation he always put him to bed and gave him some calomel and cardiac tonics to see what would happen to his apex-beat, but he did not let such patients rest absolutely, but ordered massage because he agreed with Dr. West that it was a mistake to let the heart rest absolutely. If such people were kept well massaged the heart did not go down in tone. In young people he had seen the position of the apex-beat come in an inch in one week. He did not put the patient to bed for any length of time, but as soon as the apex-beat ceased to come in it was time to get the patient up into the sitting posture first. The massage was continued until the patient was on to his feet again. After they had been walking on the flat a few days he found a certain amount of walking up-hill was good for them and hastened recovery. Brighton was particularly well suited for them as there were plenty of hills there, and it was also very bracing. He told the patient to go up some hill near his residence every day, to go as far as the first lamp-post one day, and to the next lamp-post in one, two, or three days' time according to the steepness of the hill. A week ago he saw a gentleman whom he had seen in the first instance some time ago. He was under forty

years of age and had had dilatation of the heart which had been attributed to a big strain a year before, and the whole year he had never got beyond walking two or three hundred yards at a time, and that caused him considerable breathlessness. He put the patient to bed for a fortnight and had him well massaged. The apex of his heart did not come in more than half an inch. Ever since the patient got up he had diligently persisted in the system of climbing the Brighton hills. Last week he came to him (Dr. Broadbent) rather proud of himself as he could then climb 500 feet in altitude and do a six-mile walk on the level daily. That only cost him fifteen guineas, which was cheaper than Nauheim and quite as effective. He had himself been to Nauheim and found it a very bracing climate, and that he believed had some effect in producing the results. He saw several of the cases there, but could not see the wonderful results which were said to be obtained after the baths. He had never sent a patient to Nauheim since. But to think of carrying out the Nauheim treatment in London was ridiculous. In London there was not the bracing air, and one could not expect the same effects. He agreed with Dr. West that the heart must be kept up to work, and it was easier to get a person to do the right amount of work by walking up hill than by having to go through expensive exercises and baths in Nauheim.

Dr. ALEXANDER MORISON said the Society should feel much indebted to Dr. Goodhart for having introduced the subject in the spirit in which he had done so. He had said there were at least three kinds of "John," using an expression of 'The Autocrat of the Breakfast-table' to denote varieties of cardiac failure. There was the "real John," which nobody doubted, and which was connected with gross disease. There were also other "Johns," which were not so easily identified. He thought Dr. Goodhart had rendered a public service in having introduced that debate in the way he had done, because, for almost a generation, the public had learned too much about their hearts, dilatation, strain, and so forth. One had only to sit next to a man at dinner to learn, at times, that he had broken down and had to go somewhere because of his heart's condition, and curiously, in some instances, that his wife had also broken down at the same time and had to undergo treatment for the same purpose. Or a robust candidate for Parliament was informed that he must abandon the contest and have his heart toned up or bad consequences would ensue. He did not wish unduly to praise the optimist; one had to hold the balance even. It was possible to be too optimistic. There was a "John" who required to be very carefully scrutinised to determine whether he was or was not the "real John." This was difficult at times. Speaking of the signs of dilatation, he agreed with what had been said, that to observe well-marked dilatation suddenly arise in an organically sound heart was rare. It nevertheless occurred. Not long ago he had medically to supervise a case which had to be operated upon, and the heart was perfectly sound. The anæsthetic was given carefully; the patient went through a tedious operation for the removal of gall-stones, with success. The gentleman whose case it was was in the country at the time and a friend took his place. At that stage of the case the patient did well. A week afterwards the practitioner returned and wrote to say he would like him (Dr. Morison) to examine the patient's heart again, because he had been told it was asserted to have been sound, and if that were so he had never examined a diseased heart. When he saw the patient she had a loud mitral murmur with every characteristic of organic disease, but it passed off. He thought one could not argue from the right to the left heart. In fœtal life the right heart had a very important function. It was the great driving chamber. In aërial life it had ceased to be so. The lungs did much dextrocardiac propulsion through acting only once in every four pulsations. The aspirative action of the lungs helped the onflow of blood much. The right heart was built to yield, and the tricuspid valve was made to leak. He was glad to hear recalled in this connection the name of a man, who died all too young, but who so impressed his generation that his memory was still green at Guy's, namely Dr. Wilkinson King. He recognised the safety-valve function of the tricuspid. But Dr. Morison thought the notion was older still, as John Hunter entertained a similar view of the function of the tricuspid valve. For this reason many dilatations of the right heart and movements of the apex to the left might be regarded as largely negligible. The movement of the apex to the left was, in such circumstances, a push of the apex by the more or

less distended right auricle, as Prof. Keith had pointed out. There were many cases in which the apex was well within the nipple line, which were, nevertheless, seriously ill. If there was mitral constriction and a much dilated left auricle, one frequently found the apex was in a fairly normal position so far as the geography of the chest was concerned, but in which, nevertheless, the condition of the patient was serious. When one came to consider the value of signs of dilatation, any signs affecting the *mitral* orifice had much greater importance than those affecting either the right ventricle or the tricuspid orifice. So that one was always likely to encounter the "real John" when there was evidence of mitral reflux, particularly if it had the character of constancy or persistency. Remembering the thousands of hearts one had examined in out-patient and other practice, one could say that the vast majority of them had a standard persistency of normal action, and when a heart presented any deviation from this standard it needed to be scrutinised carefully. Cardiac dilatation and heart-strain were, therefore, not common. When abnormal action affected the left ventricle, and one remembered the organisation of its large anterior mitral flap and the great papillary claws fixed on it, while every arrangement was made to prevent reflux into the left auricle, one had to attribute considerable importance to any evidence of leakage through the mitral orifice. He did not understand that it was intended to discuss the whole subject of cardiac failure and its treatment, and did propose doing so. While, moreover, he was tempted to say a few words on the second head of Dr. Goodhart's paper, namely, the possibly neural origin of certain disorders, he thought that in doing so one was on territory which would require not only one but several evenings for that topic alone. He regretted not to observe any myogenic stalwarts present. But if the Society could arrange a meeting at which the neurogenists and myogenists could meet thoroughly to discuss that subject, an interesting evening could be promised. On the present occasion, while he desired to associate himself wlth the heresy (from the point of view of some) which had been suggested by Dr. Goodhart, he thought that gentleman would agree with him that to discuss the subject at this stage of the debate would only be to touch the fringe of the matter.

Sir JOHN BROADBENT said he was delighted to hear Dr. Goodhart denounce the fetish of cardiac dilatation, especially as applied to the estimation of the area of cardiac dulness in relation to the left ventricle. Many people thought that when they had heart trouble to deal with it was necessary only to percuss it out and say "cardiac dilatation" and they had done everything that was necessary, whereas very often it was of minor importance in comparison with other physical signs. In many cases it was impossible to percuss out the area of cardiac dulness accurately, *e. g.* in very fat people and when the lungs were emphysematous. To say that a heart, because the area of dulness extended a little beyond the normal, was dilated and required treatment was absurd. The athlete would have a much better developed heart and a larger area of cardiac dulness than the sedentary bookworm. It was not the relative amount of cardiac dulness which was the important thing, but the functional capacity of the heart. A man might have a heart with a larger area of dulness than normal which functioned better than a weakly man's heart of an approximately normal area. In aortic regurgitation there was a certain amount of dilatation, followed by hypertrophy, which was an essential condition of the compensatory change. In cases of diphtheria, on the other hand, in which it was well known that heart failure was especially to be dreaded, as a fatal syncopal attack was liable to occur without any warning. there was frequently no appreciable increase in the area of cardiac dulness. When one came to deal with a large number of cases one found that it was not the relative size of the area of cardiac dulness which mattered so much, but the functional capacity of the heart which was all important, and in most instances he thought that could be better gauged, not by simply percussing out the area of cardiac dulness, but by careful observation of the pulse frequency and volume, the departure from normal in its rhythm, the general character of the heart-sounds, the position and character of the apex-beat, and the presence or absence of symptoms of cardiac origin, such as breathlessness and pain in the præcordium on exertion. He agreed with Dr. Morison that it was extremely important in connection with the subject under discussion to define what was meant by cardiac dilatation. In speaking of it, we should define whether dilatation of the right side or of the left side of the heart was referred to. Displacement of the apex-beat might be due to hypertrophy or dilatation of the right ventricle, or to hypertrophy or dilatation of the left ventricle, or the resultant of changes in both ventricles. In these conditions one had to consider whether there was a satisfactory cause for the changes in the size of the cardiac cavities, *e. g.* as

compensation for valvular lesions, or whether the heart had given way for no apparent reason. In the case of severe exercise, such as running, when a man began to exert himself the right ventricle for the time being was temporarily engorged, and possibly in some cases a systolic tricuspid murmur might temporarily be present, but equilibrium was soon established with a healthy heart. But if a murmur was generated at the apex, a mitral systolic murmur as the result of exercise, it was a more serious matter and indicated some dilatation and a relaxed condition of the left ventricle. The underlying cause for all conditions of dilatation, as pointed out by Mackenzie, was loss of tone of the cardiac muscle. That might be due to toxæmias of bacterial origin, and an extreme degree of it could be seen in the dilatation accompanying acute rheumatism, especially pericarditis. In this affection there was often, in addition, serious damage to the cardiac muscle. Loss of tone in the cardiac muscle might also be induced by other toxins, *e.g.* ptomaine poisoning or prolonged and excessive abuse of alcohol and tobacco. It might also be part and parcel of a general loss of tone throughout the muscular system, associated with a relaxed condition of the circulation, sometimes attributable to prolonged overwork and anxiety or severe nervous shock. He had seen cases of that sort. One patient was a solicitor who had been very hard worked and had a very anxious time. He discovered that when he went to play his usual Saturday golf he became breathless and soon tired. On examining he found the patient had a systolic mitral murmur with some displacement of the apex-beat outwards, but no marked dilatation of the right side of the heart, and that the systolic murmur was greatly intensified when he walked about rapidly for a few minutes. Another similar case was that of a man who had severe concussion of the brain due to an accident and had to be trephined for a depressed fracture. He had been a very active man, and when he got about again he found he could not take his usual exercise, but became very breathless on moderate exertion. Here, again, was the same condition, a displaced apex-beat and a systolic mitral murmur. In both those instances he suggested that the patient should go to a bracing place and take it easily for three or four weeks, and then commence exercise on a graduated scale. After three months of this Oertel treatment, he found that in both cases the patients were well and the murmur had disappeared. He thought the loss of tone of the cardiac muscle accounted for the dilatation of the left ventricle, but whether the murmur was due to the general dilatation or could be accounted for merely by a relaxed condition of the circular muscle surrounding the mitral orifice and loss of tone in the musculi papillares it was difficult to say. In the case of a patient recovering from a severe toxæmia such as that of typhoid fever, or rheumatism, it was, of course, advisable that he should not take active exercise until his heart had had time to recover, as in many of those cases there was more than a relaxed condition due to loss of tone; there was often definite and serious damage to the muscular fibres. In a large number of cases, symptoms such as præcordial oppression, pain, tachycardia, and various disorders of rhythm, often attributed to cardiac dilatation, were not due to that, but were, as was pointed out by Dr. Goodhart, more commonly attributable to reflex irritation from various causes of an excitable heart in neurotic people.

Dr. CHARLES W. CHAPMAN said he was not one of those who did not believe in percussion. He found it very valuable, and it enabled one to form a very good idea of the size and shape of the heart. He did not mean that it was of such vital importance as some authorities said it was, but if the percussion were done lightly, and the observer had something which was necessary, namely, a musical ear, it was astonishing what could be made out by percussion, and it was a means which should certainly be made use of. The prognosis of a case of dilatation or strain of the heart resolved itself very much into a question of age. If the person were young and healthy the dilatation was not of very much consequence, provided it did not persist any great length of time, and a little rest put matters right. But in people who had turned middle life the results of a small strain might be serious. Such people often said they had never previously been ill. A gentleman was at a health resort, and one night he danced a great deal, and afterwards was taken with severe pain in the heart. He went to his hotel, where a doctor saw him, and said there was no real trouble with the heart, but it had been a little overdone, and that if he would have three weeks' treatment with the baths he would be all right. The patient said he must go home first, and on the way to the train the pain recurred. When he reached home he was much worse. A week later he (Dr. Chapman) saw him in consultation, and his heart was tremendously dilated, and he had blocking of both lungs. Three days later he died. The serious condition of this patient was not recognised in the first instance. He saw very many nervous cases, and at his hospital there was a favourite prescription of his for cardiac trouble in which there was arrhythmia without physical signs of disease, but plenty of cardiac symptoms present, namely, valerian, assa-fœtida, and bromide of sodium. The number of such cases which became well on that treatment was remarkable. He put it to his hearers as an easy way to distinguish between nervous and organic cases to submit them to this simple treatment, and if they did not respond they could be

regarded as probably organic. Another point which he had not heard touched upon was in regard to the cases which occurred in middle and advanced life, and which were difficult of explanation on the usual lines. In many of those cases one found on investigation that there was a history of syphilis. The association of syphilis with many such cases was not, he thought, sufficiently recognised. He had seen many cases of that kind, and if they were recognised early and treated they could be improved very much. They had sternal dulness of varying intensity, dilated aorta, and thickened vessels, and they would often deny that they had had syphilis, but careful inquiry elicited that they probably had had that disease. If they were allowed to go about without treatment they would get worse; but if they were put to bed and thoroughly treated it was wonderful how well they would oftentimes do. With regard to children and young people, he had seen many cases which had been condemned to months of rest in bed but without sufficient reason. Cases of sudden death, to which Dr. Collier referred, in people in advanced life with no apparent cause for their heart trouble were often explicable by the previous existence of syphilis in the patient.

Dr. LEONARD GUTHRIE desired to thank Dr. Goodhart for his able introduction of the subject. He had never sat at Dr. Goodhart's feet without gathering crumbs, or, more correctly, loaves of knowledge from his table. He was in agreement with everything which Dr. Goodhart said with regard to the physical signs to which he referred, and did not think that they could be sufficient alone for the diagnosis of cardiac strain and dilatation. Yet there were cases of young people with a palpitating irregular heart, such as Dr. Goodhart described, with displacement outwards of the apex, with the presence of systolic bruits at the pulmonary orifice or at the apex, conducted towards the orifice, and when one met with those physical signs one was justified in thinking that although they might not be evidence of dilatation they might be evidence of past strain, and possibly dilatation on a previous occasion. Because at one time a physician examined a heart without finding signs of dilatation, he did not think it was right to assume that no dilatation existed before. He thought sometimes physicians, when they could not find dilatation themselves, were apt not to give credit to the observations of others who saw the case before and pronounced it to be one of dilatation. With regard to the symptoms, he agreed again with Dr. Goodhart. The symptoms which he described were not those which were commonly met with in cases of "genuine Johns," but they were nevertheless cardiac symptoms. He did not think there was justification for saying they were merely due to neurasthenia. He wished to raise the question whether, when those symptoms were cardiac, one was justified in telling the patient that he need not trouble about his heart. If the physician cheered up a patient in that way, he was apt to take the physician at his word, and do something very foolish in the way of exercise, indulging in over-exertion, such as climbing Swiss mountains or bicycling up-hill, with results that might be disastrous. With regard to the causation of cardiac strain and dilatation, he thought that influenza and over-exertion following it might be given first place. The majority of cases of the kind had been those which followed influenza, the patient getting up too soon and exerting himself too much. With regard to treatment in general, he agreed that rest and quietude were occasionally overdone, and as Dr. Charles Chapman had truly said, there were many children and young people who were kept much too long at rest in bed on the supposition that they had suffered from heart-strain and dilatation. Yet when there was a recent history of something which pointed to sudden strain and dilatation of the heart the physician should be careful, and in such case complete rest, mental and physical, should be imposed for at least a month. After that carefully graduated exercises should be enjoined, but all violent physical exertion should be prohibited, at all events for a time.

Dr. F. PARKES WEBER said that when the circulation of blood tended to be more or less insufficient, either from obstruction or from insufficiency of the driving power, there were two "vital" reactions which might take place: (*a*) An increase in the driving force of the circulation, associated with hypertrophy of the heart, with or without dilatation; or else (*b*) an alteration in the quality of the blood, the blood becoming "thicker" and richer in red corpuscles, *i. e.* in the oxygen-carriers. In some chronic cardiac and cardio-pulmonary cases, not only might there be a distinct increase in the number of red corpuscles, but the total quantity of blood in the body might be increased. It was nearly always the right side of the heart which was affected in these cases. Amongst these cases the best known were those of congenital pulmonary stenosis, in which the number of red corpuscles sometimes numbered ten millions, or even more, instead of the normal five millions to the cubic millimetre of blood. But, apart from congenital heart disease, great polycythæmia was not very rare in middle-aged persons with chronic cyanosis of cardio-pulmonary origin. Dilatation of the heart secondary to chlorosis or some form of anæmia (or due to the same causes which produced the anæmia) was, of course, well recognised. Sir Richard Douglas Powell also said that when there was strain of the heart from prolonged and excessive muscular exertion he noticed that there

was often a certain amount of anæmia. That was a very interesting point, and he would be glad to know whether that observation was confirmed generally, and if so, whether it was likely to be due to some auto-intoxication by fatigue-products from the muscles. The methodical study of blood-counts in all cases of chronic cardiac strain and cardiac dilatation (left-side and right-side cases) would be very interesting.

Dr. GOODHART, in replying on the discussion, said there was not much which he need say. Having opened the discussion and put forward his several points, which seemed to require stating, he had since been sitting at the feet of the many Gamaliels whom he saw around him. And the Society might congratulate itself, though it was hardly his business to say so, on the excellent discussion which had gone forward and the number of authoritative people who had been addressing the Society during the meeting. He had been exceedingly interested to hear what Dr. Collier said from his point of view, for he studied athletic people in their home, and he gathered from Dr. Collier, and from other speakers also, that a true heart-strain in a healthy heart was comparatively rare. In putting his particular point of view, which he pointedly did because he thought it wanted discussing, he did not suppose or mean to suggest that heart-strain was a condition that did not occur, or that the subjects of it were all neurasthenics. He wanted to bring out specially from those who had experience of that kind as to how often such conditions occurred, and from what Dr. Collier said it seemed that he thought that it did occur more often than he (Dr. Goodhart) had thought. With reference to the signs and symptoms which were supposed to indicate it, he would only speak as to his own feeling in the matter; he did not attempt to criticise Dr. Collier or anybody else to whom attached the responsibility of looking after the condition of young men who wished to engage in excessive athletics. When he had to decide whether a person ought to be allowed to row or not, he was probably more careful and prone to say, "Do not row," than Dr. Collier would be, and he thought the rule Dr. Collier gave—that if, under certain circumstances, a bruit were found to develop and there were an epigastric impulse—it was prudent to prevent such men rowing. It did no harm at all to say that. To such a man one would say, "I do not consider that rowing is your sport, and you had better take to things which do not cause such strenuous exertion." That did not make a man nervous or cause him to think unduly of his heart. With reference to it being hardly fair to pronounce an opinion upon whether a man should row or not until the heart was seen in a condition of active exercise, he thought that was fair and proper for a man who was engaged in looking at the athlete. But still, he had been in the habit of doing that very largely, making the man or boy run upstairs to see what the heart's action was like. But he had rather given up that practice of late years because he had heard such curious things under those circumstances, sounds which were obviously independent of any disease, so that now he was still more disposed to listen to a heart when the man was sitting quietly or lying down, or was altering his position in some way. He thought listening under those circumstances and looking at the chest was often better than examining the patient after violent exercise, unless many allowances were made. If he were in Dr. Collier's place he would do everything which could help to form an opinion. With regard to dilatation of the heart, there, again, the point which he wanted to put before the Society, and which most of the speakers had fully realised, was that for some years now there had been a great deal too much talk of dilatation of the heart. He believed there were hundreds of people going about the country now nursing their hearts, and yet one could not say there was anything the matter with them then or that there ever had been. There were certain disturbances, as Dr. Guthrie had said; they were heart symptoms, and they were symptoms which were called into existence by the nerve centres of the heart, but though the worst cases of that kind might ultimately become serious states of heart disease, yet such were very often to be attributed to the enervating treatment which was adopted from the commencement. And his feeling was that when the Recording Angel came to put down the history of the last twenty-five years he would have much to say against the Nauheim treatment. It seemed to him that a large number of people were somewhat inconsiderately packed off to Nauheim, and when there they were told and shown that their heart was largely dilated, with the result that when the patients came back their hearts were firmly fixed in their minds, and thenceforth there was but one disease and one treatment for them. He wanted to elicit the feeling of the Society on that point. Not a week passed without his seeing one of those people, who came because of a dilated heart, but in whom it was impossible to find anything the matter with them, and where they must have gone through a serious and long treatment unnecessarily. If the physician could succeed in keeping the heart out of the patient's mind it would be far better, and in doing that one was doing the best possible to get the patient well again. The subject was such that it was almost impossible to discuss it fully at one meeting, or even two. As Dr. Alexander Morison said, "it opens up the myogenic and neurogenic causes of heart failure, and it would not be a bad idea to have a discussion between the opposing factions," but he did not know that he would come on that evening.

February 6th, 1911.

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WITH SIR WILLIAM BENNETT AT THE SEAMEN'S HOSPITAL, GREENWICH.

A CASE OF TRAUMATIC HYDROCELE AND INJURY TO THE PERINÆUM.

HERE is a boy, æt. 15 years, who three weeks ago fell astride a bar. He suffered a good deal of pain in the perinæum at the time, and the first act of micturition afterwards was painful. He continued his work, suffering pain, but without much swelling so far as he knows. For three days micturition was painful, but no blood was seen in the urine; on the fourth day he found that the scrotum was beginning to swell, and the swelling increased until it was of considerable size; with the growth of swelling the pain also increased a great deal, as might be expected. It is clear that he had a somewhat severe injury in the first instance. He bruised the scrotum and probably the testicle, and to some extent also he bruised his perinæum. But no very great inconvenience was felt until three or four days afterwards, when the scrotum became swollen and red. This swelling in the scrotum was clearly due to the rapid effusion of fluid, so that he had what is often called traumatic hydrocele. You must be careful in such cases to distinguish between acute traumatic hydrocele and hæmatocele. Hæmatocele comes on much more immediately after the injury, and it is associated generally with some bruising and discoloration and more tension about the parts. Hydrocele comes on, as a rule, after an interval, in the way this boy describes. Following the ordinary course of events the fluid began to be absorbed, and little now is left; finally this will disappear, leaving a little induration, not about the testicle, but about the epididymis—a rather important point. It is well recognised by those who have had much experience of these things that when a sequence of

events like this occurs, leaving an indurated epididymis, it is very likely, supposing the patient to be of a low resistance to tubercle, that tuberculous disease of the epididymis will follow. It is the vulnerable part of the testicular apparatus to tubercle—a fact never to be forgotten. In a case of this kind, therefore, it is particularly important to be sure that the cure is complete before anything like heavy work is resumed. The question of tubercle is important from another point of view. In this case the violence of the injury was sufficient to account, in any patient, no matter how healthy, for the symptoms which followed; should symptoms of the same kind, however, follow upon *slight* injury in any subject between the ages of fifteen and twenty-five, you should have in mind the possibility of the previous existence of a latent nodule of tubercle—a thing more common in the epididymis than is sometimes thought.

An interesting point arises in connection with the slight injury to the urethra which occurred in this case. The patient had a blow on the perinæum, which was followed for two or three days by some pain during micturition; the urethra was therefore involved in the injury. The cardinal sign of damage to the urethra in a case of this kind is bleeding, and in such cases you must be prepared for the occurrence of traumatic stricture, one of the most intractable forms which is met with. It must, however, be remembered that, even in some of the very slight cases of injury to the perinæum, in which there is no hæmorrhage, if the passage of urine after the injury has been at all painful for a period, the damage to the urethra is sometimes sufficient to cause traumatic stricture. In what percentage of these cases stricture occurs I cannot say, as it is impossible to obtain any reliable statistical information. But I have had to do with a considerable number of patients who had falls of this kind, producing no sufficient injury of the urethra to cause bleeding; that is to say, cases in which there was neither gross interruption of continuity of the mucous membrane of the urethra nor sufficient bruising to cause exudation of blood, in whom tight traumatic strictures formed notwithstanding. The matter is of some account in relation to the almost automatic conclusion which is commonly arrived at when dealing with a stricture that it must be the result of gonococcal infection, although the patient may stoutly deny having ever had gonorrhœa. In such cases, before finally deciding to discredit a patient's story, it is better to go into the question of even a slight injury, because such an injury may be followed by a very tight traumatic stricture. Reverting now to the question of hydrocele, it is not common for a traumatic hydrocele to persist indefinitely, but should it do so, it may be taken as nearly certain that some chronic inflammation of the testicle or epididymis remains uncured. In passing, let me say a word concerning the treatment of ordinary vaginal hydrocele. In these days of "radical" cures by operation it is sometimes forgotten that the ordinary hydrocele can be cured, probably in two cases out of every five, by tapping and injection, the best injection in my experience being Morton's fluid. Recently the cure of vaginal hydrocele by auto-serotherapy has attracted attention. It has for a considerable time been recognised that disappearance of pleural and abdominal serous effusion may follow if a portion of the effusion in either case, say about a drachm, be injected subcutaneously in some other part of the body; the same effect in hydrocele has followed the injection into the thigh of fluid from the tunica vaginalis. By this method another alternative to the radical cure by operation seems to be provided, by a means from which, if proper care be taken in securing asepsis, no harm seems to arise, whilst it is one which can be used in circumstances which would render the radical operation impracticable.

A Case of Greenstick Fracture.

This little girl, who was going to school on Monday (four days ago), fell down with her arm bent under her. I want you to notice the shape of the forearm. Just above its middle the forearm is bent at a considerable angle. There is no crepitus, and in a child of that time of life you would not necessarily expect crepitus, and here a greenstick fracture accounts for the deformity. It will be seen by this X-ray picture that the ulna and radius are bent at the same level: the bend in the one bone is exactly opposite that in the other. There is no interruption of continuity, and the injury has been caused by direct violence; the exact correspondence in the situation of the injury in the two bones shows this. We have now to decide upon the best treatment to adopt in this child's case. It can easily be seen that if the deformity is allowed

to remain as it is, pronation and supination must be very seriously restricted. We have, as a matter of fact, to get these bones as straight as possible. In a child of this age this can be done under an anæsthetic by gently straightening out the forearm and moulding the bones into their normal position. In doing this, unless you are careful, you may convert the greenstick fracture into a complete one, a thing to be particularly avoided in the case of a fracture of both bones of the forearm. Sometimes you will find it is practically impossible to reproduce the proper position and shape without bringing about complete fracture; in such a case you will be wise, unless in spite of your attempts the deformity is strongly marked, to be content with doing the best you can, because these greenstick fractures have a very decided tendency to straighten themselves out spontaneously with the growth of the child up to the age of ten, and in girls up to the age of twelve, after which the tendency to spontaneous straightening becomes less year by year during the period of growth. This tendency to the spontaneous straightening of bent bones in growing people is very interesting; it is well seen in rickets, and it largely explains the small number of grown-up people seen walking in the streets with bent, rickety limbs compared with the number of children seen showing marked evidence of this kind of deformity. The same tendency in fractures makes it possible, indeed, sometimes almost easy, to restore the normal contour *gradually* by massage and manipulation, if an intelligent person, who really understands what massage, etc., really means, is available. In the case of the forearm it is not desirable to rely too much upon this tendency to spontaneous straightening, because even a slight bend in the bones may interfere greatly with rotation. Greenstick fracture, therefore, of both bones of the forearm should, if the deformity is great, be straightened by manipulation, even at the expense of complete fracture. If one bone can be left with its continuity uninterrupted, so much the better; especially is it desirable to leave the radius not completely broken on account of the tendency to displacement of the upper fragment and the difficulty in manipulating it in consequence of the mobility in its upper connections. In young subjects by far the best position of the forearm in these cases is supination and flexion; this is not what is commonly taught, but it is a cardinal principle in the treatment of fractures that displacement of fragments is least likely to occur when complete relaxation of muscles can be attained, and in the forearm supination with flexion attains this object most effectually. Of course, if the X rays are immediately available, and a complete fracture is being dealt with, they will be used, and such a position of the part adopted as will leave the bones in the best relations; but if the help of the X rays is not available the position which I have mentioned will, in the long run, in young subjects, produce the best results in forearm fractures, Colles's fracture being, of course, excepted. Massage and passive movements can hardly be commenced too soon. Of operation in complete fracture in cases like these I do not propose to speak; it is outside my present purpose. The one thing to be avoided in a case like this child's is the placing of the part in any form of splint—of plaster or otherwise—which cannot be immediately removed at will. You will note that at present the child shrinks a little from the gentle massage and movements which I am using; soon she will welcome the treatment as much as she fears it now, and her recovery will be immensely expedited by it.

A Case of Varix of the Abdomen.

The next patient, a man getting on towards middle age, you will see has a peculiar condition of his abdomen. Ten years ago he fell from a height into the hold of a barge and was laid up for eight weeks; he recovered, and so far as he knows was none the worse. Nothing showed on the abdomen at the time of the accident. Four years later he fell from a ship's funnel, which he was painting, on to the deck, and was laid up for seven weeks. Gradually after this the swelling which you now see on the abdominal wall commenced, and steadily increased to its present size. Two years ago, without apparent cause, he was for a long time invalided on account of painful swelling of both lower limbs, and since that time he feels tired and weak after a comparatively small amount of work. At the time of the second accident he damaged his head badly, and he still at times feels giddy and sick when lifting weights, and when doing anything entailing exertion in the stooping position; on this account he has been an outpatient at various hospitals on several occasions. The abdominal condition is what I at present

wish to direct attention to. On the anterior abdominal wall surrounding and obliterating the umbilicus is a large, lobulated looking mass, which passes down and fades to nothing as it approaches the right groin. In the erect position it is large, full and tense; when the patient is lying down it diminishes in size, but still remains prominent. Its contents can be squeezed out of it as if it were a sponge and it refills immediately pressure is removed. When emptied a considerable hole can be felt at the umbilicus through which it seems to come. There is a strong expansile impulse on coughing, and with the impulse is a very distinct, almost coarse thrill. He states that he was told by one medical man whom he saw that the thing was a rupture which might require an operation. Probably the aspect of the swelling has changed since then, as it now has none of the characteristics of a hernia excepting the expansile impulse on coughing and the hole in the abdominal wall. The thrill felt with the impulse of course at once puts the question of hernia out of court, whilst it is characteristic of varix if there is an impulse on coughing, the two things together indicating entire absence or incompetency of the vein valves. At first sight the condition looks dangerous, it is so large and tense; the man is naturally alarmed at it and wants to know if it cannot be cured by operation. In all these cases of varix there is one thing beyond all others which must be borne in mind, and it is this: operations upon dilated veins which are acting as collateral channels are sure to be followed by a recurrence of varix in the immediate neighbourhood. That in itself does not always necessarily negative the propriety of operation, because there may be a condition either of thrombosis or of thinning of the vein walls to such an extent that they appear to be on the point of bursting; or there may be pain which is sometimes very acute in varix in consequence of irritation of the peripheral nerve-ends. Either of these complications may make operation justifiable to relieve the symptoms, but not for the purpose of curing the varix. There is a great difference between operating in a case of varicose veins or any other condition for the relief of symptoms arising from it and operating for the purpose of curing the disease itself. Nothing in the shape of operation or anything else will cure the condition of varix from which this man is suffering, because these dilated veins are active collateral channels; hence, if any or all of them are removed others must form in the neighbourhood to take up the work. Supposing, therefore, circumstances of complication render an operation upon varicose veins necessary or desirable when the vessels are acting as collateral channels, it is most important to warn the patient that the sole object of the operation is the relief of symptoms and not the cure of the disease. Excepting the complications already mentioned one other condition alone remains, which justifies operation when the varicose veins are playing a part in the collateral circulation, and that is seen when one or more of the abnormal veins are situated in positions very liable to injury, *e.g.* over the shin-bone; in such cases the removal of a vein dangerously placed may be followed by a recurrence in a safe, or at all events, less dangerous spot. For the proper decision as to the propriety of operation in cases of this kind it is, of course, necessary to differentiate between dilated veins which are still serving as active collateral channels and those which, having served their purpose in this respect, are no longer necessary for the carrying on of the circulation, and this is not difficult. If dilated and tortuous veins which are prominent and tense when the patient is standing up become, immediately the flat position is assumed, empty and flaccid, disappearing for all practical purposes, leaving only the slight peculiarity in the skin along their courses, these vessels may be held to be free from proximal obstruction and to be taking no active part as collaterals. They may therefore, be dealt with by operation without recurrence necessarily following. If, on the other hand, instead of disappearing in this way they empty slowly and partially, still projecting as obvious vessels which can be emptied only by pressure of the fingers or otherwise, as happens in the case of this patient, it is quite certain that there is either direct obstruction to the flow of blood in them or that they are taking an active and essential part in the collateral circulation, and further, that their removal will therefore be followed very soon by the appearance of other similar vessels which have to take up the work of those which have been removed.

A Case of Lateral Bend in the Spine.

The last case for consideration is mainly interesting because it affords me an opportunity of showing

how easy it may be in searching for an uncommon cause to entirely overlook the obvious.

This boy, who is 17 years old, has been under treatment for what is stated to have been hip-joint disease, and after many months of rest, etc., has been cured with an excellent result, inasmuch as he has free movements in all directions. You will, however, see from the radiogram of the hip that the case was not one of hip disease, but of separation of the epiphysis of the femoral head, which is not uncommonly mistaken for hip-joint disease supervening upon injury.

It will be noticed that although the boy walks well and strongly he limps a little, and upon stripping him you see that he has a well-marked lateral bend of the vertebral column in the dorsi-lumbar region, the convexity of which is towards the damaged hip. The bend is a pure lateral deviation; there is no inclination to rotation, and the spine is perfectly supple. The limp and the bend are clearly due to the same cause, viz. a shortening of the damaged limb of about three quarters of an inch. By placing a book of a thickness equal to the shortening of the limb under the foot the bend in the spine entirely disappears, and the attitude is erect and natural.

In the diagnosis of every case there are certain cardinal points which cannot be ignored, no matter how skilful a diagnostician the practitioner may be. In all cases of lateral curvature of the spine the one thing in arriving at a diagnosis, which should never be omitted, is to ascertain whether there is any difference in the lengths of the two lower limbs. It is, I suppose, a sufficiently well-known fact that a difference in length in the lower limbs of the same individual is not very uncommon; the difference may be so slight as to be altogether unnoticeable, or it may, on the other hand, amount to as much as half an inch or even more. If this degree of difference is reached a lateral bend in the spine is unavoidable when standing with both feet planted well on the ground, the bend being always capable of correction by raising the heel of the shortened limb to a degree which is equal to the amount of its shortness.

All this is so elementary and obvious that I am only induced to mention the matter now by the occurrence of the following case, which has recently come under my notice:

A robust-looking man, æt. about 23 years, who was said to be a keen footballer, hockey player, etc., in fact, a good all-round man, was brought to me for advice concerning a lateral bend in his back, which had been discovered quite accidentally. My opinion was sought in consequence of some want of agreement between those whom he had previously consulted with regard to the cause and treatment of his trouble. In view of these differences I was desired to give my opinion without having been previously given any particulars of the previous consultations.

On stripping the patient he was seen to be as good a specimen of a well-developed man of his age as could generally be seen. There was, however, a distinct lateral bend in the dorsi-lumbar region of the vertebral column. There were no subjective symptoms of any kind; he complained of nothing and was conscious of nothing abnormal. There was no tendency to rotation, and the column straightened out completely on bending the body forwards. On measuring the lower limbs, that on the side towards which the convexity of the bend pointed was found to be rather more than half an inch shorter than the other. On standing the patient up and placing a book of the thickness mentioned under the foot of the shorter limb all signs of the spinal bend disappeared, and the man stood "fair and square." My opinion was therefore to the effect that the cause of the bend was the unequal length of the lower limbs, which could be readily corrected, if it were worth while, by raising the heel of the boot on the short side for about half an inch.

I was then assured that the question of a difference in the length of the lower limbs had not been previously raised, and that at no previous consultation had any comparative measurements of the lower limbs been made. One of those consulted had advised a course of Swedish exercises, which had been sedulously carried out for about two months without any obvious result. The opinion given at the consultation which preceded mine was, it was said, that constipation was probably the cause of the trouble, although the patient declared that he was unconscious of it, and, indeed, was a man of most regular habits. A course of medicinal treatment was prescribed to rectify this trouble, and it was stated that should this fail to bring about the desired change it might become desirable to consider the question of a serious operation. There may, of course, have been something in this case which I entirely failed to grasp, but even so, it is, I confess, a little difficult to follow the logical reasoning which could lead to the suggestion of an operation for the cure of a lateral bend in the spine, which was producing no discomfort or disability, and which could be entirely corrected by elevating the heel by something like half an inch.

February 13th, 1911.

SYPHILIS IN CONSULTING PRACTICE.*

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It is the fortune of our profession to meet at times with humour of a grim kind. As a human person, the doctor is, in the popular view, a being full of kindness and pity. Yet the doctor as a human being can extract humour even from so grim a disease as syphilis. Such is indeed the case! Syphilis, if it gets a favourable opportunity, can be a terrible mirror of the morals of society. For instance, there was a terrible epidemic of the disease consequent on the return of the sailors of Columbus from America. From "sailor society" the disease spread to higher ranks, and overflowed the confines of Spain. It has even been said that every human being is congenitally syphilitic in degree! So widespread is the disease that at least one of our ancestors cannot have failed to have acquired it, by fortune or misfortune. The humour of this statement is grim in the extreme.

With regard to syphilis in consulting practice, for practical purposes it is only the unusual which, so to speak, passes the practitioner and reaches the consultant. And before I begin to illustrate such cases I would speak of a general condition which so modifies the syphilitic manifestations that they are liable to be misunderstood. I mean the addition of sepsis or relative asepsis, cleanliness, to the manifestation. This is very important, as cleanliness may be found in poor surroundings, and uncleanliness where there is no excuse for its existence. This factor of uncleanliness or septicity means that an additional infection is grafted on to the original. For instance, the hard primary sore underneath an œdematous prepuce and accompanied by a multitude of enlarged glands in the groins, such as is so often seen in hospital practice, are unusually seen in practice amongst the well-to-do, and practically never seen in consulting practice. Indeed, in a more or less aseptic condition, a primary sore may be nothing more than a granulating spot which is slow to heal—a condition which is easily overlooked, particularly on the not easily inspected genitalia of a female. Thus a chancre or primary sore is robbed of the chief features by which it has been taught it should be recognised. Indeed, I would go so far as to urge that every granulating sore which is slow to heal, and particularly when on a part liable to infection, such as a doctor's finger, should be suspected of being a primary syphilitic sore and examined for the *Spirochæta pallida*. Since it has been proved that the finding of the *Spirochæta pallida* in the secretion of a sore shows its syphilitic nature, a search should be made for this organism in the discharge of a suspicious sore. It so happens that at the present time such an examination is not advised until the patient has been referred to a consultant. There is no reason why this should be so, and it would be better for the patient if it was done by his general practitioner. By such means I have proved the presence of chancres on the finger, nose, tonsil, skin, and inside the urethra, not to mention other unsuspicious situations in unsuspectable people. But it is not always wise to prove the nature of such a disease in every case, provided the patients are treated and others are protected from the disease. To have the presence or absence of the *Spirochæta pallida* proved is simpler and less expensive to the patient than to have a Wassermann blood reaction done, the presence of which cannot be expected until after the secondary stage has commenced.

To recapitulate, a septic infection of a primary sore may so mask its nature as to render it and its bubo unrecognisable; its nature may be suspected from its slowness in healing; its nature may be proved by the demonstration of the presence of the *Spirochæta pallida*.

In the secondary stage of syphilis it has long been recognised, "as in a glass darkly," that an additional septic infection may make the secondary symptoms and signs very much worse. For instance, it has long been advised that a subject with primary syphilis should have the mouth and teeth attended to by a competent dentist, or the secondary symptoms in the mouth will be much worse. As a criticism on smoking, for the same reasons it has long been advised that the practice should be discontinued. Certain it is that the tremendous crops of secondary rashes and condylomata seen in hospital patients are not often seen in consultant

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practice. Indeed, but for a few transient spots, and perhaps a slight transient sore throat, the secondary stage of syphilis may pass away, and may very easily be unrecognised.

At this stage it is easier to have a Wassermann reaction demonstrated positive than to find the *Spirochæta pallida* in the sparse secondary signs. But just as the primary stage of syphilis may pass unnoticed in the well-kept subject, so may the secondary stage pass unnoticed. In consequence, there is nothing to interfere with the transmission of the disease to any offspring.

In the tertiary stage there is a similar state of things. In hospital practice gummata are usually very obvious and easily diagnosable from their size, shape, the presence of a wash-leather slough, etc. In the less septic people of private practice a gumma is often a shallow ulcer with a well-defined margin, either circular in shape or with sharp margin; its base is formed of red healthy granulations, no slough is present, the sore is slow to heal, and when it does so easily breaks down again and again, unless iodide of potassium has been given.

Thus even in cleanly subjects tertiary syphilis is easily recognisable, and much could be done to prevent the spread of the disease if only all syphilitics had tertiary syphilis.

In the tertiary stage Wassermann's blood reaction is positive, and the *Spirochæta pallida* has been demonstrated to exist in gummata. The latter observation is very important, because hitherto it has been taught that the tertiary stage is not contagious. But if the *Spirochæta pallida* is present it is contagious.

From these observations it will be seen that in the more cleanly classes syphilis can be quite overlooked, and consequently its transmission unchecked and its dissemination unimpeded. The results may be almost a matter of common observation. When we are first qualified we see but little of the disease. As we grow older we see more and more, and we appreciate that it exists untreated in the houses of our acquaintances, friends, and neighbours, and sometimes amongst our own relatives. In this, syphilis is similar to insanity.

Further, it is seen that in the very places where medical students are taught to recognise syphilis the superadded septic factor predominates, making the primary sore hard and un-overlookable, its bubo obvious; the secondary signs many, multiple, and patent; and the tertiary signs glaringly unmistakable. Yet when that student qualifies as a medical man he practises amongst more cleanly and aseptic people, when the primary sore may be easily overlooked, the secondary signs sparse and unnoticed, and only a proportion get gummata; yet it is his business to recognise, treat, and prevent the transmission of the disease—a very difficult task for which the present state of medical teaching makes him inefficient.

Several cases were then detailed illustrating the varieties and difficulties of cases of syphilis brought before the consultant.

Speaking of treatment, instances of the apparent failure of the ordinary "mercury iodide" treatment were recounted. The dangers of optic atrophy and blindness from soamin and atoxyl were then mentioned. An illustrative case was described in which tertiary syphilitic ulcerations of the scalp, which did not yield to potassium iodide, were cured by the hyperleucocytosis produced by intra-muscular injections of phagocytin.

With regard to the new drug, "606," there seemed to be much doubt at present. It was indubitable that it often cleared up the outward and visible signs of the disease in a truly wonderful manner. But what was in doubt was the effect it had on the progress of the disease. Apparently our hopes of having secured a specific drug for the treatment and cure of syphilis were to be abandoned, for "606" practically did not alter the Wassermann reaction, and, so far as could be ascertained, did not cure the disease. Were it safe to prophesy it might be said that "606" would be chiefly of use in the treatment of obstinate syphilis, or when it was necessary to remove outward signs of the disease quickly.

It was a great step in the treatment of a disease to isolate the organism which caused it. With regard to syphilis, its causative organism, the *Spirochæta pallida*, had been discovered. But at present no vaccine had been obtained to protect a subject against the disease, or to cure a patient suffering from it.

All hypodermic treatments seemed to be unpopular with private patients, and were likely to be restricted in their use to the various Services.

In conclusion, it was stated that at the present time no better treatment could be followed than to use "mercury and iodide" treatment by the mouth, as was recommended by the text-books. Pills were often used for that treatment, but were an unreliable form of drug administration, only to be used instead of fluids in cases of extreme necessity.

February 13th, 1911.

A LECTURE
ON
DIAGNOSIS IN DISEASES OF THE RECTUM AND ANUS.

Delivered at the Polyclinic.

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GENTLEMEN,—The subject of my lecture to-day is the diagnosis of diseases in the rectum and anus. First, I want to insist on the importance of a local examination in all cases, and not only that, but also upon the importance of founding the diagnosis entirely upon the findings, and little, if at all, upon the patient's history and symptoms. As an example of the mistakes which may result from not making a local examination, I will recount the following case: A young lady, who for some months had been under the care of a London physician for a constitutional ailment, went to see him one day complaining that she was suffering, she thought, from piles, and that for a week she had suffered considerable pain in the rectum. He did not examine her, but after making a few inquiries, sent her to a nursing home to have high-frequency electricity applied to the rectum. The applications were made several times, but without benefit, and it was then discovered that she had a large swelling at the side of the anus. I was asked to see her, and I found a large ischio-rectal abscess, which on being opened contained a pint of foul pus. That case was not a creditable one.

The account which a patient gives of the history and symptoms is of very little value, and practically none from a diagnostic point of view. One's diagnosis should depend on the examination, and in rectal diseases I believe this to be a sound and correct procedure, as by adhering rigidly to it one is prevented from making mistakes. When a patient refuses to be examined I refuse to have anything to do with the case. Now, although the patient's account of his trouble is of little or no diagnostic value, it is always wise to go thoroughly into the account, and not to hurry him into an examination. It is always advisable on first seeing a patient to spend some time in carefully going into the history and symptoms, because although such information is not of much value, it enables the surgeon during the time so occupied to estimate the type of patient with whom he has to deal and to obtain his confidence, while the patient himself will be able to shake off the not unnatural nervousness on entering the surgeon's consulting room, and he will also feel that the surgeon is taking an interest in his case. For we must remember that although to us the history and symptoms may be of no value, the patient does not think so. The average patient thinks that his account of the symptoms is of the utmost importance; therefore it is well to listen carefully to it. Getting the patient's confidence and allaying his nervousness are as important as making a diagnosis, and therefore they should not be neglected. The patient should be asked what it is he complains of, not what is the matter, otherwise one lays oneself open to the obvious retort, "Well, doctor, that is what I came to you to find out." When the patient has described his complaint, one should cross-examine him with regard to the details, and it is important to ascertain what effect on the daily habits the patient's symptoms have. This will be of use in judging of the gravity of his condition and estimating what treatment one should advise. I can best put forward my meaning by an illustration. I was recently consulted on the same day by two patients, both of whom were suffering from prolapsed piles. One was an officer in the British army, and he told me that when on leave or on ordinary light duty his piles caused him no real inconvenience, but when on manœuvres and obliged to be many hours in the saddle in a campaign, and too busy to attend to himself properly, the piles caused him serious pain and inconvenience. The other patient was an elderly lady, with an almost identical condition in the rectum. She had lived a quiet sedentary life, she had a maid to attend to her, and needed to get about very little. Her piles caused her very little inconvenience, and she consulted me to please her relations, who were anxious about her condition. I told the soldier he should have his piles removed, but told the lady that no operation was necessary. The amount of incapacity which any abnormality or diseased condition of the rectum causes is always a matter of importance, and it has an important bearing on the treatment which we should advise, as also has the

habits and profession of the subject. One should inquire carefully how the bowels act. If the patient says they act regularly, one must inquire whether this is the result of taking aperients or not; the nature of the stools should be inquired into, and whether there are abnormal constituents, such as blood, pus, mucus, etc. When patients say, as they often do, that they pass a large quantity of matter from the bowel, on examination you will often find it is not pus but mucus. Next, we should inquire into the history and duration of the symptoms, and what it was that first drew attention to the condition. One must not allow oneself, however, to be biased by the patient's statement of his condition, nor must we assume that because the symptoms are trivial the disease is the same. I have seen patients who, from their symptoms, one supposed to be suffering from simple fissure, but on examination are found to have cancer, while the reverse is even commoner. The following case illustrates this: The patient was a man, æt. 33 years, who was in business in the Midlands. He had a wife and family, was doing very well in his business. He began to have a little diarrhœa, that is to say, his bowels acted rather loosely twice a day. He also began to get some discomfort in his abdomen. He never had an attack of acute pain in his abdomen, but he had some discomfort, which was more noticeable towards the end of the day. He called in his doctor, who examined his stools and found much mucus and large bits of membrane in the stools practically every day. Abdominal examination yielded no further evidence. His doctor concluded that he was suffering from slight mucous colitis, and treated him accordingly. The symptoms became no worse, and he went on this way for nine months. The doctor never saw any blood or anything in the stools to suggest that he had anything serious. After this had gone on for nine months he came to London to see me, and I discovered with the sigmoidoscope that he had carcinoma, the size of half a crown, in the middle of his sigmoid flexure. He had had practically none of the symptoms which are usual in cases of carcinoma of the sigmoid, and that shows how very trivial the symptoms may be when there is sometimes serious disease.

With regard to examination of the patient, first of all there is the position for examination. This is of some importance. There are two correct positions for this purpose: one is the knee-elbow position, on a low couch. Here is a photograph showing a section through the pelvis from the front showing the relationship of the sigmoid flexure to the rectum. You see the levator ani, and I want you to notice where the peritoneum is reflected on to the rectum showing the loop of the rectum. Normally it is all lying down in the pelvis. Here is another photograph showing the sigmoid flexure and its relationships. Notice the anus, bladder, and symphysis pubis. I want you particularly to notice how much the rectum tips backwards. It tilts abruptly backwards, and later on forwards. This is particularly noticeable when you are using the sigmoidoscope. Here you see the perinæum with the relationship of muscles. Notice the coccyx with the glutei on each side, and the external sphincter rising from the coccyx and passing forwards. And you will notice that the fibres are circular, and they lie parallel to the bowel wall everywhere except at the back. At the back some of the fibres still pass parallel, but the majority pass backwards to the coccyx. That is the reason why ninety-nine fissures out of one hundred are at the back. If, in a dead subject, you stretch the sphincter to the point at which it will split, you will find it will split every time at the back. In women it may be found to split in front sometimes, because the fibres decussate and there is a weakening there. But anterior fissures are excessively rare and they only occur in consequence of injuries. Notice there is a channel in the cellular tissue passing through between the two ischio-rectal fossæ, underneath this muscle (the posterior attachment of the external sphincter), and that is why you get ischio-rectal abscess tracking round. If left alone an ischio-rectal abscess will tend to track through underneath the sphincter muscle and appear on the other side. Often in ischio-rectal abscess, if you open it on one side you will be surprised in two or three days to find a small abscess here, and you will find it is the same abscess pointing on the opposite side. It will give no evidence of its presence until it gets to the opposite side. This photograph shows the valves of Morgagni, and I show you the internal and external sphincters. Notice also Hilton's white line, or the muco-cutaneous junction. Notice these little black places, which are the crypts little tiny pockets, and between them is a little

frænum, which runs all round the bowel. These are of importance, because they sometimes account for some of the cases of pruritus and other cases of queer symptoms in the rectum which one can apparently find no explanation for, because some hard fæces tears one of these valves and you get a little tract formed from the base of one of these valves, and it will track backwards or it may track towards the skin. They show no signs of themselves whatever, and it is not until you put a speculum in the bowel and examine each pocket with a hook probe that you will find the apex of the hook will come down and present under the skin. Here is a picture of a speculum view, showing the anal region. Notice these papillæ between the valves of Morgagni. There are generally five of them, but in this case there are only four. They are found in most rectums, though the size varies a good deal. Their importance is that sometimes they become hypertrophied. For a reason which we do not know these papillæ get large and form polypi. And sometimes when enlarged they get nipped in the sphincter, because they lie almost immediately above the external sphincter, and set up a fissure or other condition of the anus, and they require treatment. In other cases they become excessively sensitive and seem to be the starting-point of pruritus. This next photograph shows the correct knee-elbow position for examining a patient. The thighs should be perfectly straight, at right angles to the bed, and the knees separated, and the patient should be resting on his elbows with the arms crossed under the chest. This is a very convenient position for examining the rectum by the sigmoidoscope for fistula or for any other condition. The objections to it are that it is an undignified position and it is an uncomfortable position, it cannot be maintained for long, and it is unsuitable in the case of ladies. The position which is, on the whole, the most satisfactory is the Sims's position. The photograph I show you is not quite correct. The left knee should be touching the bed in front of the other knee. By those means there is more negative pressure in the rectum. There should be a quite small cushion underneath the hip. The left elbow should be pushed under the back, and the patient should lie almost on his face, the knees drawn well up, and the feet kept forward out of the way. Otherwise you cannot use the speculum or sigmoidoscope, or your head will get into contact with the patient's feet. It is the most convenient for every purpose for examination of the rectum. It is comfortable, it can be maintained for a considerable time, and it is in every way admirable. It is surprising what a difference getting the patient into the correct posture for examination makes to the examiner. By not getting the patient into the correct position you cause yourself all sorts of difficulties which you need not have if you take this precaution. The patient should never be examined stooping over a chair, as is common, because a satisfactory examination cannot be made in this position. Another important thing is that there should be a good light, and if daylight is scarce there can be nothing better than a good forehead lamp. I prefer to use electric light always, because by always having the same intensity of light and the same kind of light, you can judge better and make comparisons better than if you were using daylight at one time and electric light at another. First examine carefully the anal region. For this purpose pull up the upper buttocks and examine the immediate surroundings of the opening for such things as fissures, sores, prolapsed piles, and the ischio-rectal fossæ where you are looking for the openings of a fistula. Next, carefully palpate the anus all round with the pulp of the finger, to feel for areas of tenderness and induration. With regard to palpation, you can feel much better if you grease your finger and slide it along the skin; you can then feel the track of a fistula. And with the educated finger you can often map out the whole track by simply palpating with the finger. The next thing is to insert the finger into the anus. The finger should be well lubricated, and for this purpose nothing is so good as vaseline, though soap is almost as good, and in the case of soap it has the advantage that it can be washed off, which is important if you wish to make a subsequent local application, such as with nitrate of silver. If you use vaseline to lubricate you will not be able to do that. So if you are contemplating doing such things afterwards, it is better to use liquid soap to start with; the finger should be passed only just within the external sphincter, that is to say, until it is just gripped by the anus. Most people unaccustomed to rectal examination make the mistake of passing their finger in too far. Ninety-nine per cent. of all the troubles in the rectum have their origin on a

spot half to three quarters of an inch within the anal margin. Conditions in the rectum higher up than this are comparatively rare. Most conditions, such as fissures, the internal opening of the fistulæ, piles, polypi, etc., are within that distance. So if you push your finger right up into the bowel at once you will feel nothing abnormal. While your finger is there you want to estimate the amount of grip which the sphincter can give—whether it is too much or too little. A further important point must be observed here, namely, you must take care not to hurt the patient, or you will defeat the object of making a thorough examination, and your examination will impress the patient unfavourably. Of course every surgeon wants to be better in his work than his neighbour, and it is not a bad thing for your patient, after your examination, to be able to say, "Well, doctor, you have not hurt me at all, but Dr. So-and-so hurt me badly." If you can leave such an impression on the patient's mind it will go some way towards enhancing your reputation. One has to be gentle in one's movements—it is the sudden movement which causes pain. The rectal specialist's most valuable asset is an educated finger. Any sudden movement causes contraction of muscle and considerable pain, as one can see by examining oneself. Move the finger gently and slowly, not only in introducing it but also in withdrawing it, because if you move it quickly in taking it out there will be a contraction of muscle produced. Unless there be a painful fissure present, it is possible to examine any person's rectum without hurting him. Where you suspect the presence of fistula, get the pulp of your finger towards the fissure, which will be posterior in most cases, and press very gently forwards and at the same time inwards, so that your finger goes in practically without touching the area on which the fissure lies. Then allow the pulp of your finger to come up against the fissure. It is well to blow in a little "anæsthesin" a minute or two before making an examination, but it is most important to be able to examine a patient who has anything the matter with the rectum without hurting him, because it is surprising how much you can hurt such a patient.

Now with regard to the use of probes in diagnosis. The best probe to use is one similar to this, with a large bulbous point, made of silver, with a flat handle. This is very easily passed along one's finger, and you can bend it into almost any shape. It is a mistake to use a sharp probe or a hard steel one. But the less a probe is used in diagnosis in conditions of the rectum, the better. Fistula can be diagnosed without resorting to a probe at all by simply feeling with the finger in the indurated area. With one finger in the rectum and the thumb outside you can feel the fistulous tract just as you could feel a hard wire under the skin, and you can thus map out a fistula more readily than with a probe. The probe, if it must be used, should be passed like a catheter, by its own weight; it should be practically allowed to fall into the fistula, and the same gentleness should be used as when introducing the finger.

Now with regard to specula. These are of great value if properly employed, but specula must be used carefully, and you must know how to use them, otherwise you will not only see nothing but you will hurt your patient very much. I show you a photograph of a speculum which was found in the tombs of Pompeii, in the grave, presumably, of a surgeon of that period. And it does not differ very much from the modern specula which are being re-invented and published in the *Lancet*. It is a bivalve speculum. I will also pass round two other specula which I have found very satisfactory in examining the anal region for such conditions as fissure or small submucous tracts immediately within the anus. The speculum is slightly tapered but has very thick edges, so that there is nothing to cut into the mucous membrane, and the apex of the fenestra has a sloping end, so that the membrane can slide up the slope without being caught into the end of the speculum. Most specula which have the fenestrum at the side have the objection or disadvantage that the membrane gets caught badly. I have known one case in which an anæsthetic had to be given to get the speculum out. The other speculum is for looking at piles and for examining the bowel immediately above the external sphincter. The obturator is withdrawn, and the speculum is drawn back and pushed in again so as to allow you to get a view of anything which is in the bowel. That is very good for examining piles, for instance. The ordinary bivalve type of speculum which you put in and open is iniquitous, and it should only be used in the torture chamber. It has to be passed in and opened so that it will stretch the fissure wide open and hurt the patient very much. When you with-

draw it you collapse it, which means it will nip the mucous membrane which is in the opening. It should only be used under an anæsthetic.

For examining the bowel above the area, say, a couple of inches from the rectum, here is the drawing of the ordinary pattern sigmoidoscope which we use for this purpose. The lamp lies half an inch from the end of the instrument, and up against the roof it has an electric attachment for the illumination of the bowel and a pneumatic arrangement to inflate with. There is another pattern of sigmoidoscope which is used in Paris, in which the illumination is obtained by means of a mirror with a hole in it at the back of the instrument, the light being obtained from a bulb in the handle. The light thrown on to the mirror is reflected down the tube, which has to be accurately focussed, and that is done by sliding the lamp. The objection to that type of instrument, compared to the other, is that the cumbersome portion of the instrument which I show you is in the way, and that you cannot keep the light on while you are passing anything down the instrument; you must pass it in the dark. This instrument, which is used in Paris, is much more complicated than the other form, and as a rule, the simpler the instrument the more easily is it used.

Now with regard to the examination of the abdomen. It is no good operating on a man with piles if he has bad cirrhosis of the liver, because the piles will come back again. Therefore, see whether the liver is normal or whether there is a tumour in the sigmoid flexure. The urine similarly should be examined in all conditions for sugar and albumen, and it is well to examine the stools. It is never wise to take the patient's word on the question of whether there is blood in the stools or not: examine the stools yourself. One very important point arises out of that, and it is this: One is always hearing of patients who are supposed to be suffering from cancer of the bowel because they are passing tape-like fæces. You will see it mentioned in the text-books that one of the symptoms of stricture or carcinoma of the rectum is the passage of tape-like or pipe-stem fæces. That is an absolute fallacy, and the way in which it has arisen is that in the old days carcinoma of the rectum was never diagnosed until the growth stuck out of the anus, and when it reached that stage, of course there was constriction of the anal orifice, and the fæces naturally had a pipe-stem shape and size. But of course fæces can only assume the shape and size of the last opening through which they come, and that last opening must be the anus. Whatever the size of the orifice or of the fæces before reaching the anus they must be remodelled again on passing the anus. So that although a tight sphincter may easily cause tape-like fæces, stricture in other parts of the rectum will not, and therefore that symptom is absolutely valueless. Another one, which is equally valueless, but is often mentioned, is ballooning of the rectum, which simply means there is a good deal of air in it. You can balloon anybody's rectum by putting them into the Sims posture and passing a tube which will allow air to enter. It will balloon, too, if there is flatus in the rectum. But there is no diagnostic value about it; it simply means there is wind or flatus in the bowel. It cannot mean anything else.

Lastly, in difficult cases it may often be necessary to give an anæsthetic to thoroughly examine the patient. And that particularly applies to painful conditions, such as ulceration of the bowel and strangulated piles. In such instances the examination should be done under a general anæsthetic. It is also important to remember that more than one condition may be present in the same patient. He may have piles which are strangulated, complicating cancerous stricture higher up the rectum. So one should put one's finger well up into the bowel to make sure there is nothing higher up or behind. Fistulæ frequently accompany stricture in the bowel, so on finding a fistula one should eliminate one cause of it, which is stricture. Other points are bleeding; the character of the blood is important. For instance, dark clots of blood do not mean that they do not come from piles, the blood having passed up into the bowel. Bright blood practically must come from the anus, or from a place near it. Blood of a chocolate colour comes from growth or something else higher up in the bowel. With regard to diarrhœa, there is one important thing, and that is spurious diarrhœa caused by stercoliths. A stercolith in the rectum will cause, not constipation, but diarrhœa, and that frequently gives rise to difficulties and mistakes. A short time ago I saw a lady who had been

suffering from a good deal of pain and bleeding from the bowel and rectum, and some looseness of stools and diarrhœa. She went to see her doctor, who listened to the narration of her symptoms, and he said he thought she had piles, and gave her a treatment for piles—an ointment to inject, and medicine to take. Her pain and distress became no better, and the diarrhœa continued. She came to London, and on examining her bowel there was found to be a fæcal concretion as large as an orange in the lower part of the rectum. And there was an enormous mass as hard as concrete which took me three quarters of an hour to chip up with a chisel. So remember a stercolith does not always produce obstruction, but more frequently causes diarrhœa.

I will now show you slides illustrating different conditions of the rectum and anus. Here is a drawing to show the method of passing the sigmoidoscope. It is introduced with an inclination backwards towards the sacrum, and it promptly impinges against the lower part of the promontory of the sacrum. One has then to tip it forwards and slide it down into the sigmoid flexure. The anus is tipped backwards. Next I show you a photograph of a drawing of condyloma of the anus. It shows the character of the condition. These things are often mistaken for external piles—a serious mistake when you have no gloves on. There are these large, flat, leathery-looking things sticking out round the anus, and often you will find them over the perinæum and scrotum and over the buttocks. Further examination will reveal the presence of sore throat, or a history of sore throat and a rash. You need to be always on the lookout for these things, for the number of surgeons in London and elsewhere who have infected themselves with syphilis by touching these things, even men of large experience, is considerable. A condyloma like this one shown in the picture will be detected at once. But very often you see these patients when there is only one condyloma, virulently infective, and it is easy to get one's finger infected. If you examine for such a condition be sure you only do so with india-rubber gloves on. Here is a photograph showing external piles—thrombotic piles. Frequently one sees external piles described as the condition in which there is a skin tag. But a skin-tag is a skin-tag; it is not a pile, and tags are not important. True external piles contain a large blood-clot. If you split them open you find they contain a hard blood-clot. A small vessel on the edge of the anus has ruptured into the cellular tissue, which is very loose in this region, and it forms one of these large swellings. Some people say that a clot has formed inside a vein. I think these are ruptured veins, and that they are simply hæmatomata at the edge of the anus. They are dark in colour, looking like a grape. The best way, if they are causing pain, is to cut them off—that is to say, two thirds of them—then shell out the clot, dress the little wound which results for two or three days, and then they will heal up rapidly. If left to themselves they will get right; they disappear in a week or ten days, leaving merely a rough surface of skin, unless they are very large. If they are not kept very clean they will get septic, and in that case there may be a nasty abscess at the edge of the anus. Here is a photograph from a patient who came to the hospital with external piles. He had an enormous crop of external piles, nearly half the size of one's hand. He had strangulated external piles above, and there is œdema from strangulation of the piles above. The lymphatics and veins draining this area pass up into the bowel above, but you sometimes see this after operation for piles, because their removal has blocked the circulation temporarily from the anal margin. Here is a photograph of internal piles in a bad case where they are prolapsing. They are shiny, red-looking things, and here is a ring of œdematous skin which you find round the outside of piles. These may be very large swellings, but they are in the skin and due to œdema, the blood-supply being shut off owing to nipping of the piles in the sphincter. These subside after the piles have been put right. Here is a drawing of a fissure. In this case the fissure has begun to produce a fistula. Notice the large sentinel tag produced by œdema. The swelling around the fissure shuts off the lymphatics, and the skin just below the ulcer becomes œdematous and forms this large pile. Here is a photograph showing a fistula with two external openings. Notice the external opening with a bluish-looking edge, and here you see the slight elevation formed by the track of the fistula. It is very exceptional to find more than one internal opening to a fistula, though there may be a dozen external openings, and in most cases that internal opening

is posterior. Sometimes it may be an anterior or a lateral fistula.

Here is a photograph of a case showing tubercular ulceration of the anal margin. There are no proper granulations, but a thin discharge and a nasty, undermined edge. If you examine the bowel above you find other ulcers, often of an extensive character. These are very difficult cases to deal with. In fact the proper way of treating them, as for treating other bad cases of tubercle, is to deal with them at a sanatorium, keeping the parts as clean as possible until the general improvement of the patient sets in. As a rule cases of that sort are in an advanced stage of phthisis. In the next slide I show you a very rare form of tubercle of the anus. These cases are very uncommon. This looks almost like an epithelioma—a very hard, white-looking object on the edge of the anus. The first one I saw I thought it was early epithelioma of the anal margin, and so convinced was I that it was this that I cut it out, but when seen under the microscope it was found to be a tubercular lesion. And I have seen several other cases since. It is an early primary tuberculide of the anal margin. They are small things, about the size of one's finger-nail. Here is a picture of a case of epithelioma of the anus. It is a hard, ulcerating cancer with overlapping edges. In such a case you will find the inguinal glands enlarged. Here is another drawing of a case of cancer of the rectum as seen through the sigmoidoscope. Notice the recto-sigmoidal junction and the large growth as seen through the sigmoidoscope. Here is a carcinoma of the rectum high up, as seen through the same instrument. It is a very rare specimen, but it shows the mucous membrane covered over by thousands of polypi; indeed, there is no actual membrane to be seen at all. It causes symptoms similar to those of ulcerative colitis—that is to say, profuse diarrhœa, and in a short time these polypi become malignant. And then you may get three or four growths starting at the same time. Such cases are practically hopeless, from an operative point of view, because if you remove one growth you leave others behind. Here is another form of multiple polypi of the colon. The whole colon from end to end is full of these things, and two places have become malignant.

February 13th, 1911.

A RETROSPECT OF DERMATOLOGY.

By E. GRAHAM LITTLE, M.D., F.R.C.P.,
Physician in Charge of the Skin Department at St. Mary's Hospital, and at the East London Hospital for Children.

THE most conspicuous event of the year has been the discovery of the Ehrlich-Hata specific for syphilis, and many contributions have been made to the subject, which indirectly concerns dermatologists. We are promised an English translation of the small book prepared by the inventors themselves on their discovery, and as there is no reason to suppose that the present somewhat undignified booming—lay and medical—of a preparation, which has been furnished only to a few privileged and rather curiously selected persons, can have left any readers of the CLINICAL JOURNAL ignorant of the merits claimed for the treatment, it is unnecessary to describe this treatment further.

The Wassermann reaction has been greatly used and its importance becomes daily more marked. It is a matter of contention whether the various simplifications of this process give as reliable results as the original method. American authors, using both Wassermann's original method and Noguchi's modification, have found a positive reaction sufficiently frequently in scleroderma to be confronted with the problem whether scleroderma is syphilitic or whether it is one of the diseases, like leprosy and yaws, which, though non-syphilitic, give the reaction. Whitehouse* decides in favour of the proposition that scleroderma is really a syphilitic manifestation. The improvement which mercury effects in some cases of undoubted scleroderma would lend some countenance to this opinion from clinical as well as from laboratory evidence.

Sabouraud† has isolated a new organism which is responsible for the disease known as eczema marginatum or tinea cruris, which used to be considered as a trichophytosis. The organism, which Sabouraud has named *Epidermophyton inguinale*, has a special type of fructification which distinguishes it from all other dermatophytes. The disease commences in the groin, and is often

* 'Journ. Cut. Dis.,' December, 1909.

† 'Ann. de Derm. et de Syph.,' June, 1910, p. 289.

attended with vesicular eruption on the fingers and toes, easily confounded with eczema. It is singularly resistant to treatment; chrysarobin ointment 1·3 per cent. is the best application. The disease is easily identified by finding mycelial threads in the scrapings from the skin.

Castellani,* commenting on this work of Sabouraud's, claims to have identified and named three species of this organism differentiated by the characters of growth on special media.

Dubreuilh and Brandeis† have reviewed the classification of certain pus-infections, especially the clinical varieties known as impetigo contagiosa, Bockhart's impetigo, bullous impetigo, and ecthyma. The idea that the bullous varieties are due to streptococcus and the forms beginning with pustular lesions to staphylococcus is not confirmed by these researches: impetigo contagiosa, *e.g.*, was found to be equally often due to staphylococcus as to streptococcus, and these authors conclude that no indication of the bacteriological nature of the infection can be derived from merely clinical aspects.

Chauffard and Laroche‡ have a very interesting paper demonstrating the presence of cholesterin in the skin of patients suffering from xantheloma planum in the actual lesions and before these are clinically evident. The pathological identity of xanthoma tuberosum and xanthoma planum is now accepted, and both forms are the index of a more or less profound disturbance of fat-metabolism and an increase of cholesterin in the circulating blood, possibly due to an organic or functional disturbance of the pancreas. As a corollary of this demonstration, the diet of patients threatened or actually afflicted by xanthoma should be carefully regulated with the view of restricting the amount of cholesterin formed.

An extremely interesting discussion on the causation of psoriasis took place at the meeting of the American Dermatological Association and is reported by Pollitzer and Schamberg.§ Pollitzer regards the parasitic explanation of psoriasis as the most satisfactory, and accepts the identity of seborrhœic eczema and psoriasis. He regards both these diseases as due to an organism not yet identified with certainty. Schamberg made numerous attempts to isolate a microbic organism in ten cases, but found no single species constantly present. There have been various claims to have isolated organisms supposed to be the cause of the disease, but no well-accredited case has been made out for any, and the general result of this important discussion was that we are "no more advanced in our knowledge of the cause of psoriasis than we were fifty years ago."

The therapeutics of skin diseases.—No striking novelty has to be reported under this heading. The use of carbon dioxide snow has been extended and amplified. Pusey, its originator, has an excellent article,* full of practical suggestions, and reports success with this treatment in eczema, lichen planus, lupus erythematosus, chloasma and lentigo, tattoo-marks and powder-stains, warts and callosities, nævi, senile keratoses; he does not recommend the method in lupus vulgaris and epithelioma, but other authors have had good results in these two diseases.

The spread of vaccine therapy has been steady, and continental as well as American and British reports are coming in. Lassueur,† working with vaccines derived from St. Mary's Hospital, found manifest improvement as regards the pustulation in cases of acne pustulosa; less manifest improvement in comedo-formation. Davis‡ had remarkable results in a case of acute eczema with the administration of staphylococcic vaccines. The doses started with one hundred millions, and between September 15th and October 23rd there were nine injections, the largest of one thousand millions. Atkinson§ used autogenous vaccine in a case of extensive furunculosis in a baby, æt. 2 months, and much enfeebled by diarrhœa; two doses of one and a quarter millions effected a cure.

Klotz‖ recommends several formulæ for skin-varnishes, of which the less familiar may be the following: In sycosis and parasitic disease of the nails a mixture of two parts ether and one part oil of wintergreen is useful. Mercuric bichloride in tinct. benzoin co. 0·1 per cent. is successful in

* 'Brit. Journ. Derm.,' August, 1910.
† 'Ann. de Derm. et de Syph.,' June, 1910, p. 323.
‡ 'Sem. Méd.,' May 25th, 1910, p. 241.
§ 'Journ. Cut. Dis.,' November, 1909.

* 'Journ. Cut. Dis.,' July, 1910.
† 'Ann. de Derm. et de Syph.,' July, 1910, p. 377.
‡ 'Lancet,' November 6th, 1909.
§ 'Lond. Hosp. Gaz.,' March, 1910.
‖ 'Ther. Gaz.,' February 15th, 1910.

removing freckles. Tumensol (20 per cent.), in a mixture of equal parts of alcohol, ether and water, is recommended for intertrigo and affords a protection for parts exposed to be saturated with urine or sweat. Pure ichthyol mixed with equal parts of water "contracts superficial vessels and favours cornification of the epidermis," and may be used with advantage in erysipelas, burns, erythemata generally, herpes zoster, and intertrigo.

Bulkley* advocates a diet restricted to rice, bread and butter, and water in cases of acute irritative diseases of the skin, such as lichen planus, dermatitis herpetiformis, generalised eczema and psoriasis. The diet is kept up for from three to five days and ordinary diet then gradually resumed.

Hale,† from a large experience in the Army, has evolved a routine method of treatment of bromidrosis, which he describes as follows: The socks, boots and shoes of the affected person are segregated, the socks soaked for an hour in 1–2000 perchloride solution, and washed out thoroughly, and the insides of soles, vamps, quarters and counters of boots and shoes are painted with a lotion consisting of two drachms of salicylic acid to the ounce of methylated spirit. The feet are well washed with soap and water, dried completely, and painted with the same lotion. New socks are put on and the feet repainted on the following day, when the cure will be completed.

Crude coal-tar. — Brocq, Dind,‡ and other writers in the 'Annales de Dermatologie' have from time to time during the past two years advocated the use of crude coal-tar in various skin diseases, especially eczema. I have had personal experience of its value both in hospital and private cases, and can endorse this recommendation. The tar as obtained from gasworks must be washed with distilled water to remove excess of ammonia; the product finally obtained should be a thick, creamy fluid, which forms a good adhesive coat when painted on the skin, the surface of which should be carefully cleansed before application. The tar is painted on and allowed to dry, and left uncovered, or the part treated may be bandaged over lightly while still wet. The application should be renewed when the coating shows signs of cracking —usually within two days.

February 13th, 1911.

* 'Brit. Med. Journ.,' September 24th, 1910

† 'Journ. R.A.M.C.,' January, 1910.

‡ Abstr. 'Brit. Journ. Derm.,' May, 1910, pp. 175–177.

Isolated Fracture of Transverse Processes.—Tanton refers in particular to the transverse processes of the lumbar vertebræ. This lesion is one of the latest to be revealed early by radiography, and the disturbances resulting from it are generally mistaken for other traumatic or post-traumatic affections of the lumbar region. Only fifteen cases of the kind have been reported, he believes, and he adds two to the list. Pain is the chief aid to diagnosis; it may be more or less localised and more or less severe, with sometimes pain in moving the legs. The local pain is usually intense at the moment of the fracture, and it may radiate into the adjoining parts or follow the sciatic nerve, or the anterior abdominal wall may be tender. When the fracture involves the transverse process of the first lumbar vertebra the pain may be so severe as to interfere with respiration and immobilise the diaphragm. The patient generally lies on his back, and efforts to raise the leg on the side of the fracture are usually extremely painful, as is also bending the trunk forward. When the trunk is flexed the lumbar curvature increases in case only one process is fractured, while the curvature flattens out if several are involved. There is little, if any, pain as the trunk is bent backward, unless the fracture involves the fifth transverse process. The patient picks up an article from the floor like a person with Pott's disease; the gait is also characteristic, the knees are bent, the arms akimbo. The lesion generally heals in about a month without leaving appreciable traces, but in two of Haglund's cases the pains persisted for two years, and in Hoffmann's case an ossifying myositis developed. If the disturbances persist the fractured process may be removed, as has been done in one case; in two others the hæmatoma in the region suppurated. In the first the necrotic broken-off piece of the process maintained a purulent fistula, requiring two operations before recovery. In the other case the trouble was ascribed to a retrocæcal abscess, and notwithstanding two operations, the patient succumbed to suppurative psoitis and pneumonia. Any contusion of the lumbar region leaving persisting pain and functional disturbances should be examined for isolated fracture of one or more of the transverse processes.—*Journ. A.M.A.*, vol. lvi, No. 4.

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SOME COMMON GYNÆCOLOGICAL AFFECTIONS.*

By THOS. G. STEVENS, M.D.Lond., F.R.C.S.Eng., M.R.C.P.Lond.,

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GENTLEMEN,—I have chosen for consideration this evening some of those conditions which we meet with every day, which are always a difficulty, and which lead to much of the minor but prolonged suffering so common in womankind. There is no doubt that we get far more credit for the successful treatment of minor ills than for the brilliant results of major surgery, and conversely, if our efforts do not meet with success our downfall is the greater the more trivial the disease in question.

Cervical erosion and cervical catarrh or endometritis giving rise to constant leucorrhœa is perhaps the commonest of the diseases of women. It is very resistant to treatment, and is often the subject of much anxiety when its diagnosis from carcinoma of the cervix is the question. I will not bore you with its causation of symptoms, but will confine my remarks to the two practical points of diagnosis and treatment. When we meet with a cervical erosion, which bleeds on examination or on coitus, the suspicion that it may be malignant at once arises. How is this differential diagnosis to be made? The history very seldom assists us: we must rely upon digital examination, ocular inspection, and finally, the microscope. To the touch an erosion has a soft velvety surface, but the tissues beneath feel as hard as leather, so hard in fact that firm pressure makes no impression upon them. This leathery hardness results from fibrous hyperplasia in the fibro-muscular tissues and is

* Paper read before the Thanet Branch of the British Medical Association.

very important. On the other hand, the hardness of cancer of the cervix is quite different—it is a brittle hardness which will break down under pressure of the finger, setting up profuse bleeding, whilst the bleeding from an erosion is never profuse—it is a mere oozing. Further, a sharp curette makes no impression on erosion beyond removing the merest fragments from the surface, whilst it will dig out cheesy masses from a cancer. Inspection through a speculum does not give us anything like as much information as the finger, except, perhaps, in very early cases, when it will enable us to see a truly ulcerated spot in an erosion where a carcinoma is beginning. I need hardly remind you that erosion is not ulceration, but is strictly overgrowth of glands and fibrous tissue. Finally, if doubt remains, the microscope provides an infallible test. A wedge should be cut deliberately out of the suspicious part of the cervix and carefully preserved at once in a good fixing fluid. This wedge should include some normal tissue at the base, and can be practically always cut without general anæsthesia. This tissue, properly preserved and examined in sections by a competent pathologist, will not fail in 2 per cent. of the cases to lead to a certain diagnosis. But I must warn you that the pathologist must have special knowledge of uterine diseases. It is because many good all-round pathologists do not possess this knowledge that the microscope is sometimes described as a means of diagnosing cancer of the cervix and endometrium. With regard to treatment of cervical catarrh, we have at last got a drug which promises to lead to better results than we have ever been able to get in the past. I mean picric acid. It is best employed as a saturated solution in strong alcohol, and is to be used for swabbing out the cervical cancer as far as the internal os with a Playfair's probe wrapped in absorbent wool and dipped in the solution. Two or three probes should be used for the canal at one sitting, and then if there is an erosion it should be dabbed all over with the solution until it is hard and coagulated on the surface and stained bright yellow. This treatment should be carried out once a week for from four to six weeks; great improvement takes place in this time as a rule, and although, perhaps, the discharge does not absolutely stop, it is so slight that the patients are satisfied. Such a result in my experience was never obtained by carbolic acid, iodised phenol, chromic acid, etc., which were in vogue twenty years ago.

Curettage does not cure these cases, as no doubt we have all found out, simply because the source of the discharge is in the glands, which are barely disturbed by the curette. This treatment will not, of course, take the place of amputation in very bad cases where there is much scarring and laceration of the cervix. It is wise to employ daily douches whilst the picric treatment is in use, and alum is perhaps the best drug to use with the douche. I have lately tried picric acid in acetone instead of alcohol, and I am inclined to think the results are not much better.

Endometritis of the body of the uterus is perhaps the next most common disease met with, and although not so annoying to the patient, unless accompanied by cervical catarrh, is more serious because of the blood-losses it occasions. We do not always realise the importance of endometritis, but if we remember its important series of symptoms, namely, menorrhagia, leucorrhœa, and backache, we cannot fail to see that it is *the* important accompanying lesion in many conditions of the pelvic organs, such as fibroids, backward displacements, and salpingo-oöphoritis. If one could cure and prevent the recurrence of endometritis we should not have to remove many fibroids, or use many pessaries. It is, however, uncomplicated endometritis I want to talk about to-day. Given the three symptoms, menorrhagia, leucorrhœa, and backache, we can diagnose endometritis whatever other lesion may be present in addition. If, however, the uterus is not markedly enlarged when bimanually examined, and is not displaced, we are justified in diagnosing uncomplicated endometritis. In practice there is no cure for it except by the operation of currettage, and I fancy I hear someone say, and "that often fails." This is the very reason why I introduced the subject, and I think I can tell you why the operation sometimes fails. It is because of the manner of performing the operation in the main, and is also conduced to by the after-treatment by douches which is often adopted. I feel confident that the operation fails to cure in some cases because it has not been performed in a strictly aseptic manner. Further, I believe that if douches are used afterwards, sepsis may be introduced. Of course, I do

not mean that the operation introduces virulent sepsis, although I have seen that more than once, but I mean a mild septic infection which is not sufficient even to raise the temperature to 100° F., but is quite enough to keep up and set up the disease afresh.

To perform the operation in an aseptic manner three things are necessary: First, the vagina must be made aseptic; secondly, nothing whatever must be put in the uterus which is not sterile, therefore it follows that a bare finger must not be put in; thirdly, there must not be any source of sepsis introduced afterwards. The first postulate is the one in which failure is often initiated, because a vaginal douche is relied on to prepare the vagina for operation. *No* vaginal douche of a practical nature will make the vagina aseptic. It must be prepared exactly in the same way as the abdominal skin before a laparotomy. The vagina must be scrubbed out with sterile wool and ether soap first, all folds and furrows being thoroughly scoured. Then an efficient antiseptic, such as 1 in 100 lysol solution, must be used in a similar manner, at least five minutes being taken up in scrubbing. This need not be done roughly—in fact, I have often done it in a conscious patient without any great objections being raised. I need hardly say the vulva must be shaved and the skin cleaned in the same way. The second consideration implies that every instrument to be used must be boiled, even the coated probes which are to be used for applying a powerful germicide after curetting. Here I should like to show you the contrivance I use instead of cotton-wool or probes. The hands need not be gloved for this operation unless a finger is to be used for exploring the cavity, in which case a sterile glove is indispensable. Further, no packing should be used unless hæmorrhage is free and necessitates plugging the uterus. If a plug is required it must be sterile, and I would warn you that dry iodoform gauze, which is so often used, is not in itself sterile. It can be made so by soaking it in lysol or formalin solution for some days before use. Finally, no douching should be permitted afterwards; it is a survival of ancient prejudices, and cannot possibly do any good, any more than an intra-uterine douche can do anything to prevent sepsis if a dirty hand has been used to perform version. In fact the douche may do harm, because weak antiseptics only are used, and they may serve very well to introduce sepsis. We do not always realise how difficult or impossible it is to give an aseptic douche. If these directions are followed we shall not have many failures after curettage in uncomplicated cases.

Retroversion and *retroflexion* of the uterus are the commonest combined lesions found in women who have pelvic symptoms, especially backache. Further, much suffering is often caused by attempts to treat these displacements when very frequently no local treatment is required. It has been brought home to me after many years of hospital out-patients' that only a comparatively small percentage of cases of backward displacements of the uterus require any local treatment at all. My attention was first drawn to this by seeing large numbers of patients who were wearing pessaries for these displacements without any benefit to the symptoms complained of. In fact, it turned out in most of these cases that the symptoms were not due to the uterus at all. Therefore, when I see a backward displacement, I always ask myself whether the displacement is the cause of the symptoms or whether they can be explained by some other condition. It is not always possible to answer this question off-hand without trying various forms of treatment. Suppose a patient complains of backache, slight frequency of micturition, constipation, leucorrhœa, dyspepsia, etc., as patients so often do, and a retroversion is found on examination, I do not at once replace the organ and put in a pessary, because I know that possibly none of these symptoms will be improved. The micturition troubles may be caused by lithiasis; the constipation has nothing to do with retroversion, neither has dyspepsia; leucorrhœa is due to cervical catarrh in most cases, and is more likely to be due to infection than to the displacement. All these symptoms can be treated apart from the displacement of the uterus, and in most cases the patient will improve. Suppose, however, the patient complains, in addition, of bearing down, has some menorrhagia, and possibly dyspareunia, it then is clear that we have a condition of uterine congestion, with possibly prolapsed and tender ovaries, and we are then more justified in thinking that the displacement is the cause of the symptoms and therefore requires local treatment. Many unmarried women and girls have retroverted uteri, which are of congenital origin or,

at least, are acquired very early in life. Some chance muscular strain acquired by a trivial accident causes some pelvic pain and backache and leads to a pelvic examination. If a retroversion is found it is often wrongly put down to the accident, and the unfortunate girl is provided with a pessary. This is a calamity for many girls, as it draws attention to their sexual organs and lays the foundation for various neurotic disturbances, which are very difficult to eradicate. I have almost laid it down as an axiom never to use pessaries in virgins except for prolapse, but, of course, there may be exceptional cases. In my opinion a permanent retroversion is never caused by an accident; the uterus is too movable, and is as easily replaced by changes of position as it is displaced. Permanent displacements mean superadded lesions in the form of softened, stretched supports, subinvolution, and injuries to the pelvic floor. Practically speaking, acquired retroversion is caused by pregnancy and labour, no other cause being capable of proof. It is interesting to note that a congenital retroversion may cause no symptoms until the patient marries and becomes pregnant. She is then very likely to abort about the second month, and an examination reveals the displacement for the first time. These uteri always assume their faulty position after abortion, and, as far as I know, are not cured, even if the pregnancy goes to full time. After this condition is discovered it is best left alone, but most careful advice should be given to the patient to come for treatment immediately she recognises that she is again pregnant. At about the sixth week of pregnancy these uteri are very easily replaced and kept in place by a ring pessary, and abortion will not then occur again. I know of several patients who never require a pessary except during the early months of pregnancy, although their uteri are permanently displaced when not pregnant; they have no symptoms.

When pessary treatment is definitely decided upon, it must be clearly realised that the pessary is useless unless the uterus can be first replaced. I often examine people who are wearing pessaries and find the uterus displaced in spite of the pessary. These people get no relief from the pessary. If the uterus cannot be accurately replaced by the fingers alone it can always be accomplished if a little traction is made on the cervix with a vulsellum forceps, provided there are no adhesions. I never use the sound for replacing a uterus, and I would strongly advise you not to, but rather to use the vulsellum. It is no more painful to the patient, and certainly does not expose the patient to the risk of infection, which the sound always does. I cannot miss this opportunity of strongly advising you never to use the sound for any purpose except just before an operation, when the whole operation field is sterile. However carefully the sound is sterilised, when it is used in one's consulting room for diagnostic purposes there will always be a risk of carrying infection from the vulva or vagina into the uterus. Many a case of acute endometritis, salpingitis, pyosalpinx and peritonitis has been set up solely by the use of the sound for diagnostic purposes. And when all is said and done, what will the sound tell you?—the length of the uterine cavity and its direction. With all due respect, I hold that the length of the uterus can be estimated by the fingers, and the direction of its cavity is of no diagnostic importance unless an operation is about to be performed. I say nothing of the possible risk of perforating the uterus or of inducing an abortion. The vulsellum, on the other hand, makes but a small puncture in the cervix, and very slight traction suffices to disengage the fundus from under the sacral promontory. After that it is easy to push the fundus forwards with the finger in the posterior fornix, whilst the os is carried backwards by the vulsellum. With regard to pessaries, I am proud to say I have not yet invented one, and, personally, hardly ever use anything but rings. The Hodge has its uses for cases of retroversion where the vaginal entrance is narrow, but with relaxed tissues the ring will always keep its position best. It is always best to use the *smallest* pessary which will stay in and which will suffice to keep the uterus in place. Some people seem to think they should use the largest pessary they can possibly get into the vagina. This is, of course, a mistake, and often gives the patient chronic pain, only to be relieved by taking away the pessary.

In those cases where there are prolapsed and very tender ovaries pessaries may not be tolerated, and there is nothing so good as a three weeks' course of ichthyol vaginal pessaries with hot douches night and morning. Twelve grains of ichthyol in 120 grains of cacao butter makes a suitable pessary, and should be used every other night, a hot douche

being given before, and the *débris* washed out in the morning.

Radical operations are necessary in some of these cases, especially if there are adhesions, or the ovaries are so tender as to cause dyspareunia, or if a pessary absolutely fails to keep the uterus in place or cure symptoms. I am in favour of an intra-abdominal operation on the round ligaments rather than any form of ventro-fixation or vaginal fixation.

Before leaving this subject, I may just mention retroversion of the gravid uterus. You are familiar with its classical bladder symptoms, and there is no doubt that if you can keep the bladder empty and the patient in bed the uterus will often right itself. I am quite sure, however, it is much less irksome to the patient if you can replace the uterus with the fingers at once. If you cannot a very good plan is to put in a large ring pessary, put the patient to bed, and make her lie on one or the other side, but not on the back. If she can empty her bladder so much the better, and the uterus will almost invariably be found to have righted itself in a week. This method of treatment was first suggested by Japp Sinclair. I have often found it quite successful in patients who have gone home wearing the pessary, and have not been seen again for a week. Of course, these people had no retention.

Urethral caruncle is a small matter, but occasions much suffering, and is decidedly difficult to cure. The old treatment with the cautery is by no means always successful, because a granulating wound is left, and is very liable to become infected and form a fungating granuloma, which practically reproduces the original growth in a few weeks. It is much more satisfactory to excise the caruncle after placing a blanket-stitch under it ready to be tied. In this way a small wound whose edges are in apposition is produced, and primary union is to be expected. Recurrence is much less likely to occur if this operation is done.

Ovaritis.—This is a term much beloved of women, and is used too often by medical men without due regard to pathological facts. If it means anything at all, inflammation of the ovary must be the lesion it designates, and yet when one investigates the cases of this so-called condition, one finds that not a tenth of them have any evidence of inflammation at all. It is commonly applied to those patients who have pain in that region of the abdomen between the umbilicus and the anterior superior spine of the ilium, which is known as the "ovarian region." Strictly speaking, this region is the area of skin supplied by the tenth dorsal nerve, and pain referred to it may be set up equally well by disease of the uterus, Fallopian tube, kidney, and even gall-bladder, as well as the ovary. In spite of this, it is extremely common to meet with patients who have been told they have ovaritis when this pain is the only symptom complained of, and no definite lesion can be demonstrated. It is a fact that in many of these cases the ovary of the affected side is tender, but so is the ovary of a normal individual, just as the testis is sensitive in the male, and the degree of sensitiveness varies with the individual without any definite lesion being present. Now when we come down to pathological facts, we find that the only lesion which can strictly be called ovaritis is a true inflammation set up by infection, and always accompanied by local peritonitis. The cause of this infection is, in the majority of cases, a spread of bacteria from the uterus along the Fallopian tube, setting up salpingitis by the way; consequently it is very commonly gonococcal, and often due to other bacteria which cause a septic endometritis after labour, abortion, or operations on the uterus. Tubercle, too, may infect the ovary, but practically always secondarily to the Fallopian tube or peritoneum. It follows, therefore, that primary infection of the ovary is almost unknown, that ovarian infection is practically always accompanied by salpingitis, and always by local peritonitis setting up adhesions. It follows, too, that an ovary which is tender, but which is not fixed and not accompanied by signs of peritonitis, is not inflamed. What, then, is the matter with the ovary in these common cases of so-called ovarian pain, often accompanying dysmenorrhœa? Very often there is nothing the matter with the ovary at all, and on careful examination, testing the skin sensibility with a pin, it will be found that the pain is really *in the skin*, that it is a skin hyperæsthesia, and may be a referred pain from some other organ, or, more commonly, is a manifestation of hysteria. If, with skin hyperæsthesia in the tenth dorsal nerve segment, there is absence of the palate reflex, increased knee-jerks, and the so-called hysterical "stocking," the diagnosis of hysteria is

certain. Constipation alone may be the cause of this pain; renal irritation may cause it as well as real pathological conditions of the pelvic organs. True ovaritis, on the other hand, or oöphoritis as it should be called, is a part of a salpingo-oöphoritis, is accompanied by peritoneal adhesions, gives rise to a fixed lump on one or both sides of the uterus (consequently fixing the uterus), may be accompanied by suppuration, and generally shows the signs of true inflammation. The history of these cases is usually one of a definite illness, acute in onset, accompanied by fever, subsiding in about two weeks, and leaving behind it a tender, fixed swelling in the pelvis. In my opinion, to sum up, there is no such thing as ovaritis if the uterus and adnexa are not fixed by adhesions.

Whilst speaking of pelvic inflammation I cannot pass on without a word on pelvic cellulitis. I often hear the diagnosis of cellulitis made when really the lesion is peritonitis. Pelvic peritonitis around inflamed tubes and ovaries, or following a puerperal infection, is a very common lesion, and its results are to blame for much gynæcological invalidism. Pelvic cellulitis, on the other hand, is a comparatively rare condition, and its pure form invariably follows a puerperal infection, usually through a torn cervix. Of course you cannot have a salpingo-oöphoritis and peritonitis without the surrounding cellular tissue being affected to some degree, but the essential lesion is the peritonitis. On the other hand, there may be no peritonitis at all with a true puerperal pelvic cellulitis.

Prolapse of the uterus is perhaps the real bugbear of practice, and one that taxes our patience and ingenuity to the utmost. Nothing can be more distressing, without being dangerous to life, than a severe uterine prolapse. There is nothing new to tell you about prolapse, but a few ideas occur to me which may prove of interest. In ninety-nine out of one hundred cases the condition is due to pregnancy and labour, and essentially is the result of stretching of the connective tissue which runs from the cervix and vagina along the blood-vessels to the pelvic wall, plus some injury to the pelvic floor. The one thing which I can conceive may cause stretching of the peri-vascular connective tissue is a long labour with slow dilatation of the cervix and consequent downward displacement of the cervix as the head descends. Also forceps extraction with a cervix only partly dilated drags the *uterus* down along with the head and may stretch all the supports unduly. Further, if involution is delayed after labours of this kind prolapse is the invariable rule. This stretching of the supports of the uterus is recoverable if involution occurs normally, if the patient is well nourished, and if she is in good circumstances and has no hard work to do. This, I take it, is why we see so little serious prolapse among the well-to-do. Involution and recovery of labour injuries is assisted by a suitable ring pessary for a time, and, above all, by an active preparation of ergot given in full doses. I say an active preparation advisedly, because many of the liquid extracts of ergot on the market have no physiological action on smooth muscle. When once a prolapse is firmly established, pessaries or operative treatment are the only alternatives. Operations are imperative in the cases with such widely relaxed vaginal orifices that no pessary can be retained. It may be remarked here that ring pessaries often cannot be retained because they are too pliable, and any abdominal strain squeezes their sides together and they slip out. This may be overcome by having your pessaries made with about *nine* turns of a watch-spring instead of *three*, which is the usual number. With regard to operative measures, remember that all perinæal tears, however small, should be sutured *at once* after delivery. No perinæal tear ever heals except by granulation if it is not sutured. When a case of prolapse is to be treated by operation, it is not sufficient to repair an old perinæal tear. As a rule the cervix is hypertrophied and lengthened, and consequently must be amputated above the vaginal vault. The anterior vaginal wall is stretched as a cystocele and therefore demands anterior colporrhaphy. The posterior vaginal wall is redundant and requires posterior colporrhaphy before the perinæum can be repaired. All these operations are required in most cases, and should all be done at one sitting with scrupulous asepsis. It is useless to perform ventro-fixation or any such operation for prolapse unless all these plastic operations are done as well, and finally, removal of the uterus is no good, because the uterus has nothing to do with the causation of the condition. If good results are to be obtained from operations for prolapse, there is no doubt they must be performed in the early stages, before the condition has existed too long.

Dysmenorrhœa is perhaps the most difficult condition we have to treat in all the range of gynæcology, chiefly, I think, because we have no definite pathological lesion which can be demonstrated in the majority of cases. The painful menstruation of young unmarried girls or of married women who have no children forms a class practically standing by itself. In these people, as a rule, the pain begins with the flow or only an hour or two before it. The pain is spasmodic, griping, or colicky in character and is only relieved after it has been in progress for a number of hours up to two days. An examination of these patients, if it is advisable, usually reveals no definite pathological lesion, at all events of an acquired nature. What, however, is generally found is an undeveloped condition of the uterus as shown by its small size, and some malformation of the organ as shown by an undue anterior bending, and a long cervix with a pinhole os. These changes, however, can hardly be looked upon as the causes of menstrual pain, unless we believe that the cervical canal is stenosed and causes obstruction. We do not believe this now, but we rather believe that the pain is set up by an undeveloped condition of the uterine muscle, leading to imperfect spasmodic and painful uterine contractions in the efforts of the uterus to expel its contents. We must not forget that the uterus at all times has to expel its contents, and consequently a sound musculature is necessary. Further, a strong uterine muscle is required to prevent undue congestion of the organ during the premenstrual engorgement. From this it follows that a badly developed muscle will cause pain in two ways—first by allowing too great a congestion of the organ, and secondly by only contracting in a spasmodic, colicky and jerky manner. The proof that the pain is in some way due to the muscle of the uterus appears to me to lie in the natural cure of spasmodic dysmenorrhœa, namely pregnancy and labour. Pregnancy develops the uterine muscle to the highest degree possible, whilst labour stretches the lower segment to the utmost and at the same time educates the uterus in the relative importance of relaxation of the lower segment whilst the upper zone is contracting. Unfortunately so many of the subjects of spasmodic dysmenorrhœa are also sterile, and it appears to me that in many instances the same condition of the uterus is responsible for both symptoms. If we inquire carefully into the causation of sterility in these people, it seems to me that two possible causes are present in a large proportion of the cases, assuming that the ovaries and tubes are normal and that the husband is capable of procreation. Either the spermatozoa have a difficulty in getting into the uterus at all, or the fertilised ovum is unable to embed itself in the endometrium. I think these two possibilities must be admitted when we consider the effects of local operative treatment in these cases. Unfortunately we do not know exactly how the spermatozoa do get into the uterus, whether it is by their own activity or by some suction action of the uterus itself associated with muscular contraction. However, we do know that simple slitting of the cervix alone will sometimes lead to impregnation, so that it looks as if the pin-hole os were to blame in some cases. In other cases slitting the cervix has no effect, and then thorough currettage is followed by pregnancy. Why is this? I have always believed that along with the obvious malformation of the uterus as a whole, it is quite likely that the texture of the endometrium is abnormal. I cannot prove my point by histological evidence, but it seems to me to be more than likely that small changes in the endometrium of a fibrous nature may prove to be a bar to the embedding of the fertilised ovum. I am not at all sure, too, that some change in the endometrium is not an essential part of the causation of spasmodic dysmenorrhœa; for if the capillaries, engorged with blood, are not able to easily rupture under the menstrual stimulus, they will act like any other foreign body in the uterus and cause painful contractions. Following these theories out in practice I have always made a point of curetting the uterus when I operate for spasmodic dysmenorrhœa. I do not believe that simple dilatation of the cervix often gives relief from pain for more than two or three months, and very often for one month only. I find, however, that after dilatation and curettage relief from pain is much more permanent. It is to be admitted that this relief is not absolute; practically none of the patients have *no* pain, but most of them are very much relieved permanently. In married women or women about to be married I always slit the cervix backwards as well, putting in catgut sutures to prevent adhesion of the cut surfaces. I believe that on the whole I get better results from

dilatation, curettage, and slitting the cervix, than from simple dilatation alone.

With regard to the palliative treatment of this type of dysmenorrhœa, there is very little to be said which is novel. The coal-tar products, especially phenazone, are the most generally useful drugs combined with general treatment for constipation, anæmia, and dyspepsia, with the addition of daily cold baths, healthy exercise, and occupation. We do not yet realise the parts which the ductless glands play in the menstrual functions, including among them the ovary, thyroid, supra-renal, and pituitary glands. Seeing, however, what important influences the secretions of these glands have on the circulation in particular and the economy in general, it will not be surprising if we find that in the future we are able to influence menstruation through their judicious exhibition.

I still hold that there is a congestive type of dysmenorrhœa which shows itself by premenstrual pain, and is associated with definite lesions of the sexual organs. This type is best exemplified in true endometritis, in some fibroids, in retroversion with endometritis, and in cases of salpingo-oöphoritis. All these conditions are more common in women who have had children, and who may have had, and have been cured, of spasmodic dysmenorrhœa. In these cases the pathological changes in the uterus itself, or in the peritoneum covering it, lead to a want of expansile power, so that great tension in the vessels results from the premenstrual congestion, and pain results. This pain is acheing and constant in character, not spasmodic and colicky as in the preceding type of case; also, in a typical case it is relieved by the flow, as we should naturally expect. There is a tendency nowadays under the influence of the teaching of one or more authorities not to regard this type of pain as dysmenorrhœa at all, but that it is actually associated with the function of menstruation cannot be denied, although it precedes the flow, and for this reason it seems to me to be worthy of retention in our nomenclature. Its treatment fortunately is more defined and successful than that of spasmodic dysmenorrhœa, because here we have definite lesions to deal with.

February 20th, 1911.

ABSENCE OF ABDOMINAL RESPIRATORY MOVEMENT AS AN INDICATION OF PERICARDITIS.*

By W. ESSEX WYNTER, M.D., F.R.C.P.

In a disorder so serious and attended with so little obvious indication as pericarditis, an additional and manifest physical sign, provided it is to be trusted, should be especially welcome.

The sign suggested in this communication is inhibition of the action of the diaphragm, indicated by suppression of normal abdominal respiratory movement, and has probably been hitherto overlooked through examination being unduly restricted to the chest in such cases.

Loss of abdominal respiratory movement as an indication of disease, except in paralysis of the phrenic nerves, has been to a great extent monopolised by observers in connection with peritonitis and acute abdominal lesions.

It is largely from this standpoint that Dr. William Pasteur dealt with this phenomenon and the resulting thoracic lesions in his highly instructive Bradshaw Lecture.

Primary thoracic disease, however, has its share in causing suspended function of the diaphragm, though in the majority of instances, affecting the lung and pleura, this is one-sided, and the influence on abdominal movement consequently less marked.

In acute pericarditis, on the other hand, the cessation of phrenic action is bilateral, and consequently absence of abdominal movement is as obvious as in abdominal disorders, albeit other abdominal manifestations, such as muscular rigidity and general tenderness, are lacking.

The two instances I wish to mention first bring out this point with great force, in that the loss of abdominal movement led to mistaken diagnosis of abdominal lesions, and was only ascertained subsequently to indicate pericarditis, the observation being therefore entirely impartial.

The former of these, seen in October, 1905, was that of a young man in the service of a medical friend, who had been seized with acute epigastric pain while waiting at table. Conspicuous absence of abdominal movement led to the supposition that

* Delivered before the Royal Society of Medicine.

he had a perforated gastric ulcer, and I was asked to take him into the Middlesex Hospital for operation.

The observation and inference had been made by an independent local practitioner, and were quite disinterested as regards any connection with pericarditis, which was not thought of at the time.

When I saw the man next morning the acuteness of the pain had diminished; he had enjoyed some sleep and was obviously better, clamouring for food and impatient of restraint. There was no evidence of free gas in the peritoneum or of local irritation and it was considered best not to move him. A routine examination revealed nothing. Next day he got up and went out of his own accord, but experienced a recurrence of his pain. When re-examined on the fourth day pericardial friction was quite obvious, and I received him under my own care in a medical ward. By that time there was marked dyspnœa with præcordial pain, the abdomen remaining immobile. Pericardial friction was audible four days. Temperature 101·4° F., pulse 96. On getting up a week later the temperatare rose to 103° F. and the pulse to 104 but no further development occurred, and he was able to leave the hospital ten days later, after a stay of just three weeks.

The second case was somewhat similar, except that I shared with a surgical colleague a mistaken diagnosis.

On November 2nd, 1909, A. B—, a man. æt. 40 years, was admitted with complaint of pain in the abdomen extending into the thighs, headache, anorexia, vomiting; and parched mouth of two days' standing.

The temperature was 102·6° F.; pulse, 92.

He lay on his back with the legs drawn up, the right rectus abdominis was tense, and there was tenderness in the right iliac region. Abdominal respiratory movement was scarcely discernable.

A routine state on admission was taken out, but nothing more definite could be found than a reduplication of the second cardiac sound. Certainly no local indication of pericarditis then existed.

A consultation was held, and the almost inevitable conclusion arrived at that the case was one of appendicitis.

Symptoms subsided with rest; in the course of the next three weeks abdominal movement was more apparent, and advantage was taken of this quiescent period for surgical interference. Five weeks after the original attack (December 7th) appendectomy was performed. But careful examination revealed no morbid condition, and it was not adherent. The incision healed rapidly, but after five days there was a rise of temperature with recurrence of symptoms, and on December 12th pericardial friction was well marked, the heart's apex being in the fourth space in the nipple line. Pulse, 90–100.

The diaphragm was not contracting at all, the abdomen actually sinking in with each inspiration.

The stomach was dilated, and its resonance could be traced as high as the fourth intercostal space in the axilla.

Breath-sounds were absent at both bases, but there was no complete dulness on percussion.

On December 16th Mr. Cecil Lyster supplied a report under X-ray screen examination to the effect that there was a large shadow in the heart region suggestive of pericardial effusion. The diaphragm was motionless in the position of expiration.

On December 22nd diaphragm movement was returning, though friction could still be heard over the heart. Breathing became audible at both bases. A week later friction had disappeared, but a systolic apex murmur was audible. The man was discharged on January 8th.

Both these cases afford examples of defect or absence of abdominal movement, being observed quite independently of any idea of pericarditis, which only manifested itself by the usual clinical signs later. They also illustrate temporary remission of symptoms attending rest in bed without special treatment, and suggest that inflammation of the pericardium, possibly commencing in some other part than that immediately in contact with the chest-wall, indicated its presence by reflex inhibition of the diaphragm some days before the appearance of ordinary signs.

On December 1st, 1905, C. D—, male, æt. 21 years, with a history of rheumatism six years, and also three months before, was admitted with endocarditis and pericarditis. Temperature, 102·2° F.; pulse, 130; respirations, 36. There was a double aortic bruit, and in addition, the bruit di Galloppi and friction were heard over the præcordia. Abdominal respiratory movement was absent. By December 4th these active signs subsided, and there was a return of abdominal movement, but

joint pains continued till December 11th. He was discharged a month later—January 12th.

On September 22nd, 1906, S. N—, male, æt. 9 years, was admitted on account of pain in the epigastrium of one week's duration, extending to the left clavicle. Tonsillitis had also existed five days. Temperature, 102·6° F.; pulse, 120. The boy lay with his arms above his head, flushed, and breathing rapidly.

The præcordial dulness was extensive and described as pear-shaped. Heart-sounds were muffled. There was a faint mitral regurgitant murmur with accentuation of the pulmonary second sound.

Friction could be heard all over the pericardium but was loudest over the fourth costal cartilage and sternum. There was tenderness on pressure in the epigastrium and the abdomen moved very little with respiration. There was slight rigidity of the recti abdominis. By October 2nd friction had subsided but there was a recurrence of tonsillitis, and he was not discharged till November 10th.

On June 6th, 1907, E. F—, a girl, æt. 16 years, was admitted suffering from rheumatism and tonsillitis of a fortnight's duration. She improved speedily, but on getting up there was a relapse with pain in the epigastrium. Temperature, 102° F.; pulse, 120; respirations, 50. Præcordial dulness extended laterally one and a half inches to the right of the sternum, outside the nipple line to the left and upwards to the third rib cartilage. Pulsation was felt in the third left intercostal space. The observation on abdominal respiration was that, "when the chest rises in full inspiration, the abdomen falls in and *vice versâ*." She was discharged convalescent June 26th.

On June 14th, 1907, G. H—, a boy, æt. 6 years, who had been ill five weeks, was admitted with præcordial pain. Temperature, 103° F.; pulse, 144; respirations, 60.

There was definite extension of præcordial dulness, and the heart's impulse could be felt in the third and fourth left intercostal spaces. The diaphragm did not move with respiration, and breath-sounds were indistinct at both bases. The boy made a good recovery and was discharged on July 3rd.

On October 5th, 1907, I. J—, a boy, æt. 13 years, was admitted with joint pain of fourteen and præcordial pain of five days' standing. He had rheumatic fever twelve months and three months ago. Temperature ranged from 102°–104° F., pulse 130. No endocardial murmur was heard, nor any friction. Three days later marked immobility of the abdomen was noticed. There was some extension of præcordial dulness one inch to right of sternum. The apex was in the fifth space. Soft pericardial friction could be heard which continued for ten days, the temperature reaching 101° F.

A month later with a relapse friction reappeared, November 17th, and lasted five days. He was discharged December 2nd.

During the past year, 1910, which was prolific in rheumatic affections, five cases occurred in which notes were made concerning the state of the diaphragm with three screen examinations.

On March 22nd, K. L—, a girl, æt. 12 years, was admitted with inflammation of an adherent pericardium. The heart had been affected in an attack of rheumatism and chorea at nine years.

For three weeks there had been pain in the front of the chest, with cough, vomiting and dyspnœa. The apex was two inches outside the nipple line in the sixth space, the impulse extending over a large area of the præcordia with systolic retraction, and there was a mitral regurgitant murmur.

The aortic second sound, though loud at the base, was almost inaudible at the apex. The pulmonary second was accentuated. The heart apex was anchored, not shifting at all with change of position. Pulse 146.

Some friction was heard at intervals over the heart.

The diaphragm was completely inactive. Resonance was impaired at both bases posteriorly, when breathing was scarcely audible. At the end of April there was an intercurrent attack of diphtheria and she was not discharged till May 24th, having been in the hospital just over two months.

On May 16th, M. N—, a man, æt. 21 years, who had left the hospital only one week before after rheumatism, which had also stricken him on three previous occasions, was found to be breathing without his diaphragm. Friction was heard over the base of the heart, the apex was in the nipple line, and there was a systolic apex murmur. Temperature, 102° F.; pulse, 120; respirations, 30.

Five days later, May 21st, the diaphragm was

beginning to act, though friction was still audible at the cardiac base. He was discharged convalescent on June 14th.

On June 7th, O. P—, a girl, æt. 7 years, was admitted with rheumatism of a fortnight's duration. She suffered from scarlet fever and chorea at six years. Dulness extended between the right and left nipple lines, laterally and vertically, from the second rib cartilage to the ensiform. Friction could be heard at the base, and there was a systolic apex murmur. Temperature, 103° F; pulse, 136; respirations, 64. Abdominal movement was well marked. Fever lasted till June 21st.

On the 25th a screen examination was made, showing that the area of cardiac shadow was very large, but only the middle portion showed pulsation, indicating the presence of effusion. Contraction of diaphragm was *normal.*

A second examination, before the child left on July 8th, a month after admission, showed pulsation extending to the full limit of the cardiac shadow, indicating that the effusion had cleared up. The heart shadow was still very large (dilatation and adherent pericardium).

On June 22nd, R. S—, a boy, æt. 14½ years, was admitted with rheumatism and mitral regurgitation. He suffered from rheumatism and scarlet fever three years before. Five days after admission, June 27th, friction was detected over the heart, chiefly at the base, and lasted three days. Examination by the screen showed an enlarged heart, the whole area pulsating, so it was concluded there was no effusion. The diaphragm moved very slightly—estimated at a quarter of an inch.

A temperature of 103–102° F. continued for the first five days in spite of salicylates internally, but fell on the 27th, when salicylates were applied to the chest.

On July 13th, T. V—, a youth, æt. 17 years, was re-admitted twelve days after being in the hospital for rheumatism, on account of chest pain and dyspnœa, which had developed subsequently. Temperature, 100° F.; pulse, 110; Respirations, 40.

The apex was in the sixth space, nipple line. There was a systolic apex murmur, and pericardial friction was audible at the base.

A screen examination showed that the cardiac shadow occupied the middle third of the chest. The shadow extended outside the pulsating area on the upper and right borders, indicating effusion. The diaphragm moves slightly on the left side and not at all on the right. He was discharged convalescent on August 19th.

A dozen cases afford but a limited basis for extended inference, and these do not include all that came under observation, but with frequent change of assistants it is a difficult matter to obtain uniform notes, and in several the particular point was not alluded to, showing that it is not at present a prominent feature in medical teaching. There is no doubt, however, that when attention is directed to the matter, and a definite note made as to the presence or absence of abdominal movement, this feature will be found fairly constant in acute pericarditis, especially that with fibrinous exudate.

When the front of the body is fully exposed, the stillness of the abdomen is very striking, suggesting peritonitis, unless, as sometimes happens, there is inspiratory recession, which might mislead unless the want of correspondence with the movement of the upper chest is noticed.

The inertness of the diaphragm is not merely inferred, but by X-ray screen examination can be actually observed, the midriff mounting high into the thorax and exhibiting no contraction, and in some instances an actual rising of a quarter of an inch with each inspiration.

In one case the contents of the upper abdomen, such as the kidneys and opaque masses in the intestine, could be seen to participate in this upward inspiratory movement.

Concomitant, and, indeed, consequent phenomena, are a tendency to dilatation of the hollow abdominal organs, especially the stomach, collapse and loss of function in the lower lobes of the lungs, slight upward displacement of the heart, and of the organs beneath and in contact with the diaphragm.

Loss of abdominal movement, as a sign of pericarditis, is the more valuable in that it may precede and outlast the other indications and so afford grounds for anticipatory or retrospective diagnosis. The condition affords a reasonable explanation of the cyanosis and dyspnœa so usual in the complaint and of the upright position so commonly assumed by the patient, the former expressing the effect of respiration restricted to the function of the upper lobes of the lungs, and the latter pointing to an unconscious effort on the part of the patient to utilise the weight of the abdominal viscera to

depress the passive diaphragm and enlarge the thoracic area.

Restriction of abdominal movement is apparently more marked in fibrinous than in effusive pericarditis, movements being more productive of local irritation in the former than in the latter condition, as is the case in other serous sacs, such as the pleura and peritoneum.

Incidentally, the observation of the existence of a limited amount of serum in at least one of the cases by X rays, in the absence of other clinical indications, is suggestive that extended observations may show this to be more frequent than was formerly supposed, for in the recumbent posture a few ounces of fluid would naturally gravitate to the lowest part of the sac, and scarcely reveal its presence over the front of the heart.

So far these observations have been restricted to rheumatic pericarditis, mostly associated with polyarthritis, though some have been independent.

The paucity of screen observations has been due to the comparative infrequency of the cases, and the inadvisability of disturbing patients so seriously ill, especially in the acute stage.

It is, however, the conviction of the writer that where loss of abdominal respiratory movement is well marked in the absence of more obvious thoracic and abdominal lesions, pericarditis should be closely looked for, and where this sign is wanting dubious indications of pericarditis may be discounted.

As none of the cases cited proved fatal or required surgical operation, this aspect of pericarditis was unsuitable to the recent discussion on the subject at this society.

When the matter first presented itself, a reference to current descriptions of the disorder showed that the feature described was not generally recognised, but it has since been pointed out to me that Gibson, in his text-book published in 1901, makes this statement:

"Interference with the action of the diaphragm is sometimes observed, and leads to discovery of pericarditis. It is obviously due to reflex action, and may or may not be associated with painful sensations."

Commenting on this, I should say that, since I was aware of it, I have not seen an acute case which failed to show this sign, that it is perhaps less obvious in effusion and not present in adhesion of the pericardium.

But that its importance as an aid to diagnosis, especially in the more obscure cases, has been under-stated and generally unappreciated, future observations will, I am sure, demonstrate.

Dr. PERCY KIDD said he had noticed abdominal rigidity in cases of pericarditis, but he could not say he had constantly looked for it in that association, but he would do so now that he had heard the paper. Dr. Hutchison had just raised the question to him whether the abdominal rigidity was present in every case of pericarditis. Possibly where the diaphragmatic surface was chiefly affected one might expect to find this inhibition of the diaphragm, but that was a matter for further observation. He had a strong conviction that he had several times seen a very motionless abdomen in association with pericarditis, but thought the rigidity was apparently due to the pain which was elicited. He had but little to say on the matter, as he had come to learn.

Dr. M. A CASSIDY said he was glad to hear Dr. Essex Wynter lay stress on the fact that pericarditis might be confused with acute abdominal lesions, because at least twice during the last three years he had seen cases sent up to St. Thomas's Hospital as acute abdominal lesions which subsequently turned out to be pericarditis. One of the cases he regarded as of sufficient interest to relate to the meeting. A girl, æt. 20 years, was sent to Dr. Mackenzie's ward at St. Thomas's on October 15th, 1909. She gave a past history of anæmia and dyspepsia, but she had never had rheumatism. A fortnight ago she had what was described as influenza, with tonsillitis and pains in the limbs; three days ago she had a sudden attack of severe abdominal pain, chiefly epigastric, associated with frequent vomiting. Her medical attendant diagnosed a perforated gastric ulcer, administered morphia, and sent her up to the hospital for immediate operation. On admission the temperature was 101·6° F., pulse 104, respirations 32. She looked ill, but her general appearance was not suggestive of peritonitis. The abdomen was almost motionless, respiration being entirely costal in type. The abdominal wall was tender and resistant everywhere, but more so in

the epigastrium. There were no signs of peritoneal exudate nor distension of the abdomen, and the liver dulness was normal in extent. The cardiac dulness was normal on admission; there was a soft basal systolic murmur and a rough apical systolic murmur, not conducted. The tongue was moist and furred. In this case the difficulties in diagnosis were increased by the fact that morphia had been administered before the patient came under observation. The question of immediate laparotomy was seriously considered, but eventually it was decided to await further developments before adopting surgical treatment. For several days her symptoms and physical signs were entirely abdominal, and it was not until some ten days after admission that the rheumatic origin of the case was suspected. Her area of cardiac dulness was found to increase in extent, and at the same time bruits suggestive of pericardial friction made their appearance. A fortnight after admission there were signs of pulmonary collapse at both bases, and the cardiac dulness extended from the second rib, and from one inch to the right of the sternum to the left fourth intercostal space half an inch outside the nipple line. X-ray examination revealed an enormous heart, or pericardium, the departure from the normal dimensions being chiefly in the transverse direction. The diaphragm was elevated and almost motionless, and the base of the left lung was opaque. The pyrexia and other symptoms responded promptly to the administration of salicylates, and a further confirmation of the diagnosis was afforded by the appearance of a crop of typical rheumatic nodules on December 3rd.

Dr. NATHAN RAW said he was not aware of the symptom which had been so fully and ably described by Dr. Wynter in ordinary cases of acute pericarditis. But since receiving notice of the paper to be read he had observed in hospital the case of a girl, æt. 18 years, who had a very acute attack of rheumatism, and who, a week after the onset of the attack, developed acute pericarditis. He observed her very carefully, and the abdomen was quite fixed, the recti muscles tense, and on ordinary respiration the abdomen was practically motionless. The girl had very acute pain, referred to the præcordial region. Whether the pain was the cause of the almost absolute fixation of the diaphragm and abdominal muscles he could not be quite clear, but after an injection of morphia to relieve the acute pain the abdominal muscles moved somewhat easier. Still, the abdomen itself was practically fixed, and therefore, although that was the only observation he had made in acute pericarditis, he was glad to be able to confirm the symptom which had been so ably expounded.

Dr. ROBERT HUTCHISON said he was very much interested in the paper, because a month ago he saw a case very much like that mentioned by Dr. Wynter. The patient was a boy, and he was exactly like an abdominal case: everyone would have said it was a case of "acute abdomen." The patient was lying with the legs drawn up, with a rigid belly and a rapid and shallow respiration, the diaphragm not moving. He was certain the boy had no evidence of heart or lung trouble, and it was thought to be a case of abdominal peritonitis. A surgeon saw him, but decided to wait until the following day before doing anything. By the next day there had developed a very loud pericardial friction-sound. But the rigidity of the abdomen did not seem to occur in all cases of pericarditis. At the same time as the above case he had two other children of about the same age in the ward. They also had pericardial friction well marked, but neither of them had the immobility to anything like the same extent. Having seen the other case he was on the look-out for pericarditis. He would like to hear whether Dr. Wynter could say why the diaphragm was inhibited in one case and not in another. He had just been suggesting to Dr. Kidd that it was probably when the diaphragmatic surface of the pericardium was involved that this happened just as in diaphragmatic pleurisy. In some of the cases described by Dr. Wynter there was a curious interval, in some cases amounting to days, before pericardial signs showed themselves at all. He would be glad to hear any explanation of this. The Section was much indebted to Dr. Wynter for bringing the matter so prominently forward, because he was sure its importance had been largely overlooked in the text-books.

Dr. VOELCKER desired to support what Dr. Hutchison had said about the inconstancy of that sign in cases of pericarditis. He had under his care at Great Ormond Street only a short time

ago a child who had pericarditis, but there was free movement in the upper part of the abdomen. He would not have remarked on that point if Dr. Hutchison had not raised the question as to whether the restriction of movement was met with in those cases in which the diaphragmatic surface of the pericardium was affected. In his case, although the clinical picture pointed to pericarditis, the only place in which friction-sounds could be heard was in the subcostal angle, and this would point to the existence of roughening on the diaphragmatic surface of the pericardium. In this case there was an absence of any inhibition of movement in the upper part of the abdomen. The question of pulsation had been referred to by Dr. Essex Wynter. In one of his cases Dr. Wynter had described abdominal movement as being present, and pulsation was observed by the screen in the central part, and from that it was inferred that effusion was present. That was an interesting point, because the question of the presence or absence of pulsation as the result of a screen examination he had rather looked upon as being a point in the differential diagnosis between a dilated heart and the presence of pericardial effusion, because, as a rule, in pericardial effusions there was an absence of pulsation, while in a dilated heart the screen showed pulsation in the shadow. Dr. Wynter, only two days ago, very kindly saw a case in Middlesex Hospital with him (Dr. Voelcker), in which there was an absence of movement in the upper part of the abdomen, and in which, with the screen, there was seen to be a very definite shadow without any pulsation. The question of that pulsation was very important in the differential diagnosis, because everyone knew that the distinction by the ordinary methods of physical examination between dilated heart and pericardial effusion, however clearly they might be differentiated in books, was not always so simple at the bedside. He remembered joining in a procession which went to the post-mortem room to see a case which, fortunately, was just too bad for the surgeon to operate upon with the object of freeing the adhesions which were binding the heart down, but at the post-mortem examination there was no pericarditis; it was simply a case of dilated heart. Therefore it was possible for skilled observers to make a mistake between pericardial effusions and dilated hearts. With regard to mistaking pericarditis for abdominal lesions, one sometimes saw the opposite side of the story. At Christmas time a child was admitted into Middlesex Hospital, and in that case, owing, perhaps, to the attention which had been drawn to that absence of abdominal respiratory movements in pericarditis, the case was regarded as an obscure one of either pericarditis or of pneumonia. There was a limitation of abdominal respiratory movement, and there were no definite signs in the chest. In that case the movement was more limited in the lower part than in the upper part of the abdomen, so that appendicitis was diagnosed, although there was no mass to be felt in the abdomen. At the operation a perforated appendix with peritonitis was found, and the child made a good recovery.

Dr. FAWCETT said he quite realised the value and importance of Dr. Wynter's observations, but at the same time he advised caution in diagnosis in the early stages of a case which at first sight appeared to be an "abdominal" one. When the physical signs of pericarditis appeared only after a considerable interval, as, for example, in the first cases quoted, it might possibly be that the deficient movement of the diaphragm in the commencement of the illness was after all due to a localised peritonitis, the evidence of which became less obvious as the pericarditis developed. Probably the President could recall cases such as one that had recurred to the speaker's mind, where a child was admitted to hospital with signs suggestive of peritonitis in the upper part of the abdominal cavity, but where the development later of pericarditis and pleurisy was thought to explain the early abdominal signs. Nevertheless, the autopsy showed that there was a recent peritonitis in addition, leaving little doubt that to this was to be ascribed the initial abdominal condition. In this group of cases a bacterial infection of more than one serous membrane occurred, for example, as in an invasion by the pneumococcus, where the evidence of infection of the peritoneum might be much less certain than that of the pleura, or pericardium, and these cases must always be borne in mind.

The PRESIDENT desired to ask Dr. Wynter two questions: First, was there pleurisy present in any one of those cases, especially the rheumatic ones? It seemed remarkable there should have been so

many cases of rheumatic pericarditis without, as far as he knew, any reference to pleurisy, because it was common to find pleurisy in rheumatic children. In the next place, he asked whether the high inspiratory position of the diaphragm appeared to him to account for the situation of the præcordial impulse in acute pericarditis. It was known that the præcordial impulse in acute pericarditis with effusion lies upwards and outwards from the normal, and it had always been a subject of discussion how the præcordial impulse should be raised towards the axilla. The subject raised by Dr. Fawcett appealed to him (Dr. Bruce). It had occurred to him that the first two cases which Dr. Essex Wynter related were at first genuinely abdominal, and that the pericarditis might have been due to spread of some abdominal inflammatory process through the diaphragm, as it was known to spread occasionally, into the pericardium. Happily, both cases ended in recovery, so one could not test the diagnosis. Such might well have occurred, because, as Dr. Fawcett said, there was a remarkable interval between the first symptoms and the development of pericardial friction. The others were ordinary rheumatic cases.

Sir JOHN BROADBENT referred to the association of pleurisy and pericarditis. Recently he was asked to see a case in the wards in which there was marked immobility and extreme rigidity of the upper part of the abdomen, but on deep inspiration there was movement of the lower part of the abdomen. It appeared to be a spastic rigidity instead of the paralysis met with in peritonitis, and was localised to the upper part of the abdomen. In this case there was pleurisy as well as pericarditis, and it proved fatal. He asked whether, in the cases of pericarditis referred to by Dr. Wynter, there was not some movement of the lower part of the abdomen in deep inspiration though the upper part was rigid?

Dr. ESSEX WYNTER replied that there was no movement in the abdomen at all. When phrenic contraction was suppressed, he did not see how, mechanically, there could be movement of the lower abdomen, unless it were an inverse effect of aspiration. It was not true paralysis of the diaphragm, but suspended function. With regard to pleurisy, in answer to the President's question, he did meet with cases of pleurisy, but he was disarmed in respect of these, because he knew if he cited cases in which there was pericarditis and pleurisy it would invalidate the argument, for everyone knew that basic pleurisy, at all events, arrested the action of the diaphragm on its own side. He had one case, however, which, after pericarditis, developed first left and then right pleurisy, and eventually recovered. But he thought a case of that kind was too mixed to be admissible in the argument which he was bringing forward. With regard to the apex of the heart, the screen examination showed that with the rise of the diaphragm there was some elevation of the heart, but it was only slight, and he considered that in general one did not see much difference in the position of the apex in fibrinous pericarditis. There was a much grosser cause of displacement where there was a large effusion, in that the heart was floated up and rotated somewhat outwards. He felt very grateful to the other Fellows of the Society who had supplied so much evidence, which confirmed the important connection between the loss of abdominal movement and pericarditis. He did not consider that what Dr. Hutchison observed with regard to the interval between the primary attack and the recognition of the pericarditis invalidated the point at all. It really substantiated the suggestion which Dr. Hutchison previously made, that probably the arrest of abdominal movement was due to involvement of the diaphragmatic surface of the pericardium, and it seemed only reasonable that if the pericarditis began on the diaphragmatic surface, loss of abdominal movement would precede the recognised friction over the surface of the heart, which would come later. Of course, it was possible for peritoneal troubles to extend through and infect the pericardium, or for them both to be due to the same infection, but he thought the case in which he gave himself away, that of mistaken diagnosis, was rather sound evidence against that, at all events in that case, because the man had had his initial symptoms five weeks before laparotomy was performed, which afforded fairly complete evidence that the abdominal cavity was normal. It might be suggested that the occurrence of peritonitis five days later was an outcome of the laparotomy, but that would be unkind to the surgeon, and it was not borne out by the issue, because on treating it on ordinary

lines by the local application of salicylates all symptoms subsided in three days. He was not prepared to say that every case of pericarditis would present the symptom to which he had drawn attention, but, without having thought of it much before, he had a fair list of cases which showed a very exaggerated degree of it, and if it were looked for more often he thought it would be found fairly constantly. It was striking that one case in the paper which had full effusion presented a normal action of the diaphragm, forming a strong contrast to the others. But fluid in the pericardium was not likely to interfere so much. It produced pressure, but it freed the surfaces from irritation, and he thought it was found that in the peritoneum there was much more restriction while the exudate was comparatively dry than when there was effusion. He could not claim more than independent observation in the matter, but it had not so far been sufficiently insisted upon.

February 20th, 1911.

BROAD LIGAMENT MYOMATA.

BROAD ligament myomata are divisible into two classes. The first variety is the true ligament myoma, and springs from the muscle-fibres normally found in the mesometrium. These tumours may therefore be found in at least three situations: (1) In the round ligament; (2) in the ovarica-uterine ligament; (3) in the connective tissue surrounding the ovarian or uterine vessels. As a rule tumours growing in the first two situations are of small size, and can be enucleated on ordinary principles. Tumours growing in the third situation frequently attain a large size; they distend the broad ligament so that the Fallopian tube is stretched and lies sessile on their upper surface as in a broad-ligament cyst. True tumours of the broad ligament can be distinguished by the fact that they are entirely separate from the uterus, which they displace, but do not deform. Their relation to the uterine artery should be remembered; it lies beneath and on the inner side of the tumour, while the ureter is displaced inwards, and will be found running in the posterior peritoneal layer of the broad ligament, after leaving which it courses under the tumour to reach the bladder. There are two methods of dealing with these broad-ligament myomata. If the tumour is small it may be enucleated. If the tumour is very large, vascular, or adherent, it may be necessary to remove the uterus in addition, principally as a means of easily controlling the hæmorrhage. The second variety may be termed the "false" broad-ligament myoma. In this case the tumour springs from the lateral wall of the uterine body or of the cervix, and bulges outwards between the layers of the broad ligament. The uterus is therefore part and parcel of the tumour. These tumours distend the broad ligament, and also at times raise the lateral pelvic peritoneum and invade the meso-colon. Besides their relation to the uterus, they differ from the first variety in that they displace the uterine artery outwards and upwards, so that in extreme cases the uterine and ovarian vessels are approximated and run a parallel course on the top of the tumour. The ureter is displaced outwards to the pelvic wall, and, as a rule, lies under the tumour, except in the rare lateral cervical myomata already referred to, when, together with the lateral angle of the bladder, it may be undermined by the tumour and elevated on its upper surface. These tumours, when small, can sometimes be enucleated, but when large they must be dealt with as follows: It is better to begin the removal of the tumours by attacking the healthy side of the uterus. The greatest difficulty in these cases is the control of the uterine vessels on the side of the tumour, and it is often impossible to secure them until the tumour is removed from the field of view. The concluding stage of its removal has, therefore, often to be effected as quickly as possible, and this is materially aided by the previous amputation of the uterus and clamping of the uterine vessels on the healthy side.

The above is taken from 'A Text-book of Gynæcological Surgery,' by Dr. Comyns Berkeley and Dr. Victor Bonney, published by Cassell & Co. There are 392 figures in the text and sixteen coloured plates, making the work not only a most useful and ready means of reference, but also a sure guide for the perplexed practitioner lost in the maze of modern gynæcological terminology.

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NERVOUS EXHAUSTION.*

By W. H. B. STODDART, M.D., F.R.C.P.,

Lecturer on Mental Disease to the Westminster Hospital; Resident Physician and Medical Superintendent, Bethlem and Bridewell Royal Hospitals.

Ladies and Gentlemen,—You are all familiar with an ordinary muscle-nerve preparation, and with the experiment in which the nerve is stimulated electrically until the muscle fails to respond to the stimulus, the result being that the muscle becomes paralysed, and suffers from what we term the "paralysis of fatigue." The nervous system suffers in somewhat the same way. The symptoms of fatigue which follow a hard day's work, physical or mental, are, of course, very well known. A feeling of heaviness is experienced in one or other part of the body. With some people the centre of heaviness is in the head; with others it is in the legs; with others in the chest; and with all, probably, in the eyelids. Further, some people suffer to a certain extent from pain. The general result of all this is a tendency, more or less marked, to sleep. All this is, of course, a normal process.

By exhaustion of the nervous system, however, one understands a state of affairs in which this process has gone too far, the fatigue having become pathological. It has been put in this way: in normal fatigue the nervous system becomes paralysed by the fatigue products; in pathological exhaustion—exhaustion is always to be regarded as pathological—the individual neurons of the nervous system become more or less disintegrated and damaged, at least *pro tem.*

The symptoms are characteristic and considerable in number, and we group under the heading of "nervous exhaustion" a number of cases the ætiology of which is varied while the symptoms are similar.

* A Clinical Lecture delivered at the Polyclinic, Chenies Street, W.C.

They are caused by intoxications of various kinds. Alcoholism is one of them, and perhaps the most familiar variety of the particular disease that I am about to describe is delirium tremens. The same series of symptoms with slight variations is seen in the case of belladonna poisoning, hyoscyamine poisoning, and some others. The symptoms are also seen in febrile conditions, both during and after the fever. They follow upon overwork, either physical or mental; they follow upon worry; and I have seen the same identical symptom-complex arising in cases of mental shock. The shock, for instance, may be such as a person would receive upon waking and finding a bedfellow dead, or upon witnessing the sudden accidental death of a workmate. Although we have rather a varied ætiology, we have a great similarity of symptoms, and consequently we may group thes e cases under one heading. "Exhaustion psychosis," "infection psychosis," and "confusional insanity" are some of the names which have been applied to this condition.

In studying any disease it is well to apply the rule—a rule originally applied more generally by John Stuart Mill—of studying first of all the most advanced, most striking and severe examples. If we take an advanced case and study it we shall discover those symptoms which are most marked, and then, having decided for ourselves what symptoms are present, we can set to work upon milder cases and discover whether the symptoms are present in these also, although in a milder degree. Therefore, in order to get a clear conception of nervous exhaustion and its phenomena, it is desirable to study the severe cases first, and that is the rule which I propose to follow this evening.

When we come to consider a severe case of nervous exhaustion two things will strike us. In the first place the patient obviously is very ill and, in the second, he is greatly confused mentally. That there is a disturbance of the general health is shown by the pale, anæmic and wasted appearance of the patient. Generally he has a muddy complexion and a furred tongue; the bowels are usually constipated and the urine is muddy—all symptoms which point to ill-heath. One might imagine that they even pointed to a febrile condition, but the symptoms by themselves mean nothing except that the person is very ill. Coming to the nervous symptoms, I will deal with them in the order of convenience for description, and not necessarily in the order in which they would be investigated. Although, for example, I describe the patient's anæsthesia first and characterise loss of sensation to a pin-prick as a fundamental factor in the case, I do not suggest that it would be the first symptom one should investigate, and that one should go up to a patient and at once stick a pin into him. I show in diagram form the loss of sensation occurring over large areas of the body in the worst cases of exhaustion psychosis. In one instance you will observe that the whole of the body is anæsthetic with the exception of what may be called the bathing-drawers area and the feet. In a somewhat milder case a patch in the hand may retain sensation, or the loss of sensation may not be so widely distributed in the back and on the thighs. In still milder cases only the arms and legs may be anæsthetic. This form of anæsthesia differs from the hysterical variety (what is known as stocking and glove anæsthesia) in that the outline is irregular and the areas shade off, so to speak, the sensitive part merging into the anæsthetic. In hysterical anæsthesia, on the other hand, there is a straight line of well-marked demarcation around the limb. In the mildest exhaustion cases we find only a loss of sensation at the back of the fingers, where the anæsthesia appears to begin.

Rising in the scale of complexity, the next disturbance of which we have to take note is in the domain of perception. By perception I mean the process by which we are able to understand what we see, hear, smell, taste and touch. We see a piece of chalk—that is the process of visual perception. We hear bells ringing—that process is one of auditory perception. We smell around the dispensary to find out the contents of certain bottles—that is the process of olfactory perception; and so on. This function is disturbed in the particular patients with whom we are dealing. They have difficulty in telling what they hear, see and smell. This symptom forty years ago was called "imperception," but within the last fifteen years exactly the same thing has been designated "agnosia." But "imperception" is the English name which was given to the symptom by Hughlings Jackson. We discover it simply by asking the people to recognise certain objects. It is customary in cases of "sensory aphasia" to use for the

purpose a pencil or a bunch of keys. I find it advisable, however, to show something which is just a little out of the way. I have here an instrument which may be used as a paper-knife or a letter-opener, and has various other uses. If the patient is able to suggest one of its uses his perception is normal, but if he says that it is a saw his perception is imperfect. Then I have a little brush which I carry about in my pocket, also a smoker's knife; anything of that kind is useful for testing the patient. In hospital practice it is convenient to carry around the wards children's rag-books containing pictures of various simple objects, and to ask the patients to tell you what these are. With some of the pictures, which are apparently quite obvious, a candle-stick for instance, they will be altogether puzzled or make very bad guesses indeed. In testing the domain of hearing one may tear a piece of paper behind the patient's back or use the hissing sound of the soda-water syphon or the chink of money. It is still more difficult to get them to recognise smells; indeed I believe that with normal people the recognition of a smell is more difficult than recognition by the other senses. One of the symptoms of agnosia of this variety is perseveration or ideational inertia. We show a patient, for example, a pen and he is able to recognise it; but when we show him something quite different he says that is a pen also, and on showing him something else he describes it as something to write with. He is right on the first occasion, but he cannot get away from the original idea (ideational inertia).

Of the other disorders of perception, hallucination is one of the commonest. These patients see and hear things that do not exist. That is to say, the percept occurs in the absence of a normal stimulus for such a percept. The patients see objects the nature of which seems to vary according to the cause of their disease, but the details of this aspect of the question have still to be worked out. The characteristic hallucination of delirium tremens, for example, tends to take a zoological form. In belladonna poisoning the victim shows a tendency to see hairs or wires and to make motions as though he were extracting such objects from his fingers. In exhaustion cases and cases arising from shock the tendency is to see faces. In one case of exhaustion psychosis arising from overstrain—it was the case of a young man who had run a five-mile race and afterwards became exhausted and had to take to his bed for a week—the patient imagined that he saw people and flowers over his head. These visual hallucinations vary in complexity and in kind. Hallucinations of hearing also occur. It is common for the patient to hear voices; often music is heard. With regard to hallucinations of smell, it is rather difficult to get the patient in severe cases to describe them, and similarly with hallucinations of taste, but both these forms occur. There are also hallucinations of movement, *e. g.* people imagine they are moving their arms when in reality their arms are quite still.

The patients usually mistake the identity of the persons around them. The doctor becomes their father, the nurse their sister, and so forth. They cannot recognise their surroundings (disorientation). While they are lying in their own room at home they will say that it is remarkable that people should have taken them so far away from home and then have taken so much trouble to make the room like their own. They sometimes fail to recognise which town they are in and, if they are in a hospital, they may have no idea as to whether it is a theatre, a monastery or an ordinary private house; or if they do recognise that it is a hospital and are asked where the hospital is, they fail to remember.

Not only is there disorientation in place, but there is also disorientation in time. The patients may be months or even years out of reckoning. I remember a patient who in the year 1905 had the idea that it was 1870.

The memory is bad, especially for recent events. They may forget everything that has occurred for the last thirty years and it is quite common to find the events of the last ten years obliterated from their recollection. Their memory is especially bad for the events of that particular day. They cannot tell what they have had for breakfast or dinner.

Similarly, of course, the judgment is affected. They are confused and, being confused, are unable to follow a coherent train of thought. They wander and the faculty of attention is poor. They seldom keep their attention fixed upon any one thing and, should it seem to be attached to one particular object, it is very easy to divert it. In this way there arises a variety of incoherence. The

doctor finds himself unable to follow the patient's train of thinking. Any passing percept may divert the current of attention and the flow of speech. Sometimes the sound of a word may attract attention and give rise to a rhyming incoherence. For example, if they are attracted by a hat that you hold in your hand they will utter the words "hat, rat, cat," and so on. This general mental confusion involves feebleness of judgment and this, with the other symptoms, results in delusions of various kinds. Being disorientated, the patients may imagine that they are imprisoned and that the nurses are detectives. The hallucinations themselves may give rise to delusions. They imagine that they hear the roar of furnaces which are intended to burn them up, and inasmuch as the olfactory hallucination very frequently takes the form of a smell of burning, the delusion is rendered the more complete. The patients are sleepless, suffering, indeed, so much from insomnia that drugs can seldom be dispensed with. They are garrulous and talk incoherently, but as a rule they are not noisy, shouting patients.

With regard to behaviour, they show degenerate instincts. They are generally "wet and dirty" in their habits, will pass their evacuation into the bed, spit on the floor, and so forth. The emotional tone of these patients, as a rule, is that of depression, but a small number are hilarious and excited.

The duration of the disease is very variable; possibly it depends upon the cause. In the runner of the five-mile race of whom I spoke it was only a matter of something less than a week before he recovered. Some of the febrile cases last a fortnight. Delirium tremens in severe cases may last a fortnight; in mild cases its period is five days. In the exhaustion cases which arise from shock, overwork or nervous strain, the duration may go up to a year or fifteen months, or even more. I recall one case which lasted one year and eleven months. It was that of a girl who woke up one morning to find her grandmother dead beside her. She immediately went into this condition of nervous exhaustion. Thus the outlook is very variable, and it is only by experience that one is able to foretell the approximate duration.

In the treatment of these cases it is essential to look after the patient's nourishment and to ensure that they have plenty of rest. Inasmuch as they are exhausted the nervous system requires repose, but a tendency to restlessness is associated with the disease and this has to be overcome. It may be overcome by persuasion, inducing the patient to lie in bed and do nothing for weeks or even months. When he begins to improve he still has a tendency to be restless and is anxious to get up and to be in action. It is not wise to give in to him, however, and the rest treatment must be persisted in. There are various devices for making a patient more or less restful. Morphia is an excellent drug for the purpose of inducing a desire for rest, and another useful device is a prolonged bath in water warmed, say, to 97° F. If the patient can be made to rest in the warm water for about an hour, he can then be induced to go to bed, where the rest is prolonged for some time. It is then possible to increase the time spent in the bath to one and a half, two, three or four hours and the effect, when this is done gradually, is usually very quieting to the patient, who sleeps well afterwards.

I have called the disease which I have been describing "the exhaustion psychosis." The disease also appears in a milder form, which might be called "the exhaustion neurosis." The patients in this instance can scarcely be described as suffering from any real mental disorder. On first examining them they may say that they feel they have been doing too much and may complain of a numbness about the head. On examining their sensation we may discover no real loss, but the patients themselves will say, for instance, that they do not feel a pin-prick on their hands so well as they feel it on their shoulders. In other words, there is "comparative anæsthesia."

In testing the perception of these patients, we find them perfectly well able to recognise familiar objects. But here is a more severe test. I have with me a child's book, called 'Proverbs Old Newly Told.' The proverbs are told in pictures, and after obliterating the lettering underneath I have asked these patients to indicate the proverb which is portrayed in the picture. Some of them are simple, such as "You may take a horse to water, but you cannot make him drink," but others are more difficult, and even a normal person may take five minutes to think of them. The patients, however, will often quote proverbs which are obviously inappropriate. They complain, further, that upon reading a book they are unable to take in its

meaning; and although they know quite well where they are, they sometimes complain of difficulty in finding their way about. They suffer, therefore, from a slight form of disorientation.

With regard to hallucinations, these people, except occasionally, do not see faces. One patient whom I have in mind, and whose illness belongs to this order, has once seen the outline of the face of an old man. Generally, the visual hallucination takes the form of moving lights about the field of vision. The hallucination of hearing is very commonly the tinkling of bells. Whenever met with, it is a symptom of nervous exhaustion; to hear bell-ringing in hallucination is a sign that a person wants a holiday. Unlike patients suffering from the exhaustion psychosis, these sufferers from the exhaustion neurosis are able to describe their hallucinations of smell. The particular patient to whom I have just alluded suffered from the hallucination of smell in a marked degree. It was the smell of an animal that afflicted him, and it was carried to such an extent that, in his own words, the question began to dawn upon him whether he was one of the lower animals. He is an able man, trying for an honour at Cambridge, and he has been overworked. The hallucination of smell has now changed from that of an animal to that of an orange—for better or worse I know not. He also suffers from a psycho-motor hallucination, in that he feels his hand move and touch himself, when, as a matter of fact, it has not moved at all. People thus afflicted do not generally even begin to suffer from delusions, although this particular patient admitted that it had begun to dawn upon him that he was one of the lower animals.

Treatment in these cases is exactly the same as I have already described. This patient began to suffer from insomnia and could not get to sleep until two or three in the morning. But after two days' complete rest in bed he was certainly much better, and in two or three weeks I hope that he will be ready to go for convalescence to the sea. The general principle of treatment is to give the patients plenty of rest and to feed them up. Isolation is not good for them; it is likely to increase their hallucinations if they are kept too much by themselves.

The pathology of this condition of nervous exhaustion has not been thoroughly worked out. In the few cases that have died of the disease—I have not examined any myself—the *post-mortem* examination has disclosed pulverisation of Nissl's granules, and that is all. From theoretical considerations, however, one has come to the conclusion that there is an intoxication affecting the synapses between the cortical neurons and thus disturbing the processes of association.

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SOME HISTORICAL NOTES OF PLAGUE.*

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PLAGUE is a specific infectious disease affecting both man and some of the lower animals. The chief symptoms in man are fever, severe headache, giddiness, blood-shot eyes, mental depression, stammering speech and staggering gait, besides the special painful swellings in the groin, arm-pit, neck, or other parts of the body, which are the buboes characteristic of the disease. These buboes, and sometimes also the blood of the patient, contain a special microbe, which can be recognised by staining with certain dyes at its two extremities. This is the usual variety of what is called bubonic plague.

But there is a second variety, which attacks the lungs, called "pneumonic plague," in which there are no buboes. This is the kind which is most infectious from one human being to another, and you will remember that it occurred a few months ago in Suffolk.

There is a third variety, called the "fulminating" or "septicæmic" plague, in which buboes are rare, but, as in the pneumonic variety, the patient usually dies.

Plague is a disease of such antiquity that it is impossible to say when its history began. In Syria it has been known for at least 3000 years, and it is mentioned in the Bible as occurring in the land of the Philistines, where it broke out in Canaan during military operations against the Israelites. In the fifth and sixth chapters of the First Book of Samuel we read that the inhabitants of the cities of Ashdod, Gath, Ekron, and Beth-Shemesh were attacked with emerods or tumours in the groin. Everywhere the pestilence caused dire destruction, and it is stated that in Beth-Shemesh more than 50,000 people died.

Even in those old days plague was known to attack the smaller animals as well as man, for it is recorded that in order that the plague might be stayed the Philistines made offerings of propitiation to the Lord of Israel; these offerings were of gold, and consisted of images of the tumours which

* Gresham Lecture.

affected human beings, and other images of the mice, which were also attacked.

Some of my friends who are conversant with Hebrew tell me that the word used in the original writings could be translated by either rats or mice.

On another occasion, the retreat of Sennacherib's army from Pelusium in Egypt is attributed to a pestilence with which field mice were connected, and, according to Herodotus, in commemoration of the event a stone statue of Sethon was erected in the temple of Venus with a mouse in his hand, with the inscription, "Whoever looks on me, let him revere the gods."

Plague broke out at frequent intervals in the East, and vivid accounts of it have been handed down to us from writers who witnessed its ravages so long as 1400 years ago. There has been little or no change in the aspect of the disease since those days, and the account of the plague which broke out in Constantinople in 510 A.D., and was supposed to have been brought there from Egypt, might apply to recent epidemics in India and elsewhere. We read of the same sudden onset followed very shortly by the appearance of the bubo, the delirium or drowsiness, and the restlessness of many of the patients, and the impossibility of making an accurate prognosis. "The most celebrated physicians," says Procopius, "predicted the death of several who, soon after, contrary to the general opinion, recovered, and on the other hand predicted the recovery of many who were on the point of death."

History records only severe epidemics, but even when we exclude the smaller outbreaks it is appalling to think of the destruction caused by this disease. Forty-one great epidemics of plague are recorded in the 1500 years before the Christian era, 109 during the first 1500 years after Christ, and forty-five from 1500 A.D. to 1720 A.D.

Even in these days of telegrams and newspaper accounts few of us seem to realise the fearful ravages that plague has committed in our own dependency of India. It is a surprise to those who have not followed events to hear that more than six and a half million people have died of it in India in the past fourteen years, and that as many as 5000 were dying each day during some months in 1905.

Let us now consider the outbreaks in Great Britain. In 548 A.D. Ireland was visited by a pestilence, thought to be plague. Ireland was in those days in constant communication with Italy, through ecclesiastics and others, and it is probable that plague was introduced by them. "Certainly," says Prof. Simpson, "it is about the middle of the sixth century, the Irish chronicles record, that Tara, which till that time was the residence of the chief king, was abandoned. Diarmait MacCearbhail, the King, left it and never returned, and it was never inhabited again. The royal burgh appears to have been abandoned because nearly everybody had died there, and the place came to be regarded with such dread that even its fine position, far-extending view and rich pastures could not induce future kings to return to it."

In 663 the plague again ravaged Ireland, and in the following year there was an epidemic in England, probably imported from the Emerald Isle. After that there was a pause of at least a century before it returned to these shores. It was from the eleventh to the fourteenth centuries that the plague assumed such gigantic proportions, became so widely diffused and so terrible in its effects that the like of it has never been known before or since. It is uncertain where the "Black Death," as it came to be called, arose. The whole of Asia was in a war-like state, and armies marching from one country to another might easily be the means of conveying the infection from infected areas to the healthy.

It seems to have entered Europe through Constantinople, where it broke out in 1347, and devastated that city. It is estimated that within three years it had robbed Europe of one fourth its population. We have the figures of mortality in some of the principal towns of Europe. Thus, in Florence, 60,000 people died of plague; in Venice, 100,000; in Paris, 50,000; in Norwich, 51,100; and in London, 100,000.

It reached England in 1348, and a contemporary writer, Le Baker de Swynebroke, gives a graphic account of its course. "At first," he says, "it carried off almost all the inhabitants of the seaports of Dorset, and then those living inland, and from there it raged so dreadfully through Devon and Somerset, as far as Bristol, that the men of Gloucester refused those of Bristol entrance to their country, everyone thinking that the breath of those who lived amongst people who died of

plague was infectious. But at last it attacked Gloucester, yea, and Oxford and London, and finally the whole of England so violently that scarcely one in ten of either sex were left alive." In the sixteenth and seventeenth centuries plague constantly reappeared in Great Britain. In 136 years there were eighty-four outbreaks in London alone, six of them being severe.

The years 1665 and 1666 saw the great Plague of London, which carried off 70,000 of its inhabitants. It is popularly believed that the plague suddenly appeared in this city in 1665, and was banished by the Great Fire of the following year. But, as a matter of fact, plague existed in London every year (with four exceptions) from 1603 to 1679, so that the fire can have had very little to do with its cessation, though it doubtless destroyed many wooden houses and barns infested by rats and fleas.

After this plague rapidly disappeared from the whole of Western Europe, and, with the exception of the outbreak in Marseilles in 1720, may be said to have left this part of the world in peace.

You must picture to yourselves London as it was in 1665, before the Great Fire of the following year swept it away to make room for a healthier, better-built city. Isolated cases of plague hardly ever left its narrow, over-crowded, filthy alleys, where rats and other vermin swarmed in the wooden houses, where the sunlight could not enter, and where sanitation was unknown. In 1620 the city had received a good water supply, the first essential for health and cleanliness, but it was some time before the water was made available to all, and the habits of those times were undoubtedly dirty.

The plague began in St. Giles,' then one of the suburbs, and in one year is said to have carried off nearly one fifth of the population of London.

As soon as its presence became known there was a general stampede among those of the inhabitants who could afford to escape, and among those who fled were most of the doctors and clergymen, to their undying shame. The helpless, plague-stricken poor were left destitute of medical and spiritual help in their dire need, with the exception of a small handful of devoted doctors, who stayed at their posts, not more than twenty-seven or so in all, to minister to the needs of 240,000 inhabitants in a time of pestilence. "All the officers of the College of Physicians, led by their President, fled; to find, on their return, that their college had been broken into and the college coffers emptied" (Clippingdale).

I cannot here enumerate all the doctors who remained in London, but I will mention two or three. Alexander Burnett was the medical attendant of Samuel Pepys, the diarist. In making an autopsy upon a person dead of the plague he became infected and died, and the same fate overtook four other physicians who were present at this post-mortem examination. Francis Glisson, at one time Regius Professor of Physic at Cambridge, and one of the original members of the Royal Society, published the first English description of rickets. He worked energetically through the plague time, and fully deserved his promotion in 1667 to the Presidency of the College of Physicians. Thomas Wharton was Censor of the College of Physicians. When the plague broke out he was tempted to beat a retreat, but decided to remain in London to attend to the large number of poor people who came to him at St. Thomas's Hospital, where he was one of the physicians. The Foot Guards, when attacked with plague, were sent to this hospital and were attended by him. "For his services to the troops Wharton was promised the first vacant post as Physician-in-Ordinary to the King, but when the vacancy arose he was put off with an augmentation in his coat of arms, for which he had to pay the Herald £10."

I now propose to read to you extracts from Dr. Nathaniel Hodges's account of the plague in London. Dr. Hodges (1629–1688), after a distinguished career at Westminster School and at both Cambridge and Oxford Universities, settled in practice in Walbrook in the City of London in 1654. When the plague devastated the metropolis he remained in residence and attended all who sought his advice. In recognition of his services to the citizens during the plague he was granted a stipend by the City authorities as their official physician. His account of the epidemic was published in 1672, and was called 'Loimologia, or an Historical Account of the Plague in London in 1665, with precautionary directions against the like contagion.' The book shows that the author was a good observer of symptoms and of the results of treatment.

The College of Physicians, in recognition of the value of his book, made him a Fellow, and later

he became Harveian Orator to the College. Unfortunately his practice did not keep pace with his honours, for he died in the debtors' prison at Ludgate. He was buried at St. Stephen's, Walbrook, where his bust and inscription may be seen.

"The plague, which we are now to give an account of, discovered the beginnings of its future cruelties about the close of the year 1664; for at that season two or three persons died suddenly in one family at Westminster, attended with such symptoms as manifestly declared their origin: hereupon some timorous neighbours, under apprehension of a contagion, removed into the City of London, who unfortunately carried along with them the pestilential taint; whereby that disease, which was before in its infancy, in a family or two, suddenly got strength, and spread abroad its fatal poisons; and merely for want of confining the persons first seized with it, the whole city was in a little time irrecoverably infected. Not unlike what happened the year following, when a small spark, from an unknown cause, for want of timely care, increased to such a flame that neither the tears of the people, nor the profusion of their Thames, could extinguish; and which laid waste the greatest part of the city in three days' time. . . . But as soon as it was rumoured amongst the common people that the plague was in the city, it is impossible to relate what accounts were spread of its fatality. . . . Everyone predicted its future devastations, and terrified each other with remembrances of a former pestilence. . . . And these frightful apprehensions were not a little increased by the predictions of astrologers, from the conjunctions of stars and the appearances of comets. . . . A very hard frost began in December (1664), which continued three months, and seemed greatly to deaden the contagion, and very few died during that season; although even then it was not extinguished, for in the middle of Christmas holy days, I was called to a young man in a fever, who, after two days' course of medicines, had two risings about the bigness of a nutmeg broke out, one on each thigh; upon examination of which, I soon discovered the malignity, both from their black hue, and the circle round them, and pronounced it to be plague; in which opinion I was afterwards confirmed by the subsequent symptoms, although by God's blessing the patient recovered.

"As soon as the Magistracy, to whom the public care belonged, saw how the contagion daily spread, and had now extended itself to several parishes, an order was immediately issued to shut up all the infected houses, that neither relations nor acquaintances might unwarily receive it from them, and to keep the infected from carrying it about with them. But whether this method proved of service or not, is to this day doubtful, and much disputed; but, however, it is my business here to adhere to facts, and relate the arguments on both sides with all possible impartiality. In order whereunto, it is to be observed, that a law was made for marking the houses of infected persons with a red cross, having with it this subscription—*Lord have mercy upon us.* And that a guard should attend there continually, both to hand to the sick the necessaries of food and medicine, and to restrain them from coming abroad until forty days after their recovery. But although the Lord Mayor and all inferior officers readily and effectually put these orders in execution, yet it was to no purpose, for the plague more and more increased. . . . And this seclusion was on this account much the more intolerable, because if a fresh person was seized in the same house but a day before another had finished the quarantine, it was to be performed over again, which occasioned such tedious confinements of sick and well together as sometimes caused the loss of the whole.

"But what greatly contributed to the loss of people thus shut up was the wicked practices of nurses (for they are not to be mentioned but in the most bitter terms). These wretches, out of greediness to plunder the dead, would strangle their patients, and charge it to the distemper in their throats; others would secretly convey the pestilential taint from sores of the infected to those who were well; and nothing indeed deterred these abandoned miscreants from prosecuting their avaricious purposes by all the methods their wickedness could invent; who, although they were without witnesses to accuse them, yet it was not doubted but Divine vengeance will overtake such wicked barbarities with due punishment. Nay, some were remarkably struck from Heaven in the perpetration of their crimes, and one particularly amongst many, as she was leaving the house of a family, all dead, loaded with robberies, fell down dead under her burden in the streets; and the case of a worthy

citizen was very remarkable, who being supposed dying by his nurse, was beforehand stripped by her; but recovering again, he came a second time into the world naked. And so many were the atrocities of these barbarous wretches, that it is to be hoped posterity will take warning how they trust them again in like cases; and that their past impunities will not be a means of bringing on us again the like judgment.

"The infection had long doubtfully reigned, and continued through May and June with more or less severity, sometimes raging in one part and then in another, as in a running sort of fight; as often as the number of funerals decreased great hopes were conceived of its disappearance, then on a sudden, again, their increase threw them all into dejection, as if the whole city was soon to be unpeopled, which uncertainty gave advantage to the distemper, because persons were more remiss in their provisions against it during such fluctuations.

"It must not, however, be omitted with what precipitation the trembling inhabitants left the city and how they flocked in such crowds out of the town, as if London had quite gone out of itself. . . . In the space of one week were eight thousand persons cut off, and when things came to extremity all helps were called in; so it began now to be solely the Magistrate's business how to put a stop to this cruel devastation and save some part of the city, at least, from the grave. First, then, therefore, was appointed a monthly fast for public prayers to deprecate the anger of Heaven; nor proved it in vain, or were their supplications altogether fruitless, for the whole summer was refreshed with moderate breezes sufficient to prevent the air's stagnation and corruption and to carry off the pestilential streams. . . . In the months of August and September the contagion changed its former slow and languid pace, and, having as it were got master of all, made a most terrible slaughter, so that three, four, or five thousand died in a week, and once eight thousand. Who can express the calamities of such times? The whole British nation wept for the miseries of her metropolis. In some houses carcases lay waiting for burial, and in others persons in their last agonies; in one room might be heard dying groans, in another the ravings of a delirium, and not far off relations and friends bewailing both their loss and the dismal prospect of their own sudden departure. . . . Some of the infected run about staggering like drunken men and fall and expire in the streets, while others lie half dead and comatose, but never to be waked but by the last trumpet; some lie vomiting as if they had drunk poison, and others fall dead in the market while they are buying necessaries for the support of life. . . . As the city was afterwards burnt without distinction, in like manner did this plague spare no order, age, or sex; the divine was taken in the very exercise of his priestly office to be enrolled amongst the saints above, and some physicians could not find assistance in their own antidotes, but died in the administration of them to others; and although the soldiery retreated from the field of death and encamped out of the city, the contagion followed and vanquished them. Many in their old age, others in their prime, sunk under its cruelties. Of the female sex most died, and hardly any children escaped, and it was not uncommon to see an inheritance pass successively to three or four heirs in so many days. The sextons were not sufficient to bury the dead; the bells seemed hoarse with continual tolling, until at last they quite ceased. The burying places would not hold the dead, but they were thrown into large pits dug in waste grounds in heaps, thirty or forty together, and it often happened that those who attended the funerals of their friends one evening were carried the next to their own long home.

"About the beginning of September the disease was at the height, in the course of which month more than twelve thousand died in a week; but at length, that nothing might go untried to divert the contagion, it was ordered by the Governors to burn fires in the streets for three days together. . . . But whether it was from the suffocating qualities of the fuel or the wet constitution of the air that immediately followed, the most fatal night ensued, wherein more than four thousand expired.

"Nothing more contributed to the common destruction than the practice of chemists and quacks, of whose audacity and ignorance it is impossible to be altogether silent; they were indefatigable in spreading their antidotes, and although equal strangers to all learning as well as physick, they thrust into every hand some trash or other under the disguise of a pompous title. No country surely ever abounded with such wicked impostors."

In a special chapter of his book Hodges proceeds

to discuss the cure of the pestilence and examines several antidotes.

"The powder of an unicorn's horn," he says, "so much cried up for an antidote, never answered any good expectations, although I had several doses of it given me by a merchant on purpose to try its virtues." But he wisely concludes that the expense of this remedy, even if efficacious, puts it beyond the reach of the poor. "Therefore cheap and common medicines should be contrived for them by the physicians, in the number of which first occur the troches [lozenges] of vipers in compound with scordium water, or the volatile salt of vipers given in the same vehicle. (*Teucrium scordium* is wood-sage or water-germander, and used to be employed as a tonic and stimulant.) A very worthy person sent us from New England some troches made of the flesh of a rattle-snake, from which I found more success amongst the sick than those we commonly have here. The powder of toads was likewise prodigiously extolled by everybody.

"But before I proceed further, gratitude obliges me to do justice to the virtues of sack, as it deservedly is ranked amongst the principal antidotes, whether it be drunk by itself or impregnated with wormwood, angelica, etc., for I have never yet met with anything so agreeable to the nerves and spirits in all my experience. That which is best is middle-aged, neat, fine, bright, racy, and of a walnut flavour; and it is certainly true that during the late fatal times, both the infected and the well found vast benefit from it, unless they who used it too intemperately.

"It remains that we now say somewhat concerning the use of tobacco, whose virtues for this purpose are extremely cried up by Diemebrooeck, and some others; but whether we regard the narcotic quality of this American henbane or the poisonous oil which exhales from it in smoking, or that prodigious discharge of spittle which it occasions, and which Nature wants for many other important occasions, or, lastly, the exercise it gives to the lungs in drawing it, besides the aptitude of the pestilential poison to be taken down along with it, and the irksomeness of its scent, I must confess myself at uncertainties about it; though, as to myself, I am its professed enemy, and was accustomed to supply its place as an antidote with sack."

Discussing some of the public measures introduced for the safety of the community, he states: "Further, that all the means of propagating the plague may be removed, it is very wisely ordered by the Magistracy to kill all dogs, cats and other domestic brutes lest these creatures in their passage from one place to another should carry along with them the pestilential infection."

It is pleasant to read how this devoted and public spirited physician stuck to his post when the greater number of doctors ran away. At the end of his book he gives us a graphic account of how he spent his days in the plague time.

"Having thus come to a conclusion, I think it not amiss to recite the means which I used to preserve myself from the infection, during the continual course of my business among the sick.

"As soon as I rose in the morning early, I took the quantity of nutmeg of the anti-pestilential electuary; then after the dispatch of private concerns in my family, I ventured into a large room, where crowds of citizens used to be in waiting for me; and there I commonly spent two or three hours, as in an hospital, examining the several conditions and circumstances of all who came thither; some of which had ulcers yet uncured, and others to be advised under the first symptoms of seizure; all which I endeavoured to dispatch with all possible cure to their various exigencies.

"As soon as this crowd could be discharged, I judged it not proper to go abroad fasting, and therefore got my breakfast, after which, till dinner time, I visited the sick at their houses; whereupon entering their houses, I immediately had burnt some proper thing upon coals; and also kept in my mouth some lozenges all the while I was examining them.

"I further took care not to go into the rooms of the sick when I sweated, or were short breathed with walking; and I kept my mind as composed as possible, being sufficiently warned by such, who had grievously suffered by uneasiness in that respect. After some hours' visiting in this manner, I returned home. Before dinner I always drank a glass of sack, to warm the stomach, refresh the spirits and dissipate any beginning lodgment of the infection. I chose meats for my table that yielded an easy and generous nourishment, roasted before boiled, and pickles not only suitable to the meats, but the nature of the distemper (and, indeed, in this melancholy time, the city greatly abounded with variety of all good things of that nature); I

seldom likewise rose from dinner without drinking more wine. After this, I had always many persons came for advice, and as soon as I could dispatch them I again visited till 8 or 9 at night, and then concluded the evening at home by drinking to cheerfulness of my old favourite liquor, which encouraged sleep and an easy breathing through the pores all night.

"But if in the day-time I found the least approaches of infection upon me, as by giddiness, loathing at stomach, and faintness, I immediately had resource to a glass of this wine, which easily drove these beginning disorders away by transpiration. Yet in the whole course of the infection, I found myself ill but twice, but was soon again cleared of its approaches by these means, and the help of such antidotes as I kept always with me."

Another contemporary account of the great plague of London is given us by William Boghurst, an apothecary who kept a shop at the sign of the White Hart in the parish of St. Giles in the Field. Besides keeping the shop he practised as a doctor, prescribing and selling drugs. He died at the age of fifty-four, and was buried at Ditton in Kent, where his epitaph runs thus:

"Here lies William Boghurst, citizen and apothecary of London, son of William of the parish of Ditton. He was an honest, just man, skilful in his profession, and in the Greek and Latin tongue, delighting in the study of antiquity; and plaid exceeding well upon the lute, which he took naturally. He left a sorrowful widow and six children, to whom he afforded commendable breeding."

In his book, entitled 'An Experimental Relation of the plague of what hath happened remarkable in the last Plague in the City of London,' he shows himself to have been a keen observer and thinker. What is of interest to us, with our present knowledge, is the chapter dealing with "Signs Foreshadowing a Plague Coming," among which he mentions:

"(1) Times and seasons altering from their common state.

"(2) Many changes of weather in a short time.

"(3) Increase of vermin, as frogs, toads, mice, flies, ants, etc.

"(4) Famine; also war.

"(5) Ill conditions of the stars, if you will believe the astrologers.

"(6) Murraine of cattle, boiles and blaines."

Daniel Defoe's 'Journal of the Plague Year' appeared in 1722, at the time of a dread in this country of the plague, which had recently reappeared in France. The journal has such an air of authenticity that it imposed, not only upon men of a much later day, but upon Dr. Mead, who quoted it as an authority in his 'Discourse on the Plague,' published in 1744. As a matter of fact, the celebrated author of 'Robinson Crusoe' was born in the parish of St. Giles's, Cripplegate, in 1660 or 1661, and was, therefore, only four or five years old at the time of the plague of London.

Defoe's 'Journal' is described on the title-page as "Observations or Memorials of the most remarkable occurrences, as well public as private, which happened in London during the last great visitation in 1665, written by a citizen who continued all the while in London. Never made public before." Defoe had an eventful life, became a liveryman of the City in 1687, joined the army of William III during its advance on London, went bankrupt, and then became a pamphleteer in favour of the King. After William's death he was prosecuted for libelling the Church, and the House of Commons ordered one of his books to be burnt. He was tried at the Old Bailey in 1703, and was made to stand in the pillory on three successive days, and was afterwards imprisoned in Newgate for a year. The people formed a guard of honour round the pillory, which they covered with flowers, while they drank his health. Defoe published a "Hymn to the Pillory," which was sold to the crowd. It contained these lines:

"Tell them the men who placed him here
Are scandals to the times;
Are at a loss to find his guilt,
And can't commit his crimes."

Defoe's great literary gifts were often employed by the Government, for he was able to write with extraordinary facility upon every topic likely to attract public curiosity. In his lighter vein he wrote ghost stories, the imaginary history of an earthquake in St. Vincent, and accounts of various criminals such as Jonathan Wild and Jack Sheppard.

According to his biographer, William Lee, he was the author of no less than 254 works.

He was buried in Bunhill Fields cemetery, where an obelisk was erected to his memory in 1870 by many youthful admirers of his great work, 'Robinson Crusoe.'

Many of you must be familiar with the account of the great plague given in the diary of Samuel Pepys, the Admiralty Secretary, who remained at his post all through those terrible months, though he sent his wife into the country to escape infection.

On June 7th, 1665, he writes: "This day, much against my will, I did in Drury Lane see two or three houses marked with a red cross upon the doors, and 'Lord have mercy upon us!' writ there; which was a sad sight to me, being the first of the kind that, to my remembrance, I ever saw. It put me into an ill conception of myself and my smell, so that I was forced to buy some roll-tobacco to smell to and chaw, which took away the apprehension."

On July 26th: "Sad news of the death of so many in the parish of the plague—forty last night. The sickness is got everywhere, so that I begin to think of setting things in order, which I pray God enable me to put, both as to soul and body." A fortnight later: "To my office a little. The streets empty all the way now, even in London, which is a sad sight. I went away and walked to Greenwich, in my way seeing a coffin with a dead body therein, dead of the plague, lying in an open close belonging to Combe farme, which was carried out last night, and the parish have not appointed anybody to bury it, but only set a watch there all day and night, that nobody should go thither or come thence, this disease making us more cruel to one another than we are to dogs. . . . I went forth and walked towards Moorfields to see—God forgive my presumption!—whether I could see any dead corpse going to the grave, but, as God would have it, did not. But, Lord! How everybody's looks and discourse in the streets is of death, and nothing else; and few people going up and down, that the town is like a place distressed and forsaken." Again, he tells us "of a complaint brought against a man in the town for taking a child from London from an infected house; Alderman Hooker told us it was the child of a very able citizen in Gracious Street, a saddler, who had buried all the rest of his children of the plague, and himself and wife, now being shut up in despair of escaping, did desire only to save the life of this little child, and so prevailed to have it received stark-naked into the arms of a friend, who brought it, having put it into new fresh clothes, to Greenwich, where, upon hearing the story, we did agree it should be permitted to be received and kept in the town." By the middle of September Pepys was overjoyed to hear of "the decrease of 500 or more, which is the first decrease we have yet had in the sickness since it begun, and great hopes that the next week it will be greater. Then, on the other side, my finding that, though the Bill in general is abated, yet the City, within the walls, is increased, and likely to continue so, and is close to our house there. My meeting dead corpses of the plague, carried to be buried close to me at noon to-day through the City in Fenchurch Street. To see a person, sick of the sores, carried close by me by Gracechurch in a hackney-coach. My finding the Angel Tavern, at the lower end of Tower Hill, shut up; and, more than that, that the person was then dying of the plague when I was last there, a little while ago, at night. To hear that poor Payne, my waiter, hath buried a child, and is dying himself. To hear that a labourer I sent but the other day to Dagenham's is dead of the plague, and that one of my own watermen, that carried me daily, fell sick as soon as he had landed me on Friday morning last, when I had been all night upon the water, and I believe he did get his infection that day, and is now dead of the plague." On September 18th matters were still very bad. He writes: "Up, and after being trimmed, the first time I have been touched by a barber these twelve months, to the Duke of Albemarle. But, Lord! what a sad time it is to see no boats upon the river, and grass grown up all up and down White Hall court and nobody but poor wretches in the streets! . . . Come close to by the bearers with a dead corpse of the plague; but Lord! to see what custom is, that I am come almost to think nothing of it."

On January 22nd following, at last he records: "The first meeting of Gresham College since the plague. Dr. Goddard did fill us with talk, in defence of his and his fellow physicians going out of town in the plague time, saying that their particular patients were most gone out of town, and they left at liberty, and a great deal more."

This Dr. Goddard was Gresham Professor of Physic from 1655 till his death in 1674. He was the first Englishman to make telescopes, and his reputation was kept alive for several generations through certain volatile drops, which were his invention and bore his name, and were much in demand for fainting, apoplexies, lethargies and other sudden attacks.

Alexander William Kinglake, the historian, gives such a good account of the ravages of plague in an Eastern city in more modern days that I cannot refrain from reading it to you. In his book 'Eothen,' he tells the story of his travels in Ottoman dominions in the years 1834–5, and in the latter year, on reaching Cairo, he finds the city "under the terrible curse of the plague." He did not wish to linger there, but his dragoman fell ill, and he could not leave for nineteen days, during which time the plague was rapidly increasing.

"When I first arrived," he says, "it was said that the daily number of 'accidents' by plague out of a population of about 200,000 did not exceed four or five hundred, but before I went away the deaths were reckoned at 1200 a day. I had no means of knowing whether the numbers (given out, as I believe they were, by officials) were at all correct, but I could not help knowing that from day to day the number of the dead was increasing. My quarters were in one of the chief thoroughfares of the city, and as the funerals in Cairo take place between daybreak and noon (a time during which I generally stayed in my rooms), I could form some opinion as to the briskness of the plague. I don't mean that I got up every morning with the sun. It was not so, but the funerals of most people in decent circumstances at Cairo are attended by singers and howlers, and the performance of these people woke me in the early morning and prevented me from remaining in ignorance of what was going on in the street below.

"These funerals were very simply conducted. The bier was a shallow wooden tray carried upon a light and weak wooden frame. The tray had in general no lid, but the body was more or less hidden from view by a shawl or scarf. The whole was borne upon the shoulders of men and hurried forward at a great pace. Two or three singers generally preceded the bier; the howlers (these are paid for their vocal labours) followed after; and last of all came such of the dead man's friends and relations as could keep up with such a rapid procession; these, especially the women, would get terribly blown, and would straggle back into the rear; many were fairly beaten off. I never observed any appearance of mourning in the mourners; the pace was too severe for any solemn affectation of grief.

"When first I arrived at Cairo the funerals that daily passed under my windows were many, but still there were frequent and long intervals without a single howl. Every day, however, these intervals became less frequent and shorter, and at last, the passing of the howlers from morn to noon was almost incessant. I believe that about one half of the whole people was carried off by this visitation. The Orientals, however, have more quiet fortitude than Europeans under afflictions of this sort, and they never allow the plague to interfere with their religious usages. I rode one day round the great burial gound. The tombs are strewed over a great expanse among the vast mountains of rubbish (the accumulations of many centuries) which surround the city. The ground, unlike the Turkish cities of the dead, which are made so beautiful by their dark cypresses, has nothing to sweeten melancholy, nothing to mitigate the hatefulness of death. Carnivorous beasts and birds possess the place by night, and now in the fair morning it was all alive with fresh comers—alive with dead. Yet at this very time, when the plague was raging so furiously, and on this very ground which resounded so mournfully with the howls of arriving funerals, preparations were going on for the religious festival called the Kourban Bairam. Tents were pitched and swings hung for the amusement of children—a ghastly holiday! but the Mahometans take a pride, and a just pride, in following their ancient customs undisturbed by the shadow of death.

"I did not hear whilst I was at Cairo that any prayer for a remission of the plague had been offered up in the mosques. I believe that however frightful the ravages of the disease may be, the Mahometans refrain from approaching Heaven with their complaints until the plague has been endured for a long space, and then at last they pray God—not that the plague may cease, but that it may go to another city."

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February 27th, 1911.

THE SIGNIFICANCE OF SPHYGMOMANOMETRIC READINGS.

By F. HOWARD HUMPHRIS, M.D.Brux., F.R.C.P.Edin.,
First Vice-President of the American Electro-Therapeutic Association.

THE method in use all over the world, and the one by which we were all originally taught, of estimating the arterial tension, is by the pressure of one or more finger-tips over the radial artery. Much valuable information may be gained in this way, and to some a certain skill has been given or acquired by which this information is wonderfully accurate. But to the average medical man, and more especially to him in the early years of his career, the accuracy of the finger method compared with the modern sphygmomanometer is probably quite comparable to that of the estimation of the body temperature by sense of touch instead of by the clinical thermometer.

"Science is measurement," and if it is of advantage to note the blood-pressure, or to estimate tension when feeling the pulse from time to time in the course of the disease, how much more is it of an advantage to be able to make an accurate estimate, and to record the result in millimetres?

Although an altered blood-pressure is only a symptom, and not a disease *per se*, it is a symptom of such importance that in some cases diagnosis and prognosis may be decided by it, and in others treatment may be modified by its variations. And, again, it may be in itself a symptom which urgently calls for relief.

There are various instruments for making the estimation of blood-pressure; most of them are based upon the Riva-Rocci apparatus, in which there is a closed system of air connected with a rubber bulb held by the operator, with a column of mercury and with an air-inflated, broad rubber bag placed around the arm of the patient.

By the law of gases equal pressure is transmitted to every point throughout any given air-system.

So when the bulb held by the observer is compressed the rubber cuff is inflated and exerts pressure on the upper arm, which pressure is registered by the column of mercury. When this obliterates the radial pulse we have exerted a pressure from without equivalent to the systolic pressure from within, which can be read in millimetres of mercury on the manometer.

In practice this is best determined by inflating the cuff until the radial has been obliterated, and by allowing the pressure slowly to recede, the point at which the pulse is first felt is conceded to be the systolic pressure, or maximum arterial tension.

With the mean arterial tension (which may be said to be about three quarters of the maximum arterial tension), or with the diastolic pressure, this paper does not concern itself.

Also the treatment of altered blood-pressure, and when it should be attempted and when it should be let alone, is likewise outside the scope of this paper. I only mention treatment to emphasise that in every case it should be guided and supervised by frequent sphygmomanometric readings.

"A man is as old as his arteries," and if it were only that an alteration in blood-pressure gave a warning of advancing years, that fact alone would justify attention to the subject-matter of this paper. There are, however, many conditions resulting from, or concurrent with, an alteration in arterial tension that make the subject interesting as well as vital.

Increased arterial tension is in all probability responsible for the degeneration of the muscular walls of the arteries; in fact nothing else can be conceived, if granted the pathologically increased tension on the persistently contracted arterioles, than that metabolism and nutrition of the muscular structure of the arteries should suffer. Muscles in any part of the body degenerate under constant tension or contraction.

The importance of early recognition of the altered blood-pressure is, therefore, obvious; if we can recognise the pressure before it has had time to cause the degeneration, then we may, by the different therapeutic measures, diminish that pressure and thus avoid the otherwise resultant degeneration.

In some cases where the blood-pressure is giving rise to nervous trouble we can, I believe, by approximating the blood-pressure more nearly to the normal, prevent those troubles from ending in permanent disease and from crossing that borderline between nervous disease and mental derange-

ment to which they are slowly but surely progressing.

An alteration in arterial tension is probably caused by unusual stimulation of the vaso-motor system. This may be due either to the circulation of irritants, such as alcohol and toxins, in the blood-stream, or to some central nervous stimulation of the vaso-motor centres due to trouble in the brain or spinal cord.

Normal blood-pressure depends upon four factors: (1) The heart's energy; (2) the peripheral resistance; (3) the elasticity of the vessel walls; (4) the volume of the circulating blood.

High tension means a powerful ventricular contraction with contracted arterioles, just as low tension means a weak cardiac action with the dilated arterioles. And here I will submit a list, modified from Leftwich, of some of the conditions under which we may expect to find altered blood-pressure:

High Tension.	*Low Tension.*
Arterio-sclerosis.	Addison's disease.
Angina pectoris.	Anæmia.
Aortic aneurysm.	Aortic stenosis.
Chronic bronchitis.	Asthma.
Cirrhosis of kidney.	Alcoholic delirium.
Cerebral tumour (first stage).	Chlorosis.
Chronic alcoholism.	Cachexia and wasting diseases.
Constipation.	Dementia, especially post-epileptic.
Cerebral hæmorrhage.	Dilatation of heart.
Cheyne-Stokes' breathing (which, with high tension, is ominous).	Diabetes.
Dilatation of aorta.	Diarrhœa.
Emphysema.	Exhaustion.
Glycosuria.	Fatty degeneration of heart.
Gout.	Febrile conditions, especially typhoid.
Hemicrania.	Diphtheria.
Hypertrophy of heart.	Influenza.
Hysterical seizure.	Neurasthenia.
Lead poisoning and other toxæmias.	Profuse hæmorrhage (except cerebral).
Malaria (cold stage).	Jaundice.
Meningitis.	Obesity.
Migraine.	Pleurisy (second stage).
Neuralgia and other conditions of bodily and mental sufferings.	Pulmonary tuberculosis, especially in late stage.
Pregnancy.	Pyrexia.
Pleurisy, acute (first stage.	Trypanosomiasis.
Uræmia.	Shock and collapse.
All kidney affections except amyloid, suppurative and tubercular, except in terminal stages, when cardiac weakness causes a fall.	Also by hot drinks, hot air, and by the action of certain drugs, such as amyl nitrate, choral, cannabis indica, nitro-glycerine, erythrol tetranitrate, and sodium nitrate; also by radiant light-baths, and by the use of certain high-frequency currents and an ascending galvanic current (Schnee).
A too nitrogenous or too liquid diet.	Insanity with mania.
Insanity with depression, including general paralysis of the insane; also found in healthy but apathetic individuals.	Lowered atmospheric pressure.
Cold weather.	Warm weather.
Descending galvanic current.	

And with many diseases, their cause, favourable or unfavourable, varies with the blood-pressure, and thus the advantage of being able to note accurately the effect of treatment upon the blood-pressure is in line with such other scientific observations as we may make.

The effect of surgical operations varies with the nature of the operation, some causing a lowering of the tension, others marked rise; the list of these would make this article too lengthy.

Occupation has a decided influence, those occupations requiring great muscular exertion and exposure tending to give the highest pressure. However, it is being more and more recognised that those which entail mental anxiety and nervous strain are very prone to cause high pressure.

Posture makes some difference. Between the upright and the supine position the instrument will register a difference of about 10 mm.; if it be greater than this a suspicion may be entertained that compensation for the influence of gravity is insufficient.

In several diseases the knowledge of the state of the patient's arterial tension is of the greatest importance in diagnosis and prognosis.

In chronic pulmonary phthisis, when the blood-pressure is normal, or above normal, we may hope for a favourable termination, whereas a lowering

of the pressure is in most cases an unfavourable sign.

In the differentiation between paralysis caused by cerebral hæmorrhage and that caused by syphilis the sphygmomanometer will give a definite diagnosis, even though no history be obtainable.

In eclampsia, even though there is an improvement in the subjective symptoms and an increase in the quantity of the urine, if the blood-pressure remains high the prognosis is grave.

Where sugar is found in the urine my own experience is (I put it forward with some diffidence owing to the few times I have made observations) that where there is a normal blood-pressure the disease can be readily and favourably influenced; where it is low, even if we can reduce the sugar, we have a case of true diabetes with which to deal.

Careful observations by the sphygmomanometer aid in the choice of an anæsthetic, chloroform lowering the blood-pressure. Thus it would not be chosen in a case of low arterial tension, while, as Cushing* has pointed out, there is a tendency towards a rise in blood-pressure during the administration of ether, this anæsthetic would not be selected either in cranial operations or given to those patients who suffer from high arterial tension.

In patients suffering from arterio-sclerosis, frequent blood-pressure readings should be regarded as of the greatest importance in diagnosis as well as in treatment. A patient with failing power of concentration and enfeebled memory may come complaining of a slight dizziness, headache, irritability, and ill-defined morbid fears; the sphygmomanometer will decide whether we are dealing with a simple neurasthenia, or a patient liable at any moment to an apoplectic seizure, and when we consider the serious results of the latter, should we not use every means at our command to recognise its Damoclean danger and to forestall it?

In cases of heart disease, and even in those where we can detect no valve lesion but suspect a lack of muscular tone, a sphygmomanometric reading taken sitting, and again after a brief exercise, will enable us to decide whether to warn or reassure the patient. If the second reading is higher, then we have a normal muscular tone, but if not raised, or certainly in the event of its being decreased, the patient is in danger and needs careful attention.

The relation between blood-pressure and insanity is of enormous importance, both in recognising it as a causative factor and possibly giving treatment with a view of preserving sanity as well as the treatment of already existing insanity. There are other diseases and conditions in which diagnosis and treatment are assisted by the frequent reading of arterial tension, but if I have shown, however incompletely, that by the aid of the sphygmomanometer we may possibly get a warning of the advance of old age, help in the treatment of insanity, and prevent the occurrence of it in borderline cases, add another precautionary measure to the surgical operation and to general anæsthesia, assist in the prognosis and treatment of heart disease, differentiate in cases of glycosuria, anticipate and diminish the chances of nephritis or forestall an apoplectic seizure, I shall have justified the title of this paper—"The Significance of Sphygmomanometric Readings."

February 27th, 1911.

* 'Boston Med. and Surg. Journ.,' vol. cxlvii, March 5th, 1903.

Mr. Arbuthnot Lane, at Guy's Hospital, in the course of his teaching lately demonstrated in a case of ileal kink, in which there was also present a duodenal ulcer, that a kink or obstruction existed at the junction of the duodenum with the jejunum. He ascribed the symptoms which characterised duodenal ulceration in its several stages as being due to the stasis arising from the kink at the junction of the duodenum with the jejunum. According to Mr. Arbuthnot Lane the obstruction in this situation was a variable one, and depended largely on the general condition of the patient at the time. As in the case of the ileal kink, the obstruction might be purely static or brought about by the development of a band which developed in the under surface of the transverse colon, and was so placed as to control the duodenal jejunal junction in certain circumstances. The performance of a gastro-enterostomy removes this obstruction by rendering the kink impossible after the operative interference by the fact of the part of the bowel in which the kink occurred being straightened out. The formation of an aperture between the stomach and the jejunum plays no part in the relief of the symptoms of duodenal ulcer providing there is no mechanical obstruction in the first portion of the duodenum. In other words, the conditions that bring about the duodenal symptoms are simply dependent on the general condition of intestinal stasis.

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WHEN TO EXAMINE AND WHAT TO LOOK FOR.*

By ARTHUR E. GILES, M.D., B.Sc., F.R.C.S.,
Surgeon to the Chelsea Hospital for Women; Gynæcologist to the Prince of Wales's General Hospital, Tottenham.

GENTLEMEN,—I wish to make a few practical observations on the subject of the examination of patients presenting symptoms referable to the pelvic organs. In my book on 'Gynæcological Diagnosis' I concluded with the following words as the result of deliberate conviction: "The most expert and the most experienced must make mistakes in diagnosis; there is only one unpardonable sin, and that is to make a mistake as the result of careless or inadequate examination, or from having failed to make any examination at all." There are patients who refuse to allow an examination; I do not say that the medical attendant should stand on his dignity and withdraw from attending the case—there is no true dignity in such a course—but he should make it clear that he will not hold himself responsible either for correct diagnosis or for proper treatment until he has been given an opportunity of ascertaining the facts of the case. In the meantime his task will be to set himself to win the patient's confidence, and he will soon find that she will be willing to do or to submit to anything that he can show her to be necessary for her health. There are a few conditions under which it is permissible and even proper to prescribe without examination; I shall endeavour to define what these conditions are.

I will begin with a few general propositions:

Vulvar swellings.—When a swelling in the region of the vulva is complained of, the vulva should be inspected, because it is impossible to guess what the swelling is: we may find such widely different

* A lecture delivered at the Medical Graduates' College and Polyclinic.

conditions as Bartholinian cyst, hernia, hydrocele of the canal of Nuck, urethral caruncle, condylomata or other venereal sores, cystocele, rectocele, procidentia, simple tumours of the vulva, and carcinoma of the vulva.

Metrorrhagia.—When this is complained of, always make a vaginal examination, under anæsthetic if necessary. Look for polypus, uterine fibroids, carcinoma of the cervix, vagina, or vulva, retained products of conception, and extra-uterine pregnancy.

Abdominal swelling.—Always examine the abdomen; look for pregnancy, fibroids, ovarian cyst, and hydroperitoneum, and bear in mind the possibility of abdominal tumours generally and simple enlargement of the abdomen due to fat or flatus.

I may remark that while patients may object to a vaginal or "internal" examination, the examination of the abdomen and breasts never hurts susceptibilities, and yet often gives valuable information. It may also give an indication as to whether a vaginal examination is necessary or desirable or can be dispensed with.

Never make a diagnosis without first making an examination.

Never suggest diagnostic possibilities except as a reason for making an examination; and even then it is better to make the symptoms and not the possibilities the reason for the examination. A wise reticence inspires more confidence than unsubstantial conjectures. When a careful examination still leaves the diagnosis doubtful, throw the responsibility on the state of medical knowledge by such a formula as "It is impossible to say": do not incur the charge of ignorance by saying, "I do not know."

A rectal examination may give information as to pelvic tumours or displacements; but a vaginal examination is nearly always necessary to determine the cause of hæmorrhage. In making a rectal examination do not be misled by the cervix: it normally projects quite prominently into the rectum, and must not be mistaken for a displaced fundus.

Lastly, open inspection of the vulva is always better than digital fumbling.

Coming now to the more particular subject of these remarks, "when to examine and what to look for," we may consider it under three headings, according as we have to deal with unmarried girls, unmarried women, or married women.

(1) *Unmarried girls (say under twenty).*—If a girl complains of dysmenorrhœa, or of a recent or occasional increase in the monthly loss, it is proper to treat the case in the first instance without examination: suitable drugs and hygienic measures should be tried.

If the dysmenorrhœa fails to respond to treatment and is incapacitating the patient, or if the monthly loss continues excessive and is producing anæmia, or if persistent pelvic pain is present, examination becomes necessary. The first step should be to make a rectal examination, looking for flexions and displacements of the uterus and tubal or ovarian enlargement.

If the rectal examination is negative, and in any case if metrorrhagia is complained of, a vaginal examination must be made.

Whenever possible this should be done under an anæsthetic, and it is well to be prepared to proceed at once to dilatation and curetting if necessary.

With primary amenorrhœa, if there are no symptoms the case should be left alone and no examination made. If the patient complains of periodic pain or abdominal swelling, look for malformations, especially atresia of the vaginal orifice with retained menses.

In the absence of symptoms it is desirable to make an examination if the patient is distressed or her friends are uneasy about the non-appearance of menstruation, or if marriage is contemplated. Under-development, pseudo-hermaphrodism and other malformations are the things to look for, and the proper procedure is first to inspect the vulva: if this looks normal, a rectal examination is made to determine the size of the uterus; and if this is not conclusive a vaginal examination is made, under anæsthetic if necessary, to determine the size of the uterus and ovaries.

When there is secondary amenorrhœa examination may or may not be called for. Thus, with a history of gradual diminution in the amount lost and symptoms and signs of anæmia, phthisis, acromegaly, or other serious constitutional disorder, an examination is not necessary. On the other hand, when menstruation has ceased abruptly after going on much as usual, and the girl looks well and is not anæmic, an examination should be made: in the first instance the abdomen and breasts are examined, and if there are no signs of

pregnancy further examination may be deferred. With positive signs a vaginal examination should be made, and this will be easy in such a case, as the hymen will almost certainly not be found intact.

In cases of doubt always examine the abdomen.

The same remark applies to cases in which the complaint is of abdominal swelling. The three principal causes of abdominal swelling in young girls are pregnancy, ovarian cyst, and tuberculous peritonitis. If the abdominal examination is not conclusive, inspect the vulva and test the hymen.

When the complaint is of yellow discharge from the privates, the vulva should be inspected. The two conditions to look for are vulvo-vaginitis from some kind of non-sexual infection and gonorrhœal vaginitis. If the hymen is intact the former may be assumed; if it is torn, the case is probably one of gonorrhœa. A culture of the pus should be made whenever possible in order to arrive at a positive diagnosis.

(2) *Unmarried women.*—Most of the rules just laid down apply to the case of unmarried women; but there are additional reasons for examining and more things to look for.

Menorrhagia in unmarried women may be treated with drugs, without examination, for two or three months, but the medical attendant should always state at the outset that if the flow is not checked by drugs an examination will be necessary.

When this turns out to be the case the two principal things to look for are uterine polypus and uterine fibroids; but the position of the uterus and the condition of the tubes must also be noted, since retroversion and pyosalpinx may both produce menorrhagia. With metrorrhagia there is only one rule for single and married women, young and old, namely, "Always make an examination at the earliest opportunity." In the class that we are considering metrorrhagia leads us to look for polypus and fibroids in women up to the age of the menopause; at and beyond that age we must look for carcinoma of the body of the uterus.

Persistent backache or bearing-down pains necessitate examination: Look for displacements and endometritis.

Chronic pelvic pain associated with xanthorrhœa (yellow discharge) also necessitates examination: Look for tubal disease.

In unmarried women the method of procedure should always be:

(i) Examine the abdomen. If negative—

(ii) Inspect the vulva. If the hymen is intact—

(iii) Examine by the rectum. If this is inconclusive—

(iv) Examine by the vagina, under anæsthesia if possible.

(3) *Married women.*—When married women present symptoms pointing to trouble in the pelvis there is no reason to refrain from making an examination; and the man who attempts to treat any such troubles without making an examination is courting disaster. I would make only this one exception, that slight pains in the pelvis, slight white discharge, and slight menorrhagia may be treated symptomatically in the first instance, examination being reserved for those cases in which there is not speedy improvement. It is always much better to make an examination and find nothing of note than to omit to do so when some organic disease is present. Moreover, the patient who is unreasonable enough to object to a necessary or a desirable examination is not in the least likely to excuse her doctor if some important condition is discovered by someone else; she will be the first to say that he should have insisted that an examination was necessary.

Dyspareunia in the newly-married calls for examination, chiefly because the persistence of this trouble may lead to much domestic unhappiness: Look for malformations, rigid hymen, vaginitis, and vaginismus.

When dyspareunia comes on later in married life look for tubal and ovarian disease.

When a patient complains of sterility, three classes of case may be differentiated:

Sterility with dysmenorrhœa: Look for anteflexion, stenosis, and under-development.

Sterility with xanthorrhœa: Look for pus-tubes and pelvic inflammation.

Sterility with menorrhagia: Look for fibroids.

We might add a fourth category, and say—

Sterility with normal pelvic organs: Look for impotence in the man.

When a married woman complains of persistent pelvic pain the principal things to look for are tubal disease, tubal pregnancy, impacted tumours, displacements, and carcinoma. A diagnosis of "ovaritis" should never be made, because the term

is a blot on nomenclature; "oöphoritis" is a good term, but may be a bad diagnosis. A diagnosis of "oöphoritis" should never be based on the existence of iliac pain or tenderness; it should be reserved for those cases in which an ovary presents very marked tenderness on bimanual manipulation. True oöphoritis is almost always associated with tubal inflammation.

At the risk of repetition, I must say that irregular hæmorrhage always calls for early examination: Look out especially for fibroids and carcinoma of the cervix. In younger married women look out for missed or threatened abortion and for extra-uterine pregnancy. When a woman gives a history of an early miscarriage and no fœtus has been seen, look out for extra-uterine pregnancy.

Never treat hæmorrhage at the time of the menopause without examination.

With reference to examination in connection with pregnancy, I will submit the following observations:

When a married woman states that she has missed some periods, never assume that she is pregnant; and if she has not missed any periods never assume that she is not pregnant. Always examine before expressing any opinion.

Generally speaking, five examinations are advisable in connection with pregnancy, labour, and the puerperium:

(1) Examine early in pregnancy, to make sure that the patient is really pregnant; and in the case of a primipara, to ascertain the proportions of the pelvis.

(2) Examine about mid-term to see if the pregnancy is normal; it may be complicated by the presence of tumours.

(3) Examine in the last month, to ascertain whether the presentation is normal and whether the head is likely to engage. A faulty presentation can be corrected beforehand by external version.

(4) Examine at the conclusion of labour to see if there are any tears or other abnormal conditions.

(5) Examine a fortnight after labour to see if the uterus is in normal position and has involuted satisfactorily.

The first examination will prevent the fiasco of a patient waiting on for a confinement that will never come off because she has never been pregnant at all; such a fiasco is most liable to occur in the case of an elderly bride who is getting stout, or perhaps has an ovarian tumour.

The second and third examinations will avert many abnormal labours, and eventually save the doctor's time as well as safeguard the patient's health. The last two examinations, carried out carefully, and followed by the correction of anything that is wrong, would do a great deal to remove the reproach that the obstetrician provides work for the gynæcologist.

Another definite indication for examination during pregnancy is found when a woman who has missed two or three periods complains of undue frequency of micturition, of difficulty in passing water, or of inability to retain it. In all these cases look for retroversion of the gravid uterus. Early examination may avert the complication of impaction of the uterus and over-distension of the bladder.

March 6th, 1911.

Trifacial Neuralgia.—V. P. Blair reports the results of treatment by deep injection in twenty cases of true tic, all showing similar clinical features, the neuralgia being confined to the distribution of one or more branches of the fifth nerve. This, judging from the anæsthesia caused by the injections, seems to vary a little in different cases, in one or two the anæsthesia crossing the medium line or extending behind the ear. The pain was always paroxysmal and sometimes irregularly periodic, always in the same place and usually confined to one side. The trunks and branches of the affected nerves were not, as a rule, tender, but there were almost always special points, irritation of which would cause twinges in the pain spot. The patients acquired a certain habituation to pain in the older cases. Fifteen of the twenty patients were subjected to deep injections of alcohol, and with other anodynes with alcohol as a basis, though slightly varying otherwise. The details seem to warrant the conclusion that the more definitely a paroxysmal pain has been confined to certain spots, especially on the second and third division, the greater becomes the relief from deep injections. The procedure, he thinks, cannot be absolutely free from danger, but it is probably safer than a Gasserian ganglion operation.—*Med. Record*, vol. lxxix, No. 7.

MODERN DISCOVERIES REGARDING PLAGUE.*

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FOR upwards of 250 years England has been free from plague, and the occasional outbreaks in southern Europe have invariably been traced to infected ships from the Far East, and have been strictly limited in area.

The seat of origin of former great epidemics or pandemics has never been definitely settled, although their course through Europe has been traced. To this extent the present pandemic differs from former ones, for its origin is known to have been Yunnan, in China.

Since Hong Kong was acquired by Great Britain in 1841 the veil of mystery which hid the face of China from the European since time immemorial has to a great extent been removed; travellers have made their way through this great country and have brought to light much that has been of service to mankind generally. One such traveller, Monsieur Rocher, reached the Province of Yunnan, on the eastern borders of Upper Burmah, in 1871, and reported that he found plague prevalent among the people. The province, chiefly inhabited by Mohammedans, was at that time in revolt against the Government of China, and Rocher says that plague had devastated the country since the outbreak of the rebellion, although from local reports he judged that it had been introduced into the country previously to this, probably by caravans. He had the popular belief that the disease was due to poisonous miasms exhaled from the earth, and cited as a proof of this that the smaller animals, especially rats, living in drains and underground, were the first to be attacked. "When they become ill," he says, "they come out in numbers, invade the interiors of houses, run about madly, and after describing some circles they fall down dead. The same phenomena may be observed among other animals, large and small." After that the population is attacked. "To aggravate the situation," he continues, "the superstitious Chinese, instead of burying the plague corpses, merely place them in biers exposed to the sun on the slope of a hill or in an open field. It follows that travellers or others roaming near the plague-stricken villages are almost asphyxiated by the nauseous stench proceeding from decomposing bodies."

It is not known how plague was introduced into Yunnan, but it may have become infected early in the nineteenth century by Mohammedan pilgrims returning from Mecca, where the plague was then prevalent, or the infection may have been even earlier still. It has remained an endemic area of the disease ever since Rocher's visit.

There is no doubt that plague was distributed broadcast by means of the rebellion, which was so determined that it took the Chinese Government twenty years to subdue it. All that time the rival armies were carrying the infection from place to place, and as each regiment was in due time relieved and returned home, it carried the plague bacilli with it.

In the year 1894 the plague had travelled as far as Canton, the chief port and most important city of Southern China. Dr. Mary Niles recorded the first case from that city in January, 1894, when she was called in to see a lady of distinction who was suffering from a "boil," which Dr. Niles certified as a case of plague. Other cases followed. "It has been noticeable to the people," she reports, "that rats in infected houses have died. In the house where a child from school was visiting when she took the disease, thirteen dead rats were swept out one morning. . . . One of the officials, I am told, offered ten cash for every dead rat brought to him. He had collected 35,000 in one month; 2000 were brought to him in one day."

From Canton, a central port in communication with every important coast town in China, the disease was bound to spread. Hong Kong is only eighty miles distant from Canton, and is in daily river communication. It is not surprising, therefore, that plague was found to be present in Hong Kong four months later.

In that year (1894) Dr. Kitasato, of Tokyo, discovered the plague bacillus there, and Dr. Yersin made the same discovery a little later. Every year since then the plague has reappeared in Hong Kong and Canton, with the exception of 1895.

There is constant trade intercourse between Hong Kong and Bombay, and it is strange that

* A Gresham Lecture.

public anxiety was not aroused as to the probability of the latter port becoming infected. Even when the first cases occurred among people living near the docks, in March, 1896, they were not at first thought to be plague. The people attacked were those who had trade dealings with China, and the infection may have been introduced in several different ways.

A member of the crew of a ship coming from Hong Kong may have carried the disease on him, or the bacilli may have been present in the cargo, or, more probably, the rats of a ship may have been infected. These foreign rats would infect the Bombay rats, and from the fleas on the dead rats the plague was doubtless conveyed to man.

Bombay had been free from plague for nearly 200 years, and had become careless, just as we in England do not take the question of plague infection very seriously, because we have come to think of plague as an antiquated scourge, one of the terrors of the past, confined to uncivilised and filthy people. Whatever the reason, Bombay undoubtedly did not grapple with the question at once. It accepted the sudden high mortality near the docks as being due to malarial fever and lung disease, for the greater number of cases do not appear to have been severe, nor did they end in death. There was, however, a great mortality among the rats, and whenever dead rats were found in numbers in any locality human plague followed.

Six valuable months were lost, but public anxiety was aroused in September, 1896, when Dr. A. G. Viegas pronounced the disease to be probably plague. The Government then lost no time in summoning their bacteriologist, Mr. Haffkine, who confirmed the diagnosis by bacteriological examination. The Health Department at once set to work to disinfect and cleanse the infected houses and to segregate the sick. But the difficulties with an oriental population of the type there met with are enormous. Superstitious and fanatical to a degree which can be little understood by western folk, they resented every attempt made to check the spread of the disease. All they wanted was to be left alone. If the plague killed their neighbours it was the will of God, and that officials should step in to try and stem the hand of the destroyer was impious and not to be tolerated.

Prof. Simpson, who is well acquainted with India, writes:

"Every sanitary measure was opposed. Denunciations and protests were soon followed by active demonstrations of ill-feeling, by stoning the officers engaged in plague work, attacking the ambulances, and even storming the plague hospital. To such a pitch of excitement were the rougher classes of the population aroused that there were good grounds for fearing a riot, and, worse than a riot, there were threatenings of an exodus of the whole conservancy staff and of the dock and mill hands, and possibly of the police themselves. To allay this state of feeling the notification authorising compulsory removal to hospital was withdrawn three weeks after its first appearance. This was on October 30th. The difficulties which beset the local authority in its endeavour to stay the progress of the disease were not removed by this concession Popular feeling, moved by wild rumours, the offspring of an excited imagination, or evil design, or ignorance, was swayed first in one direction, then in another, but never in sympathy with, or in support of, the sanitary measures devised to check the disease. And so the plague continued to spread. The people had lost confidence in medical treatment. It was not a question of notification of of the sick by medical men, but of finding out the sick and dead, and cleansing and disinfecting the houses. The disease spread remarkably slowly, considering the conditions which it met with."

A curious fact was observed. Although when the people took alarm and fled from the infected districts into towns not yet attacked, the infection did not appear to be conveyed to any great extent by them, but to whatever place those grim precursors of death, the rats, migrated, there the plague broke out.

Though the native population had been incredulous and philosophical at first, it was seized with panic as the mortality increased from week to week, and reached its climax in February, 1897. The people fled from Bombay, houses and shops were deserted, and the infection was conveyed into all the country round. The Government now took over the plague operations; its work has been hard, continuous, and wise, but in spite of all efforts the pandemic increased from year to year, dying down in the hot months and blazing out again in the cooler weather with ever-increasing fury.

The disease was soon not confined to the

Bombay Presidency, but reached far-away localities. The development of the epidemic was at first slow in every place it touched, but in spite of every endeavour to stay its progress it spread throughout the Peninsula, some places being ravaged, others but slightly affected, and some escaping altogether.

At first the plague seemed to affect the general death-rate but little, but every year, till recently, it has grown, and is as bad as any scourge that has ever attacked India. In the first year of the epidemic there were 30,000 deaths from plague, by 1903 the number had risen to little less than one million per annum, and in 1907 it attained its maximum of 1,315,880 deaths in British India.

From 1896 to September, 1910, inclusive, no fewer than 6,680,650 deaths have been certified in India from plague, and very many more have undoubtedly taken place. The authorities have had the greatest difficulty in tackling the cases, for, wherever possible, they have been concealed and the deaths registered as being due to some other disease, so that the relations and friends might escape the inconvenience of disinfection and isolation. Any attempt to search dwellings or otherwise effect a change in the habits of the natives is likely to interfere with their religious sentiments and prejudices, and the Government do all in their power to avoid this.

The Government of India has had largely to fall back upon three main measures:

(1) The temporary evacuation of quarters in which plague is prevalent, in this way removing man from the danger of contact with infected rat fleas, and for this purpose the Government has been ready to provide temporary dwellings to minimize the discomforts likely to arise in those who voluntarily submit to the evacuation of their houses.

(2) Inoculation with Haffkine's prophylactic fluid.

(3) The systematic destruction of rats, together with measures to prevent these rodents from obtaining food in human dwellings, and the rendering of dwellings, as far as possible, rat-proof.

The death-rate very much abated in the years 1908 and 1909; in the latter year only 175,114 deaths from plague were certified. But the diminution, unfortunately, was only temporary, for in the first eight months of 1910, 372,000 deaths took place.

A noticeable fact is that Europeans appear to have a certain immunity; among the many sanitary officials, whose work has been extremely arduous, and among the nurses, comparatively few have fallen victims to bubonic plague.

As long as plague is epidemic in Hong Kong and Bombay, two cities which have dealings with nearly every other great port in the world, it is inevitable that the infection should be distributed, and, as a matter of fact, it has been carried to every quarter of the globe. In Egypt and South Africa it has extended some way inland, but elsewhere it has mostly been confined to sea-ports, and nowhere has it assumed great proportions as yet.

Last year's accounts report cases from China, Japan, Straits Settlements, the Persian Gulf, Australia, Hawaiian Islands, South America, United States, Africa, North and South and Central, and the Azores. In Europe there were some ship-borne cases at various ports, and other cases from Russia and Turkish territories. Finally, you will all remember the cases in Suffolk last year, which I shall hope to discuss in my next course of lectures. Some infected rats were found in the London docks and at Hull in 1909, and again in London in 1910, but in every case the disease was said to have been arrested.

Until recent years the nature of the infection of plague was unknown. Among early authorities the belief was extant that pestiferous emanations proceeded from the soil and poisoned the atmosphere, and their belief in this doctrine was strengthened, as we saw in the case of the traveller, Monsieur Rocher, by the observation that rats and mice became ill and died in great numbers before the advent of plague. These animals, living underground, they argued, would be the first to be affected by its poison.

When the plague broke out in Hong Kong in 1894 the Japanese Government sent two of their scientists, Kitasato and Aoyama, to study the disease. They arrived in Hong Kong on June 12th, and two days later Kitasato discovered in the body of a person who had died of plague numerous bacilli in the buboes, liver and spleen, and blood of the heart. Similar bacilli were found in a living case of plague on the same day. Cultures were made with these bacilli and various animals were injected, with the result that all such animals, with the exception of pigeons, who are not

susceptible, died shortly after with symptoms similar to those exhibited by human cases of bubonic plague. The French Government had meanwhile sent Dr. Yersin, who arrived at Hong Kong a few days later than the Japanese Commission, and independently discovered the same bacillus.

When this bacillus is isolated from a plague case and is injected into a rat or guinea-pig or some other animal, that animal contracts the disease.

There have, besides, been several human cases in laboratory workers in Europe, who have, unfortunately, been accidentally infected with the bacillus and have died of plague, after the bacillus had travelled in cultures all the way from India or China to a country where there could have been no possibility of any local infection.

The plague bacillus is a short thick rod, rounded at the extremities and slightly ovoid in shape.

The bacilli are very numerous in the buboes, in the sputum of pneumonic cases, and in the blood of septicæmic cases. They are found after death in the buboes, in the spleen, liver, lungs, bone-marrow, bile, urine, peritoneal fluid, and the fluid of the brain.

The bacillus retains its vitality for many days unless exposed to dry heat; even intense cold does not affect it, but exposure to the direct rays of a hot sun kills the micro-organism in a few hours.

The houses of the native poor in India and in other tropical countries afford the bacillus every facility of life. They are dark, close, with no ventilation except through the door and through the tiles of the roof, by which means the smoke from the fire escapes, for there are no chimneys. If there are windows, they are either boarded up or closed with piles of rubbish. In every house there are sacks of grain, which attract the rats, and the overcrowding is appalling, families and animals all living together. One house visited by the Plague Commissioners is described thus:

"It was a house of three fairly large rooms and a verandah. It was occupied at the time of our visit by twelve adults, three children, four buffaloes, six bullocks, a goat, a dog, a cat, and three fowls." The houses of the very poor are merely single-roomed mud huts with tiled roofs.

Plague is primarily a rat disease, and all rats are subject to it. Professor Koch, after a visit paid to India in 1897, emphasised the very important fact that rats are concerned in the spread of the disease, and he later defined plague as a disease of rats in which men participate.

Credit must be given to Mr. J. Cantlie, who was the first in this country to draw attention to the close relationship which exists between rat plague and human plague, in a paper which he read in December, 1896, upon the disease studied by him in Hong Kong.

How does the rat infect man? Plague is a rat disease, and as long as there are plenty of rats the infection may not spread to man. It is when many rats have died, and when their dead bodies are lying about, that man develops the disease. In all countries where plague is present the people have learnt this fact, that dead rats herald plague among men. If a hut is vacated after an outbreak of plague the infection seems to hang about it for a time, so that if anyone enters it within the first week or so, even for a few moments, he is liable to catch plague. On the other hand, doctors and nurses who attend the sick in hospital rarely catch it. Bubonic plague is, therefore, not infectious in the ordinary way.

The yearly outbreaks of plague in Hong Kong and Bombay have aroused the scientific curiosity of men all over the world, and every opportunity has been afforded them for investigation. In Bombay alone no fewer than six scientific commissions have studied the disease in the last few years, and in other countries the epidemics have aroused similar interest.

In searching for the solution of the causation of the disease, the Plague Commissions first came to the conclusion that the mode of entry of the bacillus was through the skin. It was at first thought possible that natives became infected through their bare feet, when a small abrasion might come in contact with bacilli left by rats, but this theory could not possibly account for the thousands of cases attacked. It became evident that there was in all probability a living intermediary between rat and rat and rat and man which could carry the infection.

Rats are infested with fleas, and these blood-sucking insects were found on examination by Ogata, a Japanese, to contain plague bacilli when they were taken from a plague-infected rat, even some time after they had sucked the blood, and the fleas did not appear to be at all affected by the presence of the bacilli in their bodies. In 1897

Ogata next succeeded in producing plague in mice which he had injected with crushed fleas taken from infected rats.

Here, then, seemed to be the missing link in the chain of evidence. When the plague-stricken rat dies the fleas leave the dead body as it becomes cold, and either transfer themselves to other rats, their natural hosts, or, failing these, to men or other animals.

The common rat flea is not the flea which generally infests man, but it has been proved that it will do so, and frequently does do so, when its usual host, the rat, is dead and the flea is starving.

A flea which has gorged itself with blood may remain alive for three weeks, and the bacilli within it remain virulent for fifteen days; and it can, therefore, be the carrier of plague in clothes, or luggage, or cargo, if it finds rats at the end of a fortnight's journey to which it can communicate plague.

The value of this discovery of the plague carrier cannot be over-estimated. We now know with certainty that the disease is a rat disease, and that the fleas on the rats are the means of conveying plague from rat to rat and from rat to man.

A confirmation of the flea theory is to be found in the periodical rise and fall of plague epidemics, which occur every year where the disease is prevalent, and which coincide with the rise and decline of the flea season.

There is a season for the prevalence of fleas even here in England, where they are far more numerous in the warm weather than in the cold.

In tropical countries the flea season is much more marked. Very careful observations on the question have been conducted in various parts of India, and certain definite conclusions were obtained.

From Belgaum, for instance, the observations indicate:

"(1) That there is in Belgaum a definite seasonal variation in the prevalence of the rat flea.

"(2) That this seasonal prevalence of the rat flea is closely correlated with the seasonal prevalence of plague.

"(3) If it be granted that the seasonal prevalence of fleas is likely to be constant year after year, the height of each epidemic that has ever occurred in Belgaum was in a month of the year in which the rat flea was very prevalent."

Again, from Poona:

"(1) When infection (of the district) takes root immediately before the flea season or with a rising flea prevalence, a severe epidemic follows.

"(2) When infection takes root immediately after the flea season, or with a declining flea prevalence, a less severe epidemic results.

"(3) The epidemics in Poona city generally terminate owing to a combination of adverse factors, viz. a decrease in the number of fleas, a decrease in the number of rats, and an increase in the proportion of immune rats to susceptible rats."

Lieut.-Col. W. B. Bannerman, of the Indian Medical Service, in a most interesting pamphlet on 'Plague in India,' tells us of some experiments, accidental or not, which go to prove the flea theory:

"A striking illustration," he says, "was furnished by a house in Bombay where some hundred people lived in a number of small rooms. Rats died in the tenement, and living rats disappeared. Two days after the inhabitants were so troubled with fleas that they had to forsake their rooms and sleep in the verandahs. A few days more, and plague appeared among them. A search for fleas on the people revealed the interesting fact that a large proportion of the capture were rat fleas, and some of them contained plague germs in their stomachs. In the light of the fuller knowledge that subsequent experiment on animals threw on this series of events, it is easy to understand what happened in this crowded tenement. First, plague among rats; secondly, death and disappearance of the rats; thirdly, starvation of the fleas; fourthly, attack on the people by the fleas, some of which were infected; fifthly, development of plague among the people."

Again, "It having been proved that rat fleas are in certain circumstances found on man, it became necessary to determine whether they could transmit plague itself. A glass box was made, large enough to contain a couple of wire cages in which rats or other animals might be confined. The glass box was covered in by muslin, so as to make it insect-proof. Each animal cage stood in a tray filled with sand to absorb the discharges; so that no possible communication could take place except through the air, or by some insect capable of passing from one cage to the other.

"Into one of the cages was put a rat artificially infected with plague, and along with it ten to

twenty rat fleas. When this rat died a healthy one was placed in the other cage contained within the same glass case. Eight hours after, when its body was cold, the corpse of the first rat was removed and examined to make quite certain that it had really died of plague. If the healthy rat in the second cage died subsequently, it was also examined to determine the cause of death. When the experiment was performed as above described with English rats, especially imported so as to ensure that they had not previously been exposed to plague infection, in eleven out of sixteen experiments the healthy rats contracted plague."

Another question to be decided was whether plague could be conveyed from one animal to another by some means other than the flea. For this purpose experimental rooms similar to those inhabited by the poorest natives of Bombay were constructed in such a manner as "to be flea-proof, and all animals placed in them were carefully searched to make sure that *no fleas* were introduced accidentally." Fifty healthy guinea-pigs were put into one room measuring seven by six feet, and then ten others artificially infected with plague but without fleas were added. The room was purposely not cleaned out, so that in a few days the animals were living in most insanitary conditions. As the infected guinea-pigs died of plague the bodies were left for twenty-four hours before removal, so as to afford every chance for contagion to play its part. Yet not one of the fifty healthy animals died.

"It was likewise ascertained that animals suspended at more than the height of a flea's jumping powers above the floor invariably escaped plague, while those suspended in cages into which fleas could jump were liable to contract it."

Let us now see how the rat-flea theory gradually conquered scientific minds. I have already told you that in 1897, Ogata, in Japan, communicated plague to mice by inoculation with crushed fleas taken from infected rats. In the following year, Simond, who had visited India, successfully repeated the experiment of Ogata, and also communicated plague from rat to rat by means of fleas. In 1902, Zirolia, who was working in Rome with a strain of plague derived from Naples, found that the plague bacilli not only survived in fleas for seven or eight days, but actually multiplied within the fleas and preserved their virulence. In the same year, Tidswell, a bacteriologist at Sydney, found that the common rat flea in Australia, which is identical with the common rat flea of India, would bite man if starving; this experiment was carried out by means of test-tubes, the mouths of which were closed by fine gauze through which the flea could bite.

Dr. Ashburton Thompson, Chief Government Medical Officer of Sydney, had arrived by induction at the conclusion that some intermediary insect was necessary, which had a power of taking the plague bacillus from the rat and inoculating it into man. It had long been known that in most cases of bubonic plague it was the femoral glands of the groin which were affected, and this showed that inoculation had taken place somewhere in the lower limb of that side. When plague attacked the coloured inhabitants of India and Egypt it was assumed that their legs were infected because of the absence of boots and stockings. But in Sydney, where the patients were all clothed as we are in this country, and where, therefore, their lower limbs were thoroughly well protected from any casual contact with infection by boots and socks or stockings and by trousers or petticoats, while their hands and arms and faces were fully exposed, the matter was different. For they were not inoculated in those exposed parts of the body, but in protected localities. It was thus evident that inoculation must have been effected by some agent to which neither clothes nor the skin offered any serious obstacle, by some agent to which clothes were no difficulty, and which could penetrate the skin without causing any noticeable pain or a visible wound. It was then deduced that only the flea answered all these requirements, and it was pointed out that fleas commonly breed in houses in dusty corners and in the cracks between the boards of the floor, and that they notoriously reach first the human legs in places where the fleas are numerous.

The next step in the argument was to enunciate that the plague season corresponded with the flea season. For instance, in New South Wales, as I also have observed in Egypt, from about February onwards is the time when sandy patches, far removed from dwellings, begin to swarm with fleas, when pet dogs are worried by the species of fleas which infest them as at no other time of year, and when a room in a fairly clean house may become for a time overrun with fleas.

Since we have all begun to be interested in fleas, and even to dare to talk about them, which used not to be permitted in polite society, we have become aware that different species of fleas have their proper hosts and prefer them to other kinds of animals. Human fleas do not usually infest rats, nor do rat fleas infest man. Why, then, should it be supposed that rat fleas ever leave the rat for man? In 1905, Capt. Liston, working in India, suggested that rat fleas remain with their proper host so long as it is available for food, but when it dies or is removed they proceed to seek food from the next nearest animals. It was found that dogs and cats saved their owners from being bitten by the rat flea so long as they were present to afford food, but if they were removed their owners were bitten. You may have noticed that the mistresses of pet dogs are not tormented with the fleas which distress the animal which they nurse in their laps.

But even when the authorities were convinced that rats and their fleas were the agents which introduced plague to man they were confronted with the difficulty of ridding the country of these pests. It was certain that the ordinary disinfectants were useless, that the thousands of pounds spent by the Indian Government at the beginning of the epidemic on every kind of disinfectant for clothes and houses had been so much money wasted. It did not affect the rats, who continued to die of plague and to leave their fleas behind them as an inheritance.

Very strenuous efforts have been made in Hong Kong, India, Japan, and other places where plague has broken out to destroy the rats. In many cities a small sum has been awarded for each rat brought to the authorities. But the greatest destroyer of rats is the plague itself. In 1901 about thirty men were employed for rat destruction in Hong Kong, and it is noteworthy that seven of them died of plague, apparently contracted at their work, and five others left on account of illness, the nature of which was not ascertained, though two of these five apparently died of plague.

It is extraordinarily interesting to think that we are indebted to Kitasato, the Japanese, for discovering the bacillus of plague, though it is a good instance of a prophet having no honour in his own country, for many classes of the Japanese population still refuse to destroy rats, believing them to be "guardian angels" of the Japanese Empire.

During 1909, in the city of Poona alone, which has a human population of 11,000, there were caught more than 240,000 rat fleas, and only two cat fleas and four human fleas were found on the rats.

Rats have been destroyed by hundreds of thousands wherever plague has broken out, and undoubtedly this has been a factor in checking its spread. But besides this, there is now a scientific method of protecting people against infection, and this method has been so well described by Colonel Bannerman that I propose to read his account to you.

"Mr. Haffkine, working in Bombay on the lines laid down by his great master, Pasteur, discovered that he could protect rabbits from plague by inoculating them with the dead germs of the disease. The rabbits so rendered immune could not be made to take plague afterwards as other rabbits easily could. After Mr. Haffkine had tested the harmlessness of an injection of these dead germs on his own person, and on several medical men at Bombay, an outbreak of plague in one of the Bombay jails afforded an opportunity of ascertaining whether such an injection would protect human beings actually threatened with plague. It was found that it does afford protection.

"How is such protection brought about? It is common knowledge that most infectious diseases are caused by bacteria, and that each disease is due to the action of a special germ. It is likewise known that in most of such diseases one attack renders a person free from liability to a second attack; the person has become immune, as we say. Now how does Nature produce this immunity? The experimental method has shown that bacteria do not act injuriously on account of their numbers in the body, but because they have the power of manufacturing a poison, or toxin, as it is called, which acts on the body and injures it. When this poison, or toxin, is poured out into the body, the body proceeds to evolve an antidote to the poison, and this is called an antitoxin, or anti-poison. If time and strength are granted, the body produces enough of this antitoxin to neutralise the toxin and destroy it, and the patient recovers, retaining in his body the power of rapidly producing this antitoxin, so that any fresh germs of this disease gaining entrance will be at once killed by it, and

no second illness will ensue. If we can in any way imitate Nature in this process without producing the disease itself, it is evident that we can protect people from an attack. Can we manufacture this toxin and introduce it in amounts that will be harmless to the body, and so start the manufacture of the desired antitoxin? We know from experiment that bacteria produce toxin while they are alive. It is possible to keep them alive while they are doing this by growing them on artificial medium, and then by heat or other means to kill them so as to make a preparation of lifeless bacteria and their toxins. The germs, being dead, cannot make any more toxin, and the toxin, being a lifeless chemical substance, cannot increase in amount. If, then, we take this lifeless preparation and introduce a limited amount of it into the body, it is found that the body will react to it and produce antitoxin, to neutralize the toxin introduced into it. In this way we can stir up the body to make antitoxin without subjecting it to the inconvenience and danger of an attack of the disease itself. It was by a similar process that Pasteur was able to protect sheep against anthrax, and fowls against chicken-cholera; and it was he who proposed to call all such preparations "vaccines" in honour of Jenner's immortal discovery, though they have nothing whatever to do with the cow.

"The anti-plague vaccine (or plague prophylactic) of Mr. Haffkine is made by growing the germs of plague in a broth rendered sterile by heat. After many weeks of growth the broth is seen to have become quite thick and turbid from the countless millions of plague germs that have been produced in the previously clear broth. When the proper time arrives the flask containing this mixture is placed in hot water so as to heat the contents to such an extent as to kill off all the germs. A small quantity of carbolic acid is added to preserve it, and it is then decanted into bottles which are hermetically sealed. What have we in these bottles? Dead plague germs, the toxin they have made while still alive, and the broth in which they grew. We can then measure the strength of this mixture and regulate the dose so that the amount injected is just enough to start the machinery of the body without doing any harm. In this way people are protected against attacks of plague just as they would have been by an attack of the disease itself if they had chanced to survive.

"In popularizing the plague prophylactic, the one difficulty that has to be fought against consists in overcoming the popular idea that it is the living virus of plague that is injected. Medical science has to contend sometimes, even in England, against credulous readiness to believe evil of others. What it has to contend with in India may be judged of by the fact that, when inoculation against the plague was started in the Punjab, it was popularly believed that the doctors were being sent round to propagate the plague, in order that human livers might be available, with which to make a potent drug that could renew the youth of the Empress, whose Diamond Jubilee had recently been celebrated!

"Yet, in spite of such ideas, the total demand for the prophylactic continues slowly to increase; its protective power is found to be so marked wherever it is put to a practical test."

The natives of India are urgently persuaded to submit to inoculation, and although the prejudice against it was at first fierce, it has gradually made way for a more reasonable attitude, and a large proportion of the population have benefited by it.

Thus, in a report dated 1909–1910, we read that in the Punjab some 41,000 persons submitted to injection with Haffkine's prophylactic, and in the year 1908, when the recollection of the previous year's enormous death-roll was still fresh in their memory, there were 53,629 such inoculations. As an instance of the efficiency of the prophylactic the following case may be given: Certain go-downs, or store-rooms, in India had been selected for experimental work on animals, and the men whose duty it was to go into them for tending these animals—many of them plague-stricken—were well immunized with Haffkine's prophylactic. They were invariably bitten by fleas, dozens of these vermin being found on their legs when they emerged, but not one of them contracted plague.

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March 6th, 1911.

WITH DR. GUTHRIE RANKIN AT THE DREADNOUGHT HOSPITAL, GREENWICH.

GENTLEMEN,—I show you here a girl with ordinary Bell's paralysis. You will remember having seen her two or three weeks ago. The cause of the paralysis seems to have been exposure, and at her previous visit we ascertained, and were able to demonstrate, that loss of taste had been sustained in the anterior two thirds of the tongue—evidence that the lesion is situated within the Fallopian canal. There is no history of otitis or otorrhœa; since she was last here her ears have been carefully examined in the aural room, and are reported normal. You can see that there is still complete lack of movement in the upper part of the brow, but she can now shut her eye better than she could, though recovery in that respect is still partial. The cheek remains in a condition of complete paralysis, so that she is unable to balloon her mouth or to whistle. The paralysis, as I pointed out previously, is not lessened by emotional conditions—indeed, efforts at smiling increase the contrast between the affected and normal sides of the face. There is no loss of power in the limbs. Since her last visit her face muscles have been massaged daily and stimulated by the application of a gentle Faradic current, and, in the way of medicine, she has taken regularly syrup of the phosphate and iodide of iron, with Fowler's solution. On electrical examination there was a late reaction to Faradism, but no polar changes to the galvanic current. In another month or six weeks we may therefore expect the child to be well again, and she is already showing marked evidence of improvement.

For the purposes of comparison here is an old case of a similar paralysis which Dr. Lucas has kindly asked to come up. The man is 72 years of age, and, as you see, he has a definite facial paralysis, which dates back nearly sixty years. From the history he gives us it was probably caused by an injury to the nerve inside the canal from an accident in boyhood. He is still unable to shut his eye on the paralysed side as completely as on the other. Though there are no marked muscular contractures, for which he has reason to be thankful, the whole side of the face looks smaller than the neighbouring sound side, and there is a notable twitching, which he says goes on continually, of the levator anguli oris muscle. He is deaf in the corresponding ear, but has no paralysis elsewhere, and has been well enough to lead an active and useful life. He has escaped conjunctival troubles, which is surprising when it is remembered to what risks the imperfect closure of his eye has exposed his conjunctiva during all these years. When looked at together, the objective appearance of the face in these two cases is exactly the same, but the injury to the nerve in the one instance is ephemeral, whereas in the other it created a permanent lesion.

Our next patient is a case of achondroplasia. I have seen the girl casually on two occasions, and first discovered her at the gate of the hospital one day as I passed out. She looked so dwarfed in comparison with the size of her head and her general appearance of age that I asked her how old she was, and she replied, "Seventeen." When one sees a case of dwarfism the first thing one suspects is cretinism. But the general appearance and the quick, intelligent way in which this girl answered questions was against the likelihood of cretinism being the explanation in her case. She grew, she says, like other children, until she was seven years of age, but since then has increased in height very little. It is at once obvious that the shortening of stature is due to arrest of growth of her legs, and especially of the femora. The arms, you will observe, are similarly below the normal length, so that her fingers reach but little below the rim of her pelvis. Ordinary rickety dwarfism is not like this. In it the disease has affected the spinal bones, and if the legs appear short it is because they are bent, the bones themselves not being arrested in their development. You will notice that when sitting this girl appears to be of the ordinary stature, her trunk not being much less than the average length for her age. She manifests some lordosis, so that the line of her shoulders falls behind the line of her hips. The abdomen is protuberant, and the umbilicus, which should be midway between the head and the sole of the feet, is markedly decentralised. Please note the sunken condition of the bridge of her nose; the big, square head and beaded ribs, which might belong to an ordinary rickety child; the projection of the lower part of the jaw; the extremely high arch of the palate; and the characteristic shape of the hands, the fingers being fore-shortened, broadened across

the ends, and separated at the terminal joints so as to present the trident configuration. All these points indicate that the dwarfism, in this case, must be attributed to achondroplasia, which we have not known much about until recently. The explanation of the sunken nose, high palate, and prognathus is that the two wings of the sphenoid bone and the basilar portion of the occipital bone have ossified too early, with the result that the skull has been unable to expand properly. Achondroplasic children are especially distinguished from cretinous children in that they are very intelligent, and this girl tells us that she was in the seventh standard at school, and that she is quite as sharp and clever now as her compeers. Achondroplasia was at one time regarded as a condition of congenital rickets, but it is now recognised that rickets and achondroplasia, though closely allied, are quite distinct. Cases are met with in which probably the two co-exist. In achondroplasia the pathological fault lies in the long bones, and in those that grow from cartilage only, and the child is "born short," but otherwise apparently healthy; whereas in rickets the whole bony framework of the body suffers, the disease does not develop until some months after birth; dentition, intelligence, and the power of walking are late in development, and head sweatings, restlessness, and mucous-membrane catarrhs are constant accompaniments. We cannot, perhaps, eliminate altogether rickets from this case, but the foundational fault is undoubtedly achondroplasia, and growth in stature will probably go no further than that now reached.

The next patient is a woman, æt. 28 years, who first came to the out-patient room on account of a swelling outside her throat, bulging of her eyes, and palpitation. The case is obviously one of exophthalmic goitre, characteristic manifestations of which are to be found in the neck, the eyes, and the heart. One other feature which is always present, but which she has not mentioned to us, though she admits to being very nervous, is muscular tremor.

It is interesting, as a matter of contrast, to turn from this woman for a moment to another patient who has been labelled as suffering from myxœdema. There is probably some doubt as to the correctness of this diagnosis, but the man portrays sufficiently the leading features of myxœdema to emphasise how diametrically opposed its manifestations are to those of exophthalmic goitre, though both disorders are, as you will remember, related to disturbance of the thyroid gland. This patient, you will observe, is lethargic and somnolent. His features are swollen and expressionless. His speech is slow and drawling. His pulse is not more than 68, the skin is dry and inelastic, and there is a certain amount of diffused swelling all over the body. There is no enlargement of the thyroid gland, and the eyes are normal. In the case of this exophthalmic patient, on the other hand, we note that, in addition to the three cardinal signs of her disease, the skin is flushed and perspiring freely; there is no swelling of the surface of the body; the patient is alert and nervous; her speech is ready, and is only marred by the nervous tremors which accompany the expression of her words. Her pulse at the present moment numbers 140, and the instability of her vaso-motor system is well exemplified by the striking dermographic state of her skin. Her heart is normal, but a loud systolic bruit is present over her enlarged thyroid. Both Stellwag's and von Graefe's signs are present in her eyes. She is married and has two children, the elder being five and the younger three years of age. She dates the commencement of this trouble from the birth of her first boy. Exophthalmic goitre comes on under all sorts of circumstances associated with strain, or excitement, or depressed health; it is not infrequently, as in this instance, a sequel of a first pregnancy. There is no time to enter now into the question of treatment, but we are all aware how unsatisfactory it proves in many of these cases. There is, however, one method of management which I desire to mention and which we will carry out in this case, namely, the administration of thymus-gland extract. I am well aware that it often fails entirely, but in my experience there are patients who apparently derive more benefit from thymus gland than from any other drug. I may mention one patient at the Waterloo Hospital, who, under its influence, has entirely lost her exophthalmos, her thyroid gland has diminished in size, her tremors have vanished, and her pulse, which was over 160 when first observed, has gone down to 80. The disease began, as in the case now before us, after the birth of her first child, and had lasted for about eighteen months before she came to the hospital. She has taken thymus extract steadily for more than a year, and

though still far from well, she is so much better that she can now get along with moderate comfort and do all her house-work. It is impossible to be sure that this is an instance of cause and effect, but, at any rate, it is certain that she experienced no striking change in her symptoms until thymus extract was given her. The theory that exophthalmic goitre is due to excessive activity and myxœdema to diminished activity of the thyroid gland receives general acceptance, but I think there are reasons for wondering whether this explains the full pathology, at least of exophthalmic goitre. Nerve troubles enter so largely into the symptomatology of exophthalmic cases that one is tempted to ask whether some central nervous disturbance is not an essential ætiological factor. A striking case came under my observation years ago, in which exophthalmic goitre developed at the menopause in a patient who had been subjected to prolonged nervous strain over many years. Treatment of all kinds was ineffectual, and eventually she was attacked by bulbar paralysis, from which she died. The occurrence may, it is true, have been merely coincidental, but it is suggestive of some close relationship between cerebral change and thyroid disturbance. This woman will be given 10 gr. of thymus extract three times a day, with 30 gr. of bromide of potassium every night at bedtime, and we will ask her to present herself again for examination after three months, that we may observe what results ensue.

Here is a new case which has just been admitted. The patient complains of pain on the left side and cough. The illness began in September with sickness every morning before breakfast, when a white frothy material was vomited. He subsequently developed a cough, which was accompanied by a moderate amount of blood-stained sputum. The cough has improved lately, and there are no night-sweats. He has lost some flesh since September, but the pain in the left side of which he complains has only lasted seven weeks. It is increased by heavy exertion, and latterly it has been impossible for him to go aloft at sea. He is now short of breath when he walks, and his feet swell at night, but are better in the morning. He also suffers from shooting pains in his shoulders. He has had gonorrhœa, and also a chancre fifteen years ago. He is a moderate drinker, and smokes about one ounce of tobacco a week. The family history, so far as he knows, is negative. He lies comfortably in bed on his left side, but gets restless at times. There is no dyspnœa when at rest, his bowels are constipated, the œdema of the legs has disappeared, and his cough is less troublesome. His pulse is irregular, both in time and force, and is of the Corrigan type. The apex-beat of the heart is visible at the sixth interspace in the anterior axillary line, and the right margin of cardiac dulness may be defined, on deep percussion, outside the sternal edge. There is no thrill. Auscultation reveals the presence of a double aortic and a pre-systolic mitral murmur. The first cardiac sound is muffled and impure, and the second is accentuated in the pulmonary area. The lungs are emphysematous, and there are moist sounds over both bases. The liver and spleen are both enlarged. There is no albumen in the urine. He has a moderate degree of plus tension and his arteries are atheromatous. His symptoms have all improved since he has been confined to bed. In view of the type of murmurs, the increase in size of the heart, the pulsation in the vessels, the character of the pulse and the history of the patient, the diagnosis of aortic disease is easy. But is the mitral pre-systolic murmur, which is definitely present, to be accepted as evidence of organic change in the segments of the mitral valve, or is it an Austin Flint murmur? A favourite theory of the Austin Flint murmur regards it as being produced by a regurgitant flow of blood from the aortic orifice hitting the anterior flap of the mitral valve, but if this be the true explanation, why do we get it in some cases of double aortic disease and not in others? Is it not more likely that the mitral murmur is really due to secondary disease in the curtains of the mitral valve? In aortic cases this mitral murmur, when present, comes and goes, but this is not true of aortic cases only, because it is notorious that no cardiac murmur is so liable to variations in intensity as that of mitral stenosis. It has been noted that the pre-systolic murmur in this man's case has been subject, since he came under observation, to this well-recognised variation, but a correct history is difficult to obtain, and we cannot get away from the possibility that, in this instance, the mitral element may not be secondary at all, but may have been produced along with the aortic disease by a primary attack of rheumatic fever or of some

other variety of infective illness. The case, therefore, though clearly one of double aortic and mitral systolic disease, is, from the point of view of ætiology, capable of more than one interpretation, and I must leave it to you to come to your own conclusions. There is ample evidence of degenerative change in his tissues elsewhere, but is the cardiac condition entirely the result of similar changes in both aortic and mitral valves? Or is it due to organic changes in the aortic valve only, with a mitral murmur produced mechanically by regurgitation through the aortic orifice, and independent of any actual deformity in the mitral valve? Or is it the result of an original infective illness—rheumatic or otherwise—the history of which, especially in a foreigner, is difficult to obtain, associated with, and probably preceding, the occurrence of a progressive arterial and cardiac sclerosis?

Whatever the true explanation be of this particular combination of murmurs, they are not uncommonly met with, but, to my way of thinking, the mitral element adds to the gravity of the condition always, and the Austin Flint murmur, as it is called, always means a downward step in the progress of aortic disease. In this case we have evidence of its evil influence in the record of hypostatic congestion of the lungs, enlargement of the spleen and liver, and œdema of the ankles.

We have just time to look for a moment at one more case. This man has a distinct pleuritic rub in his right axillary region. He has not any such degree of anæmia as would account for his very pale appearance. His sputum, which is very scanty, contains no tubercle bacilli. The Von Pirquet reaction has been tried, and is negative. There are no physical signs otherwise, but a dry rub is in many cases due to tuberculosis. The history is suggestive of chill as the cause of the pleurisy, and our investigations justify the belief that, at any rate up to the present time, he is free from tubercle. To those who regard pleurisy as unconditionally tuberculous, physical signs such as we have here would warrant treatment by tuberculin injections, but without more positive evidence of tuberculous infection I, personally, do not think such a course commendable. We will have his opsonic index estimated, and, should this prove abnormal, we will have him injected diagnostically with a suitable dose of old tuberculin. The result of these further investigations will be related to you at a future meeting, and according to them we will be guided in the course of treatment to be adopted. I hope some of you may share my view that it is going too far to claim a tuberculous foundation for every pleurisy. I am convinced that many are the result of an ordinary chill, and we all have experience of cases apparently attributable to such a cause occurring in strong, healthy patients, which recover both satisfactorily and permanently. Moreover, treatment by tuberculin is still on its trial, and should only be undertaken, in my opinion, in cases where the diagnosis is assured, and where Nature seems unable to bring about a cure by her unaided efforts.

March 6th, 1911.

Diet in Typhoid Fever.—C. E. Nammack states that it should be fluid, it should be easily digested, it should be readily absorbed. For the average patient these requirements must be sought for in foods which are simple, efficient, and cheap. No single article of diet meets the requirements so well as does milk. A close second is found in eggs, when they are fresh and not released from cold storage plants. When milk disagrees, when there is abdominal distention, and when curds appear in the stools, some modification in the administration becomes necessary. In fact, pure milk is never ordered, the author's favourite formulæ being milk, four ounces; fresh cream, one ounce; lime-water, one ounce, given every three hours, and followed by a mixture of dilute hydrochloric acid, ten minims in one drachm of a reliable essence of pepsin. To this prescription strychnine can be added when required. Between each two portions of milk, albumen water, made by adding the white of two eggs to six ounces of boiled water, is given. The patient is also solicited to drink plain water freely. If there is complete intolerance or aversion to milk, then buttermilk or kumyss or kefir can frequently be borne. After the third week the author usually allows crackers with butter, well-cooked rice, calves' foot jelly, and broth, and other solid foods when the evening temperature reaches normal.—*Med. Record*, vol. lxxix, No. 7.

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A CLINICAL DEMONSTRATION* OF A CASE OF SPASMODIC TORTICOLLIS AND A CASE OF CEREBELLAR TUMOUR.

By PURVES STEWART, M.A., M.D., F.R.C.P.,

Physician to the Westminster Hospital, to the West-End Hospital for Nervous Diseases, and to the Royal National Orthopædic Hospital.

LADIES AND GENTLEMEN,—The first case I have to show you this afternoon is that of an unmarried woman, æt. 46 years, who complains of pain and involuntary movements in her neck. You will see for yourselves the nature of the movement. Notice, first of all, that the movement is to the left: you will hardly ever see a case of this sort in which the movement is to the right. The history, so far as we can make it out, is as follows: Family history unimportant; fifteen months ago this patient began to have severe pain in the left side of the face; this passed off, and we do not know whether it had anything to do with her present condition or not. But thirteen months ago her head began to turn to the left side, and there was much difficulty in keeping it still. She also noticed that there were cramps on the right side of the neck. The movements in the head became worse and the spasms stronger until she was no longer able to control her neck. During the last four months she has become much worse, and now she has to rest her head on supports, even when sitting on a chair, to keep it still. In other respects she is healthy. The periods are still regular, so that she has not yet passed the climacteric. On looking at her, you notice that her head is strongly rotated to the left. It is mainly a tonic spasm. But she has also some rhythmic tremor superadded. The tonic spasm is so intense that even when she pushes her head strongly round with her left hand she cannot completely overcome it. When she makes an effort to

* At the Polyclinic.

move the head actively to the right side we can feel the opposing muscles contract, but they are not strong enough to overcome the action of the spasmodic muscles. The main muscle concerned is the right sterno-mastoid. But on placing the hand on the back of the neck you can feel that the muscles on both sides are contracting synchronously with the sterno-mastoid. In fact, although the spasm is at first sight unilateral, it is really bilateral. The deep muscles of the neck on both sides are affected. On the left side the spasm is rather stronger than on the right side. She has always had headache, but she has not had it now for some months.

The diagnosis presents no difficulty. It is a case of spasmodic torticollis. There are several varieties which have to be recognised. First, there is neuralgic torticollis, that is, where the patient has occipital pain, or pain in a tooth, or in some part of the head, and during the time of the pain there is twisting of the head to one side. That is more a reflex spasm, not a true major torticollis. Secondly, there is a rare variety called labyrinthine torticollis, in which the patient keeps the head persistently dropped over to one side so as to compensate for the vertigo which he feels when the head is straight. Variations in the position of the head in semi-circular canal disease cause intense vertigo, and certain patients learn to overcome that vertigo by putting their head on one side so as to avoid sudden stresses in that semi-circular canal. Those cases are accompanied by other evidences of labyrinthine disease, either deafness and other symptoms of cochlear disease, or vertigo, and sometimes nystagmus, and there are also Bárány's tests by means of cold and hot water, as well as the test by the revolving stool. This patient has no symptoms of this sort. There is also professional torticollis. The tailor or cobbler when sitting sewing at his work follows his needle with his head, and in course of time some of these cobblers and tailors develop torticollis. But those cases of professional torticollis are not the same as that which you see before you; they are more of the nature of occupation neuroses.

In left-handed people this idiopathic torticollis is to the reverse side, *i.e.* to the right. This kind of torticollis most often comes on at or after middle life, and a large number of cases come on at about the time of the menopause. As this patient is 46 years of age she is probably approaching the menopause. Her torticollis has persisted for the last eighteen months. But the menopause does not produce the torticollis; there must be some other factor, and the other factor here is the patient's underlying psychasthenic constitution. These patients have other stigmata of neurosis. Most people with tic are rather above the average of intelligence, but we cannot say this so frequently of torticollis. At first, when the spasm is slight the patient can control it by some compensatory action; generally a mere touch of the finger on the chin will hold it steady. But afterwards it becomes so severe that the patient cannot do that.

What can we do for these patients? If this patient had been thirty years younger we should probably have re-educated her, and might have cured her with comparative ease. Tics in young people can, with care and thoroughness, generally be overcome by exercises, by drill and encouragement, and by psychical suggestion. But the prospect is not the same when we come to the degenerative period of life, for in such cases suggestion does not cure so readily. So we are obliged in their case to resort to an extremely unscientific method of treatment, which apparently cures the condition. This condition, of course, is cortical in origin; it is due to hyperexcitability of certain centres in the cortex, and the ideal thing would be to get at those centres directly. In young people we can get at them by suggestion and re-education. In these other patients, as a second best measure, we recommend division of the motor nerves which supply the spasmodic muscles. We divide the spinal accessory, which supplies the sterno-mastoid, and the deep cervical nerves which supply the deep muscles at the back of the neck. The result is that the muscles in which there is spasm become temporarily paralysed. The brain will persist in trying to move the head as before, but there will no longer be nerve paths to conduct the impulses down to the muscles. That is not cure; it is merely replacing spasm by paralysis. But if we thoroughly recognise this and explain it to the patient, we are justified in doing it, because the paralysis is much less inconvenient than the spasm. This patient's discomfort from the condition is so great that she is going to have the paralysis instead, and I am asking my colleague, Mr. Wilfred Trotter, to divide the spinal accessory on the right side and the deep cervical nerves on the other side.

This next patient is a very well-marked instance of organic disease. He is thirty-six years of age, a grocer, and a very intelligent man. Seven years ago he was taken with violent pain in the back of his head, in the middle line. It came on early one morning, and in a few hours it had gone. That was on a Monday morning. On the Tuesday night he lost hearing in both ears, and this lasted four days, and then cleared up. He has had hissing tinnitus in the right ear ever since. He went on very well until three years ago. Three years ago he was in the garden one very hot afternoon, when he was seized with pain in the back of the head, sickness, and violent giddiness. That lasted five weeks. The giddiness was very bad at times. Then he seemed to resume his normal condition except for the noises in his right ear. Eighteen months ago his wife noticed that when he laughed his nose was twisted round to the right side, and later she noticed that his left eye watered. Later still he had some difficulty in swallowing. Five months ago he suddenly became totally deaf again in the left ear. This deafness lasted for a week, but it has gradually become somewhat better. Within the last six weeks he has developed vomiting, independently of his food, and during the same period he has again become giddy and unsteady in his walk. That is the entire history that we have. There is no history of venereal disease, or of otorrhœa, or of head injury. We have looked at his optic discs, and they are normal. Rough test of the visual fields shows them to be normal. The pupils are equal and normal. On his looking to the left there is coarse nystagmus, also less markedly when he looks to the right, and in the latter case it is also slower. The external ocular movements are otherwise normal. The masseters are normal, and the jaws open straight. His motor fifth nerve is therefore all right. There is no anæsthesia of his face. He does not hear a watch on contact with either ear. He hears the talking voice better with the right ear. The tuning-fork at the vertex is not heard better in one ear than in the other.

There is complete facial palsy on the left side. He shows one nice point with regard to facial palsy, viz. that if you make him look down slowly and ask him to close his eyes, the good lid goes down and the bad lid goes up. The reason is that when we perform voluntary movement we do so not only with our prime-movers or protagonists, but we control the movement with the antagonists. Normally when we close the eye we also contract the antagonist, the levator palpebræ, so as to steady the orbicularis. This patient, having lost power in the orbicularis, only contracts the antagonist muscle and the lid therefore goes up.

His palate, instead of going straight up in the middle line, is pulled up and dimpled to the right, *i.e.* the left palate is paralysed. The tongue is normal. The sterno-mastoids and the trapezii are normal. He has no cutaneous anæsthesia or analgesia. When he walks he lurches somewhat. We will test the motor and sensory power in his limbs. His left heel-knee movement is not so steady as with the right heel. He cannot stand as well on the left leg alone as he can on the right. There is no motor weakness, but there is evidently some unsteadiness of the left leg. The deep reflexes of the upper limbs are equal; the reflexes of the lower limbs are brisk and equal. Ankle-jerks normal and equal. There is no sign of an extensor plantar response. His urine is free from sugar and albumen.

Now, on these facts, the first question is, Where is the lesion? And we must first try to see if we can suggest some lesion which will account for all his symptoms. We must have something implicating the left seventh nerve, because there is complete facial palsy, and that lesion must extend down the left side of the medulla as far as the upper roots of his vagus, because there is paralysis of the left palate. He has slight deafness on the left side, which would fit in very well. The ninth cranial is the glosso-pharyngeal, of which we know very little as regards the clinical signs of its paralysis. So there is implication of cranial nerves on the left side from the lower part of the pons downwards. He must have at any rate one lesion there. Could that lesion account for his other symptoms? Yes. There is inco-ordination of his left leg, a reeling gait, and nystagmus, which is more marked to the left. These suggest implication of the left lateral lobe of the cerebellum. We know that each cerebellar hemisphere is connected with the homolateral limbs. A lesion there, starting or most intense in his seventh, eighth, and tenth nerves on the left side and pressing upwards against the cerebellum on the left side, will account for most of his symptoms except the deafness on the opposite side. (Some of you may think I am disregarding the opposite deafness, but, as a matter of fact, that corroborates our opinion, as we shall see presently.) His main lesion is obviously somewhere at the base of the brain, on the left side, at the lower edge of the pons,

spreading downwards along the left medulla and pressing upwards against the left lateral lobe of the cerebellum. A left-sided lesion at the base will account for everything except the fact that he is deaf on the right side. What is this lesion? The prolonged history of the case, which goes back for at least seven years, shows we cannot have to do with a vascular lesion; it is not anything like a hæmorrhage, or thrombosis, or an embolus; not even an abscess would date back seven years. What is it most likely to be? It must be either some degenerative condition or something in the way of a neoplasm. We have really only these two alternatives to choose from. Some might say that the absence of optic neuritis is against that, but it is not so. The presence of optic neuritis would strongly corroborate our view, but the absence of optic neuritis does not justify the exclusion of cerebral tumour. Many symptoms are valuable when present but their absence is not of much clinical importance. He has headache and vomiting, without any apparent cause. You must never diagnose a neoplasm until you have excluded several other things. The urine is free from sugar and albumen. We carefully examined the urine because renal disease can produce headache and vomiting, and also optic neuritis. But this is not a case of renal disease. Severe anæmia can also cause headache and optic neuritis, and so can certain cases of lead-poisoning, but he has neither anæmia nor plumbism. So we diagnose an intra-cranial, extra-cerebral lesion, one which is on the surface of the brain and in the ponto-cerebellar angle. We see many cases like this, and as a matter of experience we find that the commonest nerve in the skull to develop a neoplasm is the auditory nerve. A large proportion of auditory-nerve tumours are bilateral—a large tumour on one side, and a smaller one on the other side. And the fact that this patient has deafness on both sides, so far from antagonising our diagnosis, goes a long way to corroborate it, because it probably means he has a slighter condition in the right auditory nerve. The diagnosis in this case is a tumour in the left ponto-cerebellar angle, and probably another in the right. The one causing the most symptoms is that on the left side. For such a tumour there is only one kind of treatment, and that is surgical removal, and I am going to ask my colleague, Mr. Ballance, who has not yet seen the case, to open this man's left cerebellar fossa in a few days and to remove this tumour. To a skilful operator the procedure presents nothing very formidable, but the chief trouble is that of profuse hæmorrhage. The posterior inferior cerebellar artery runs very close to the tumour, and sometimes it is difficult to control the hæmorrhage while the tumour is being removed.

March 13th, 1911.

A CLINICAL LECTURE
ON
FACIAL TETANUS.

Delivered at St. George's Hospital,
By W. F. FEDDEN, M.S., M.B., F.R.C.S.,
Assistant Surgeon to the Hospital.

GENTLEMEN,—To-day I want to speak of two cases of facial tetanus which I have come across in the last few months. One was a very mild case, and the other an exceedingly severe one; both of them recovered.

The first case was at the Weybridge Cottage Hospital, under Dr. Crofton-Atkins, an old St. George's man. The patient was a boy, æt. 14, who was run over by a motor-car and had all the soft parts of the dorsum of the left foot torn away. There were also three minute irregular scalp-wounds in the left temporal region above the zygoma; there was no scratch on the right side of the head or face. Probably these small scalp-wounds were not very carefully cleaned up because the whole attention was directed to the severe injury to the foot. I decided against amputation, and the foot ultimately recovered in an exceedingly rapid manner. On May 24th, which was a matter of seventeen days after the accident, the boy was wheeled into the garden, apparently perfectly well as regards his general health. On May 25th he had ptosis of the left eyelid, that is to say, on the same side as the scalp-wounds which I mentioned. There was slight dilatation of the left pupil. The following day the ptosis of the left eyelid was much more marked, and there was some paresis of the left masseter muscle. Some slight stiffness about the jaw appeared, and he was unable to open his mouth to the full extent. On May 27th there was marked trismus. The face in repose looked as if it were the subject of Bell's paralysis. He was given a dose of anti-tetanic serum. On May 28th the teeth were clenched and could not be separated, and it was found very difficult to feed him. His temperature was 100° F., he was very restless, and was said to start and twitch constantly during sleep. A second dose of anti-tetanic serum was given. On the 30th, which was the twenty-third day from the date of the accident, the temperature was normal, he was distinctly better, the mouth

could be opened sufficiently to admit a small pencil, and the patient's facial paralysis was less remarkable. After that he rapidly improved, and on June 3rd there is a note to say that all his symptoms of tetanus had disappeared. That is nine days after their onset, and they did not appear until the seventeenth day from the accident. In all probability that tetanus was due, as I shall point out later, to his scalp wounds, which probably harboured the infecting agent. The extensive and severe wound of the foot, which was at once properly cleaned, probably had nothing whatever to do with the occurrence of the tetanus, because it was immediately treated with antiseptics. This was a very mild case of tetanus coming on late and resolving very early after three doses of anti-tetanic serum.

Now I want to speak about a case in this hospital which was very different in its course indeed. A man who had to deal with horses was kicked on the forehead on September 23rd by a horse, and the result was a ragged wound which was very dirty. It was cleaned up at once, and the patient progressed satisfactorily for the time being. The ragged wound was on the right side of the forehead, and extended down into the right orbit, exposing the frontal bone. On the eighth day from the accident he complained of stiffness on the right side of the face, that is to say on the same side of the face as the wound. This became worse until the right side of the face was completely immobile and the right side of the tongue was noticed to be paralysed. Two days later there was difficulty in swallowing, which he said was mainly due to difficulty in getting the food to the back of the mouth. In other respects he was apparently healthy. There was slight ptosis of both eyelids, frequent spasm of the facial muscles on the left side of the face, drawing the mouth to the left. The muscles on the right side of the face were in a state of fixed spasm. The muscles on the left side of the face had sufficient tone to produce a definite but slight sardonic expression. Both sterno-mastoids were tense; the platysma was not contracted. On the right side of the forehead there was a large scar suggesting that a flap of scalp had been turned back, and a sinus led from this to the frontal bone, which was bare. Most of the wound had healed except the lower part where it crossed the eyebrow. There was some stiffness of muscles of the back. The knee-jerks were brisk, and on the whole the man was not in a severe condition when admitted. But later in the day definite spasms of the masseters appeared and the jaws could be separated only by three quarters of an inch so that he was unable to protrude his tongue. On the following day there was increased stiffness; he could not separate the jaws more than a quarter of an inch. He complained that during the night his teeth were clenched and he bit his tongue and lip. At midnight he had increasing spasm of the jaws; he complained of his neck being stiff, which he could not bend forwards. The spasm of sterno-mastoids seemed to be increasing and was elicited on very slight stimulation. He was given 20 c.c. of anti-tetanic serum. At that time he was taking his food well. On the following day, October 19th, he was transferred to a separation ward. On account of the increased difficulty in swallowing and increasing spasm of the muscles about the head and neck he was again given 20 c.c. of anti-tetanic serum. In the evening he had very intense facial spasms, but so far no general spasms. The history after that is very prolonged and I do not think I shall have time to read the whole of the notes. For three weeks he was in a very serious condition. From this slight facial spasm he passed on to general tetanic spasms of the trunk and abdominal muscles, the muscles of the back and neck. He did not, however, get any real spasms of the extremities. He was treated with anti-tetanic serum throughout the whole course of his spasmodic attacks, having as a preliminary dose 20 c.c. of the serum, and for the next nine days 20 c.c. daily; it was then omitted for one day and immediately his spasms became worse; the injections were therefore continued daily. On two subsequent occasions the injection of anti-tetanic serum was omitted, and on each occasion the omission was followed by an increase of the spasmodic attacks. This is a point one would like to call special attention to: whenever we ceased to give the serum there was an increase in the symptoms. The other point I want to specially draw your attention to is the tremendous difficulty this man had in swallowing food. The spasms seem to involve the pharyngeal and laryngeal muscles, and at times he almost choked when he attempted to swallow food. Up to the time when we got him into the separation ward he took food, but when

the spasms increased in severity he was scarcely able to swallow any food without inducing severe pharyngeal spasm. The great difficulty in a case of tetanus is to feed the patient, and if you cannot feed him he dies. This man was fed mainly by giving fluids through a tube while he was under mild chloroform anæsthesia. On eight occasions he was anæsthetised, and while under chloroform food was passed by means of the nasal tube. Except for this I think he must have died. The two points to emphasise about the case are: first, that an enormous quantity of anti-tetanic serum was injected, and that he had to be fed whilst under the influence of chloroform for the best part of a week before he was able to take any food spontaneously.

You will notice it is a case in which there was facial paralysis and a wound about the head. The first case I mentioned also had facial paralysis and a wound about the head. If you look at Allbutt and Rolleston's 'System of Medicine,' vol. i, p. 167, there is an account of four of these cases of so-called cephalic tetanus collected by the late Mr. Humphrey, of Cambridge, and all four of them correspond closely with these which I have mentioned to you. Those four cases are recorded in detail, and are followed by a short summary, which I will read to you. "It will be observed that in all these four cases, in addition to the wound of the face or head, with paralysis and clenching of the jaws and tension of the abdominal muscles, there was spasm or paralysis of the glottis, which constituted the most urgent symptom. This was fatal in the fourth case, and probably also in the first case, and required tracheotomy in the third case. In No. 2 a pipe of tobacco seemed to give relief. The paralysis of the glottis permitted fluids to pass from the mouth into the respiratory passages, and this was a very troublesome feature in the third case. In the fourth the pupil was dilated." [In my first case you will remember the pupil was dilated.] "The back muscles were affected, but the abdomen was soft. In all four the limbs were free. The facial paralysis is difficult to explain. It has been attributed to swelling or some inflammatory or other influence radiating from the wound and involving the facial nerve, to inflammation of the nerve in its course through the temporal bone, or on the cranial side of that bone, but no satisfactory conclusion has been arrived at. In No. 4 the paralysis extended to the iris. There was no loss of consciousness or of sensation in any of the parts affected in any of these cases." You will see those four cases are essentially similar to the two I have mentioned; many of the points come out repeatedly in these separate cases.

As to this facial paralysis, in the case which we observed closely here, I concluded we were not dealing with true facial paralysis as is ordinarily understood; that we were not dealing with flaccid paralysis of the muscles supplied by the facial nerve, but rather with constant tonic spasm of those muscles; that we were dealing with a spasticity rather than a true paralysis of those muscles. This is very interesting when we consider the experimental tetanus produced in animals, because in animals it is not infrequent to find that the tetanic spasms at first involve the part in which the injection or inoculation is made. The explanation is said to be that the tetano-toxin travels up the nerve-sheaths in the perineural lymphatics or perineural veins, and so ultimately arrives at the spinal cord or at the brain in the region of the nerve-cells which supply the part where the injection is made. I should be inclined to say, from a study of this one case alone, that we are not dealing with a true facial paralysis, but rather with excess of spasm in those parts which first received the infection. When speaking of the boy who had his foot severely injured, together with two or three minute scalp-wounds, I mentioned that he had no tetanic spasms of the extremities at all; he only had a slight affection of the trunk, but the main effect was in the neck and jaw and the side of his face on which the scalp-wounds occurred. Here I think the tetanic poison must have travelled along the filaments of the cranial nerves and so affected the anterior horn-cells or what corresponds to them in the medulla, with the result that the cranial nerve-fibres were those first affected in such an instance, and that the affection was a spasmodic one leading to a contracture of the muscles rather than a true paralysis.

Now I want to say a few words on tetanus in general. It is a virulent infective disease characterised essentially by contraction of the muscles together with spasm, and generally occurring in certain groups of muscles, but ultimately extending to the whole body, the limbs included. It also affects muscles of the thorax, and so tends to fix the chest and stop respiratory action, proving fatal

in this manner in many instances. Tetanus is due to intoxication by the products of the specific bacillus on the nerve-centres. It is most commonly found in cases where wounds have been exposed to contamination with earth, dirt, and horse-droppings. It is therefore far more common in those wounds which are severely lacerated. In ordinary civil practice, in recent years, it has been exceedingly rare for us to ever see cases of tetanus, and happily so, because I know of no more dreadful death to witness than the death of a healthy man from this disease: it is one of the most repulsive sights which you can see in the whole of your medical work. I am very much afraid, however, that tetanus is on the increase again in large cities. It seems to me that in the last twelve months we have had more tetanus in this hospital than in the preceding five or six years. I think that is possibly due to the very severe lacerated wounds which we get in people who have had accidents caused by motor vehicles, wounds which are ground with dirt and horse-droppings from the streets. Wound tetanus was much more common in campaigns and sieges in the pre-antiseptic days than since the introduction of antiseptics. An antiseptic rapidly applied to a wound seems almost to preclude the possibility of tetanus in modern surgery. In days gone by tetanus occasionally followed what should have been an aseptic operation. Tetanus was not by any means unknown following such operations as ovariotomy, ligature of piles, etc. Fortunately, nowadays, this is but rarely seen, and is probably due to the more frequent use of antiseptics. Tetanus is more common in warm climates than in the temperate zones in which we live.

The toxin of tetanus affects the central nervous system, and does not affect the peripheral nerves nor the muscles themselves; this has been proved conclusively by a series of experiments. For example, if tetanus is introduced into the limb of an animal and the peripheral nerves are all cut, you will find that the spasms affect the whole of the remainder of the body except the part supplied by the nerves which have been cut; this shows that the poison does not act directly on the muscles. Conversely, if you take an animal and curarise it, with the object of paralysing the nerve-endings in muscle, tetanic spasms do not occur in those muscles, although the animal may die of the infection. Local tetanic spasms in experimental tetanus are first seen in the region where the poison is introduced; this, however, is not so common in general tetanus in man. I do not know if there is any explanation of this, but in those cases of facial tetanus which I have had under consideration, I regard the facial condition as being analogous to the experimental condition of the tetanic spasms first occurring in the region in which the infection is introduced.

Regarding the use of antitoxin in tetanus; tetanus and diphtheria are often compared pathologically, and they are essentially similar conditions in that both of them have an organism growing locally, which organism never gets into the blood-stream, and never causes a general infection. Both diphtheria and tetanus are produced by the growth of virulent organisms locally, the poisons of which are absorbed into the general system, and act on the central nervous system and produce spasms or paralysis. Why is it, then, that we have had such exceedingly good results in the past from the use of anti-diphtheritic serum but not equally good results from anti-tetanic serum? I think the explanation is that in diphtheria there is a lesion which is recognised early by the taking of throat-swabs and by making a bacteriological diagnosis. By this means it is generally recognised in the first few days. In the case of tetanus, on the other hand, you have a lesion which is generally a suppurating wound. At first you think it is nothing more than an ugly wound which is not healing nicely, and it is not until the nervous system is actually affected that you recognise you are dealing with a case of tetanus. There is an old saying of Mr. Humphrey's which is very true indeed: that when a man with tetanus begins to have spasms he is not beginning to have tetanus—he is beginning to die of tetanus. And I think the reason why the use of anti-tetanic serum has not been so universally proclaimed as is that of anti-diphtheritic serum, is that in the latter the poison has got well into the central nervous system by the time the disease is recognised as tetanus, whereas in the case of diphtheria you are usually able to recognise the condition of affairs in the first or second day. When the spasms resulting from the poison in the nervous system commence they are evidence of the commencement of the end, and until an infected wound is cleaned out the manufacture of toxins goes on rapidly.

Now, as regards the symptoms of tetanus. The essential symptom is a spasm, and this spasm is followed with persistent tension of the affected muscles; the tension of the muscles persists after the spasm. As we have already said, the first spasm may be in the neighbourhood of the wound; more commonly it is in the neck, the platysma, sterno-mastoids, masseters or the face muscles. When there is spasm of the face muscles the peculiar "risus sardonicus" is produced. This is not easy to forget when once it has been seen in a bad case. In an advanced case of tetanus you always get trismus present, that is to say closure of jaws from the masseteric spasm; the masseter muscles stand out, and you can feel their anterior borders beneath the skin. The spasms tend to rapidly spread to the abdominal muscles and sometimes to the erectores spinæ, and then to the limbs, and they may be so severe as to cause rupture of a muscle during the spasm. Rupture of the rectus abdominis is sometimes seen, and rupture of the psoas muscle has been reported. With the spasms there is fairly severe pain, the patient often lies groaning, and yet you can do practically nothing to relieve the pain. In bad cases of spasm of the back opisthotonos may be seen, that is to say, the individual rests on his occiput and his heels, the back being arched. The spasm about the mouth is often a source of great distress because of the inability to swallow not only food but even saliva, and in the case of a man under consideration the dribbling of the saliva from the corner of the mouth was a source of extraordinary trouble.

The earlier the onset of tetanic spasms in a case of tetanus the more likely is there to be a fatal result, and cases developing symptoms within the first four or five days after infection had a fatality of 90 per cent. before the days of antitoxic injection. The later the spasms come on, that is to say the more chronic the affection is, the better chance there is of the patient's recovery. But even now one cannot say that there is a chance of the recovery of more than 60 per cent. with the use of anti-tetanic serum and modern hospital treatment.

As regards the intellect of the individual, it is nearly always quite clear throughout the attack; clear up to the time of death. He knows death is coming on by the extraordinary spasm of the respiratory muscles, the fixation of the jaws, and so on. Delirium occasionally ensues, but it is rare. The temperature is raised and the pulse-rate is increased during the spasms, but rarely do you see a condition of hyperpyrexia. Frequently there is rapid wasting and marked exhaustion, which in many cases is due to inability to take food; but I think it is not so altogether, because in those cases where the patient is fed regularly, rapid wasting may still be seen. This is probably one of the results of the action of the tetanus poison. In the spasms the involuntary muscles do not participate; the heart is not affected by them, nor are the movements of the stomach and intestines.

Speaking of the diagnosis of tetanus, a well-formed characteristic case cannot be mistaken for anything else, but there are milder and more chronic cases which might be mistaken for other conditions. For example, the second case I have spoken to you about might, in the absence of a history, have been mistaken for hydrophobia owing to the extraordinary difficulty that the man had in swallowing fluids. Spasm was set up by any attempt to take fluids. In hydrophobia you have usually the history of the mad dog, and a great point of difference between the two conditions is that in tetanus the contraction persists between the spasms, whereas in hydrophobia it is said to disappear. Beyond that I cannot give you much information as to the distinction, because in modern times we practically never see a case of hydrophobia.

Another possible condition to mistake it for is inflammation and abscess about the masseter muscle; this might, perhaps, be diagnosed as tetanus in the chronic form. If there is an unerupted molar or periostitis about the angle of the jaw, with resulting trismus, it may be mistaken for tetanic spasm. So in doubtful cases be careful to inspect the region of the ramus of the jaw, and think of the possibility of a local cause for the masseteric spasm, which may closely resemble the condition seen in tetanus. Another thing which has been sometimes mistaken for tetanus is basic meningitis; this is not very likely to cause confusion, though in tetanus head-retraction to a marked degree may occur, but the retraction of the head in meningitis is unassociated with spasms of the neck and of the face and jaw muscles. Again, in strychnine poisoning there is a condition which is essentially similar in many

instances to poisoning by tetanus toxin. Here, however, the onset is generally much more sudden, and the sequence of events more rapid, and strychnine poisoning involves the whole body simultaneously. Another point is that the tetanus poison rarely causes contractions of the hands and feet, whereas in strychnine poisoning the digits are affected. Fatal poisoning by strychnine is much more rapid than is death from tetanus, and if the case does not prove fatal the recovery from the effects of strychnine is much more rapid. Lastly, there is the condition of tetany frequently seen in badly nourished children who often are the subjects of rickets, and in them there are spasms, especially of the hands and feet, and but rarely of the other parts of the body. This is probably due to the absorption of poisons from the alimentary canal where imperfect digestive processes are taking place. I may also mention the convulsions of hysteria, which are not likely to be mistaken for tetanus by anybody who has ever seen them.

Now a few words with regard to the treatment of tetanus in general. It is a disease which runs a definite course, just as other toxic conditions do. There are certain things to bear in mind. It has an incubation period before tetanic symptoms appear, and then there is a period of increasing violence of the spasms, and finally there is a period of decline. As we have said, the spasms are first seen in the face or neck or jaws, or at the point of the first entrance of the poison. In a few instances the condition may go no further than this. Consider the case of the boy who had a few spasms of the face, which spread to the body, the condition subsiding in nine days after the onset of symptoms. You see it was a very mild case and came on very late. The disease may be so slight that there is only trismus for a few days, and nothing else happens. I think it is right to give anti-tetanic serum in any case to prevent the condition going further. In the case seen at this hospital, and which I have narrated to you, the man when I first saw him was very much worse than the boy; he developed fairly well generalised tetanus, but his extremities were not affected. For ten days he was very near death. The boy I have spoken of practically recovered spontaneously, and I think it is probable he would have recovered even without any serum being given. Now that we have this definite antidote to the tetanic poison, namely, anti-tetanic serum, I think it should be given in any case where we suspect there is tetanus. It is also advisable that an injection should be given as a prophylactic measure in cases of contused wounds in which there is much road or stable dirt. In hospital practice we have to remember that the serum is expensive, but if any tetanic symptoms appear there is no question that it is one's duty at once to give anti-tetanic serum, and not only once, but to continue it daily so long as the spasms persist. I have told you how, in one of the cases, as soon as we ceased giving the anti-tetanic serum the symptoms increased immediately, to be relieved by further injections. Another important point is to increase the strength of the individual and keep up his resisting power, so that he can hold on during the course of the malady. You must feed these people, and go on feeding them; if you do not they will die. This is not as simple as it seems, for you have to feed them under great difficulties. When there is pharyngeal or laryngeal spasm it is difficult to get food into them at all. I attribute the success in the severe case which we had in the hospital to the fact that my house-surgeon, Mr. C. J. Blaikie, was up night after night giving him chloroform so as to enable him to swallow, and I think that if he had not been fed in that way he would undoubtedly have died. Strong beef-tea and brandy and other food was given whilst lightly under the influence of chloroform through a tube passed into the pharynx. The patient also received rectal feeding and saline by the rectum, because that was the only way in which we could get sufficient fluid into his system. At all costs you must feed these people by some means. It has been suggested that gastrostomy should be performed so as to feed them, but this is a course I should hardly like to adopt. Drugs seem to have no influence on the disease, unless it be that chloral and the bromides, by keeping the individual quiet, diminish the reflex excitability of the nervous system. In the case of my patient, after he had been fed by a tube passed into the pharynx he was given a 25-gr. dose of chloralamide, which gave him a moderately quiet night. The mainstay in the treatment of these cases is feeding, together with the continuous use of antitoxin. In some instances

trephining has been carried out, and the antitoxin has been injected directly into the cerebro-spinal fluid; in other instances it has been injected directly into the anterior horn of the lateral ventricle of the brain. As far as I know the results of these procedures have been anything but satisfactory, and the results I have seen from the subcutaneous injection of the serum have been far better than the results obtained from intra-cerebral or intra-cranial injection.

The disease requires to be attacked at the earliest possible moment, but it is one which you should not despair of so long as you can feed your patient and keep his strength up. I think that in future, when we are dealing with motor-car injuries, it would be as well to give prophylactic injections of anti-tetanic serum—certainly in cases of severe contused wounds. There is another point which I think you should remember: a patient suffering from tetanus should not be subjected to any sudden auditory tactile or visual stimulus, such as the switching on of an electric light or any loud noise. All variety of stimuli tend to increase the severity of the spasms. It has been advised that these patients should be kept constantly under the influence of chloroform; although this may sound correct theoretically, I do not think it would turn out at all well in practice, because you must remember that chloroform is an essential poison to the cells of the central nervous system, and such prolonged chloroform anæsthesia would probably in itself prove fatal. In the stage of subsidence the person gets better gradually after the severe symptoms have ceased; stiffness may remain for days or weeks. For some time afterwards the stiffness becomes worse in wet weather, in the same way as chronic articular rheumatism, and this may persist for many months; occasionally the spasms may return. In such a case one has to think of the possibility of any remaining infection.

[*Note.*—On December 30th, 1910, the second case came to see me with a slight recurrence of symptoms, after having remained perfectly well in the interval. The wound was firmly healed and appeared quite healthy, but for a few days he had noticed a spasm of the right orbicularis palpebrarum together with slight stiffness of the erectores spinæ. These symptoms reacted to four injections of antitoxic serum. The orbicular spasm was such as to produce the appearance of ptosis; any attempt to open the right eye was followed by an increased spasm of the muscle. One must here suppose a further formation of tetano-toxin in the wound in the absence of any suppurative process there.]

March 13th, 1911.

ON THE

TREATMENT OF CANCER OF THE RECTUM.

A DISCUSSION* BY

Mr. Swinford Edwards. Mr. Godlee.
Sir Frederick Eve. Mr. F. C. Wallis.
Mr. Waterhouse. Mr. Lockhart Mummery.
Mr. Boyce Barrow. Mr. Fagge.
Mr. Sampson Handley. Mr. Gordon Watson.

Mr. Swinford Edwards said: I cannot but consider it a very great honour that I should have been asked to open from the practical side what is sure to be a very interesting and important debate on the treatment of cancer of the rectum, a disease which, from all available statistics, appears to be on the increase.

Had I to treat the subject from a historical and statistical point of view I know I should fall short of reaching even the lowest ideal to which I might wish to attain. But for an extensive review of the subject time does not permit, and I feel that the wishes of the Society are more likely to be met if I briefly state my own views on the present position of the operative measures necessary for the removal of a rectal cancer and my experiences of the various methods I have employed.

I may at once say that the chief object of my remarks is to enter a protest against what I believe to be a growing tendency, viz. the performance of an unnecessarily severe operation, attended with a heavy mortality, for cases of cancer which can be cured by older methods, attended with but little risk to life, and which, moreover, entail no crippling of the patient.

All forms of treatment other than operative whether by radium, X rays, high-frequency currents, trypsin, and other digestive ferments, have, I think it is generally admitted, proved unavailing, so we can confine our attention to the best means of effecting a cure by excision.

The first successful extirpation of the rectum was, I believe, performed by Lisfranc in the year 1826. From this time to the year 1885 perinæal excision for low-lying growths, as advocated and carried out by the Allinghams and Harrison Cripps in this country, was the only method in vogue; even to-day it has its proper place in operative

* At the Royal Society of Medicine.

surgery, which is for the extirpation of anal growths.

The work of Kraske, of Freiburg, in Breisgau, given to the world in 1885, marked a great advance, for by his trans-sacral method, cancers which before, owing to their high-lying position, were considered inoperable, could be successfully removed and the function of the part, in many cases, restored by an end-to-end anastomosis.

The sphere of Kraske's operation was enlarged by the modifications of such men as Badenheuer, Hochenegg, and Rosé, whilst the names of Billroth, Rahn, etc., are known in connection with the formation and preservation of bone-flaps. About this period the vaginal route found favour with some surgeons, and is comparable to the perinæal operation as amplified and carried out by Quénu.

The next onward step was undertaken with the object of removing cancers situated in the upper rectum and lower sigmoid colon, which did not permit of removal or could only be excised with difficulty and incompleteness by Kraske's route. It consisted in freeing the bowel through an abdominal incision, and then excising it through the perinæal or sacral route, with various modifications according to the exigencies of the case. This is called the abdomino-sacral, abdomino-perinæal, or combined operation. It is an operation of considerable magnitude, and is attended with a mortality of about 20 per cent. In the ideal operation an artificial anus is necessary, the gut being brought down through the sphincters and stitched to the anal margin. It should be restricted to cases where the disease has not passed beyond the walls of the bowel.

Nowadays the position of the growth, *i. e.* its distance from the anus, is no bar to extirpation. The only real bar to a successful operation is the fact that the growth is no longer confined to the bowel, but has become adherent to neighbouring parts, whilst the lymphatics are infected. Another condition which quite bars any attempt at removal is metastasis in the liver, often not discovered until the abdomen has been opened, either for exploration of the growth or for the purpose of forming an artificial anus.

In considering the best method to employ for removing the disease due regard must be paid to the immediate mortality of any given operation, the risk of recurrence, and the amount of crippling, if any, likely to follow the operation. In a paper read by Mr. Ernest Miles before the British Medical Association last year, and published in the Journal of October 1st, the author advocates a very extensive proceeding, which he calls the "radical abdomino-perinæal operation for *all* malignant growths of the rectum and pelvic colon, no matter how small they may be or where placed. This operation consists in the formation of an iliac anus, with the entire removal of the rest of the sigmoid colon, the rectum with the soft parts adjacent thereto, and the anus. He states that his reason for advocating this extensive operation in *all* cases of rectal cancer is the great frequency of recurrence which followed his perinæal operations, under which heading he includes para-sacral excision with end-to-end anastomosis. Out of fifty-eight operations there were fifty-four recurrences.

This paper was accompanied by an elaborate table of cases, whilst the investigation into the spread of the disease, involving a large amount of work, reflects, if he will forgive my presumption in saying so, great credit on the author.

I, for one, sympathise with Mr. Miles in the unfortunate results attending his perinæal and sacral incisions, nor can I offer any explanation to account for them, unless it be that many of these cases were unfavourable for any operation, the disease having probably passed beyond the limits of the bowel at the time of the operation, and had better have been left alone!

On reading through his tables I find, in reference to "the spread of the disease found during the operation," by which is meant extension beyond the bowel, that in no less than thirty cases this, in fact, did obtain.

In some cases the peritoneal coat was permeated, in others the growth was adherent either to the sacrum, prostate, or vagina, whilst the pre-sacral glands were usually involved!

Concerning his table of twenty-six abdomino-perinæal operations, there was evidence of the extension of the disease beyond the limits of the bowel in all but three cases, and yet of those who did not succumb to the operation (and of those who did there were ten) there had only been one case of recurrence.

From this it seems to me we learn that cases which should be deemed inoperable when attacked

from below can be successfully treated by the radical abdomino-perinæal, though the immediate mortality up to the present is very high, but then desperate diseases may need desperate remedies!

I have always held the opinion that all growths fixed anteriorly or laterally, even when involving only a part of the bowel circumference, are best left alone, and, indeed, most of those fixed posteriorly —slight sacral or coccygeal fixation need not bar operation.

If the meso-rectal glands or lymphatics are markedly enlarged and remain so, either after a preliminary colotomy or after rest in bed with frequent rectal douching, recurrence after operation is not improbable. It must be admitted that enlarged glands are often only inflammatory and not malignant.

I have several times met with these glands when carrying out a para-sacral excision, and have been much disappointed on finding that I was unable to remove the entire chain. Some of these cases are alive and well to-day, having passed the three years' limit—a proof, I think, that these glands were not cancerous.

Mr. Sampson Handley's paper in the 'British Medical Journal' of April last is full of interest. By the help of a muci-carmine stain he claims to show that the spread of rectal cancer, even in early cases, extends in the submucosa, or superficial to it, far beyond the tangible disease, and therefore joins with those who advocate the freest removal possible, even for early and small cases.

He records a case (evidently a very favourable one for the posterior operation) in which, by the combined method, he removed the growth and ten inches of gut, including the sphincters and anus. He states that the disease, as proved by the muci-carmine staining process, extended up to the point of section six inches above the growth, "and for all he knew it might have extended higher." I ask, then, wherein did the patient benefit by this extensive operation? Would she not have been better off after a para-sacral operation with restoration of bowel continuity and use of normal anus, and at least as well off as far as the risk of recurrence goes?

If Mr. Handley is right I cannot understand how we have managed to obtain so many cures when contenting ourselves with the removal of the growth and one inch of apparently healthy bowel above and below it. Either, it seems to me, that Mr. Handley's stained foci are histological and not pathological, or the cancer-cells degenerate still more rapidly after the removal of the growth, until they become quite inert. Mr. Harrison Cripps published a long letter in the 'British Medical Journal' last May criticising Mr. Handley's paper, and pointing out that it was quite at variance with his (Mr. Cripps's) clinical experience. With this letter I am in full accord.

A few words now as to my own results.

For some ten or twelve years I have abandoned the old perinæal operation, excepting for cases of epithelioma of the anus, and in the majority of cases have performed the extirpation by the sacral or coccygeal route. I have not concerned myself with the preservation of a bone flap, as patients appear to suffer no inconvenience from the loss of the coccyx and lower part of the sacrum. If the growth is extensive, involving the removal of, say, four inches or more of bowel, or if it extends so low down that end-to-end union is problematical, I advise a preliminary colotomy. If the anus has been preserved and the continuity of the bowel re-established the artificial anus can be closed subsequently, but bearing in mind the chance of recurrence, this may well be postponed for a year. When performing the colotomy the hand should be passed into the abdomen for the purpose not only of defining the limits of the growth but also of exploring the liver and ascertaining the presence or otherwise of enlarged glands and lymphatics or of secondary growths elsewhere, the existence of which would, of course, negative any attempt at excision.

Extirpation by the sacro-coccygeal route I have carried out eighty-one times with four deaths, or an immediate mortality of 5 per cent. There was recurrence in twenty-two cases; hospital cases lost sight of numbered fourteen. Forty-one cases are well as far as is known; of these one is still in a surgical home making a rapid convalescence.

The statistics of my cases in private since the year 1895 are as follows: Twenty-four cases; one death from septicæmia; eight recurrences, in the majority of cases at six months; fifteen are alive and well, and of these six have the use of the normal anus; six underwent a previous colostomy, and in one case this was closed after the lapse of two years; ten cases have passed the three years' limit. To sum up, the mortality after this operation is 5 per

cent.; recurrences, about 33 per cent.; cures, about 50 per cent.

In looking through my cases the following points especially strike me:

(1) That a preliminary colotomy seems to offer some bar to recurrence.

(2) That when recurrence did take place it was with one exception *in situ*, *i. e.* at the level of the bowel section.

(3) That partial excisions are *not* to be condemned. By partial, I mean the removal of only part of the circumference of the rectum. Theoretically they are unsurgical, but let facts speak for themselves!

I have had five cases of partial excision, all recovered, and have passed the three years' limit; three are quite well, with use of the normal anus; one has a sacral anus, and one operated upon in 1895 died some years afterwards without recurrence. In all cases the growths were small, none larger than a florin, freely movable, and limited to the post-rectal wall. Microscopical examination proved them all to be malignant ademonata.

In one case operated upon four years ago I removed in addition four enlarged glands, in two of which metastatic deposits were found. This lady is now in perfect (rectal) health, and by a digital examination it is almost impossible to say that she has undergone any operation.

(4) Most of the cases of recurrence have been either in young subjects or in cases where the section of the bowel was perilously near the disease, *i. e.* the specimen when removed did not show an inch of healthy mucous membrane above and below the growth.

In conclusion, I would express the opinion that rectal cancer in persons of forty-five and over is not prone to recur if removed sufficiently freely, but in the young it is very malignant, and is likely to recur unless removed at a very early stage.

That cases of cancer of the sigmoid colon are very favourable ones for operation by the abdominal route, with end-to-end union, unless obstruction is present, when a temporary colotomy should be the rule. Subsequent excision with intestinal anastomosis can then be carried out. In these cases I have had excellent results.

That for growths involving the upper rectum and pelvic colon an abdomino-perinæal operation is necessary. Miles's amplification of it also has its field of usefulness for cases in which the disease has passed beyond the walls of the bowel and which otherwise should be considered inoperable.

The radical abdomino-perinæal operation entails an artificial anus, which, when necessary, is no great evil—indeed, as I have said, it may be a positive advantage in preventing a tendency to recurrence. When the artificial anus is intended to be permanent I prefer hypogastric colostomy through the left rectus muscle to an iliac one as I find it confers a much larger measure of control. But where a subsequent abdominal section is purposed for an excision the iliac site is the better, for, being placed farther out, the abdominal wound is not so likely to be infected. To my mind the ideal operation for cancer of the rectum when situated between the fourth sacral vertebra above and a point two and a half inches above the anus below is Badenheuer's modification of Kraske's operation with end-to-end suture. It is true that a dorsal fistula will persist for a time, but this usually heals in from two to three months if the stricture at the line of union is kept dilated. I have always found contraction take place at this point with a very marked anterior spur. This in time practically disappears if treated by dilatation, for which purpose I find nothing to equal Hegar's double curved metal dilators.

In the face of my own results I cannot agree with those surgeons who advocate a combined operation for *all* cases of rectal carcinoma any more than I agree with those who, for a small and limited cancer of the breast, would remove, shall I say, all but the ribs and thus cripple the patient for life.

Sir FREDERIC EVE said: It had been truly stated that the surgery of cancer was the surgery of the lymphatic system. It was only in recent years that the widespread permeation of cancer had been understood. Mr. Handley believed that the permeation in the bowel wall extended five inches above and below a rectal growth. Judged by clinical observation and the test of permanent cure, the speaker felt no doubt that operations of the Kraske or perinæal type were not sufficiently thorough. Steinthal, taking the statistics of numerous surgeons, came to the conclusion that primary cure was obtained in only one fourth or one fifth of the cases operated upon. Gant, taking the work of nine surgeons, found permanent cure

in only 15·6 per cent. Mr. Swinford Edwards' statistics, in his article in Burghard's 'Operative Surgery,' showed that out of fifty of his cases traced, in only seven did patients reach the three years' limit, and only three of them exceeded it. Every surgeon was aware that the three years' limit was too short, for many cases recurred after that time. Two of his own cases had recurred after five years. With regard to Miles's statistics, out of fifty-eight cases traced, fifty-four were known to have had recurrence. Even allowing, with Mr. Cripps, that some of these cases were unsuitable for operation, it was impossible that any large number of them could have been. These facts showed very clearly that the Kraske or perinæal method could not be relied on to give a permanent cure. For many years in clinical teaching he had emphasised the importance of exploring the abdomen in the majority of cases of rectal cancer. The necessity of that was obvious to ascertain the condition of the glands, of the pelvic peritoneum, the amount of fixation of the upper part of the bowel as well as that of the lower, and finally the condition of the liver. For that reason he formerly advocated the performance of a preliminary colostomy. The advisability of exploring the abdomen was a strong point in favour of the abdomino-perinæal method. In recent years his work in that department had been largely influenced by an important article by Rotter on the combined operation, which appeared in 'Langenbeck's Archives' for 1906. Rotter strongly advocated restoring the continuity of the bowel. The speaker, from his own experience, had come to the conclusion that the operation which fulfilled the two most important conditions—namely, a low mortality and a prospect of permanent cure—consisted in performing the abdomino-perinæal operation with the formation of a permanent colostomy. The method of controlling bleeding during the operation was of importance. After the abdomen had been opened the peritoneum was incised on the outer side of the pelvic colon and sigmoid flexure, and the large intestine, with the peritoneum, was stripped inwards to the middle line; the inferior mesenteric artery was thus exposed with the left ureter. A ligature was placed on the inferior mesenteric artery as high as was necessary, according to the condition of the glands which lay along it. That completely controlled the blood supply. The glands and cellular tissue were stripped from the sacrum, and the peri-rectal tissue removed. He agreed with Mr. Miles that in many cases the fat in the ischio-rectal fossæ and the levator ani should be taken away. Since many rectal cases occupied the ampulla, or true rectum, it seemed to him impossible to excise sufficient bowel below the growth unless the anus were sacrificed. He always separated the rectum as low as possible from the abdomen (as far as the levator ani), and then performed the rest of the operation through a perinæal incision, having first closed the peritoneum through the abdomen. The operation of restoring the bowel with the combined abdomino-perinæal operation could not be dissociated from the great risk of sloughing of the bowel or of leakage from giving way of the posterior sutures. It was to that sloughing and leakage, together with the more prolonged operation, that the high mortality of the abdomino-perinæal operation was due. Among twenty-five cases published in 1906 Rotter's mortality was 44 per cent., and in twenty-six cases published in 1908 Mayo's mortality was 26 per cent. During the last two years the speaker had performed the abdomino-perinæal operation, combined with a permanent colostomy, with uniform success. He believed that that operation would present a very low mortality. Though he advocated it for a large number of cases of carcinoma of the rectum—viz. those occupying the ampulla or true rectum—he recognised that when the disease affected only the anal canal and anus it might be right to attack it from the perinæum, but the coccyx should be removed, the sacrum cleared, and the groin glands extirpated. If the disease occupied the lower part of the pelvic colon, or supra-peritoneal portion of the bowel, so that it could not be dealt with entirely abdominally, then it was justifiable to attempt the combined operation, with restoration of the bowel. The invagination method appeared to be least likely to be followed by sloughing. In performing it there need never be any anxiety as to the possibility of bringing down the colon. Every surgeon had used the incision on the outer side of the sigmoid flexure in order to mobilise it. Mr. Moynihan had suggested that this method could be still further utilised in the combined operation to mobilise the descending colon by prolonging the incision upwards along it.

After closing the abdomen the operation was completed through a perinæal incision through which the tumour, with the lower portion of the bowel, was brought and the junction affected. In this way soiling of the peritoneum was avoided, and the perinæal wound was very useful for drainage of the pelvis; it also served to keep an observation on the suture line.

Mr. WATERHOUSE said: I desire to emphasise the vast importance of preliminary preparation. The surgeon who performs excision of a cancerous rectum with the bowel distended with fæces courts disaster. When intestinal obstruction, the result of cancer of the rectum, is present, an inguinal colotomy must be immediately performed, and all attempts at radical cure abandoned for some days. Failure to recognise the importance of this rule has, to my knowledge, proved disastrous.

Even in cases in which no obstruction has occurred, and in which daily or more frequent evacuations of the bowel take place, it is, in my opinion, essential to attempt to diminish as far as possible the septicity of the bowel. I am accustomed to administer castor-oil on the sixth and fifth days preceding the operation. During the four days prior to operation the patient is given beta-naphthol and salol āā gr. v in cachet four times daily, and his food is restricted to milk and meat extracts. Eight hours prior to the operation $\frac{1}{2}$ gr. of opium is given in pill form, and half an hour prior to the administration of the anæsthetic a hypodermic injection of $\frac{1}{3}$ gr. of morphine hydrochlorate and $\frac{1}{80}$ gr. of atropine sulphate is administered. The rectum is washed out twice daily for two days and again on the operating table with a 1 : 2000 solution of permanganate of potassium. Thanks to the employment of the above measures the bowels rarely act until the third, fourth, or fifth day, and a considerable number of incisions (para-sacral) heal completely, or more commonly largely by primary union.

Excision of rectum.—I am a strong adherent of the para-sacral method, to which we are indebted to Kraske and Kocher. In my opinion it is the method of choice in almost every case of cancer of the rectum in which radical treatment is justifiable. I am in the habit of making use of this method in practically all the cases that come under my care. On no occasion have I, in order to gain sufficient space, found it necessary to do more than remove the coccyx, *i. e.* I invariably follow the method of Kocher. In the roomy pelvis of the female I have never been inconvenienced by want of space. In the narrower pelvis of the male I confess that often have I felt that the space was too limited, and have felt tempted to remove a portion of the sacrum. I have, however, persevered, and have in the end managed to complete the operation without further bone excision. I am convinced that this is a matter of real importance. I have no doubt but that division of the sacrum markedly increases the shock of the operation, and that the existence of a bone wound in a cavity likely to become septic is a very real danger.

Even in cases of anal cancer I have long abandoned perinæal excision. In such cases admirable access is obtained by the para-sacral incision. Operating thus, from above downwards, is a far less sanguinary procedure than the method of perinæal excision, for the reason that the blood-vessels can easily be clamped before division. No one, I believe, who has operated by this method would return to the older perinæal excision. If the anus be sutured, or plugged with gauze at the commencement of the operation, the diseased part can readily be pushed downwards outside the anal orifice, and being there retained by an assistant, the para-sacral incision is sutured before the protrusion is amputated. In this way union of the para-sacral by first intention may almost always be obtained, as no fouling of the wound, hands, or instruments should take place.

I am fully aware, however, that the great question we are called upon to discuss to-day, and if possible, to settle, is, What is the best method of procedure in cases of cancer involving the first and second segments of the rectum of our student days, or what should now be called the lower end of the pelvic colon and the rectum proper? Is the operation of the future to be a combined abdominal perinæal or abdomino-anal, or some modification of the para-sacral method of Kraske and Kocher? My vote is decidedly for the latter.

I freely admit that the combined abdominal methods appear at first sight very attractive. They seem in accord with the modern doctrine of freely removing all possibly infected glands, and I gave the method a fair trial. I have, however, for all cases which can properly be called "rectal cancers," practically abandoned it. Among the reasons for my decision, the chief is the difference in the mortality of the two methods. So far as I am able to judge, the mortality of the combined abdominal method is not less than 30 per cent.,

and probably more. That of the para-sacral method is, I believe, not more than 5 per cent. Surely the adherents of the more serious operation must submit reasons more cogent than any I have hitherto held before they can expect surgeons to abandon the far safer para-sacral method? The reasons for the greater mortality of the former method are not far to seek. It is a question of two extensive incisions instead of one, of an operation of much longer duration—a matter of great moment in patients exhausted by pain and sleeplessness, not to mention septic absorption from the cancerous growth.

Further, in practically every case operated upon by the para-sacral method, it is possible to suture the peritoneal margins of the opening to the bowel after it has been drawn down to the fullest extent, and thus to close the opening into the peritoneal cavity prior to excising the growth. The operation thus becomes practically an extra-peritoneal one Thus I have no knowledge of an operation performed by the para-sacral method proving fatal from septic peritonitis, even in cases in which some of the stitches uniting the segments of bowel cut through and fæcal extravasation with septic cellulitis resulted.

In the excision by the para-sacral incision it will almost always be found possible to unite the two ends of the rectum, and not infrequently to bring down the upper end and suture it to the anal margin after enucleating the mucosa lining the anal canal. This latter method is unquestionably the better of the two, though the surgeon must not expect union *per primam* in every case between the upper end of the bowel and the anal margin. Even when union fails completely the result is generally quite satisfactory. The secret of success is free division of the peritoneal attachments of the bowel and free division of all bands connecting the posterior surface of the rectum with the sacrum. The amount of mobility of the bowel after free division of the peritoneal reflections is a revelation to those who practise this manœuvre for the first time.

End-to-end union of the bowel segments has afforded me satisfactory results in many cases, though I am bound to confess that in the majority of instances leakage has occurred on the posterior aspect of the line of union. The leakage of fæcal contents has in such instances merely led to the occurrence of a posterior sinus, which has required division by means of a curved knife introduced *per anum* and directed backwards, dividing the sphincters. Even in such cases very fair control of the motions has been attained, except, perhaps, in the presence of diarrhœa.

As to the various combined abdominal methods, I freely state that the more I see of them the less I like them. They are in my opinion far too severe, certainly for the ordinary run of cases of operable rectal cancer. They are double operations, and entail a severe strain upon debilitated patients. To my mind, operations upon the rectum, *i. e.* below the brim of the true pelvis *viâ* the abdominal route, especially in stout patients, are extraordinarily difficult and unsatisfactory. I am certain that many instances in which the surgeon has experienced grave difficulties in the performance of the para-sacral operation have been examples of cancer of the large intestine above the pelvic brim, and therefore not cases of rectal cancer. I willingly admit that for cancers of the colon above the pelvic brim the abdominal route is preferable, but I contend that for rectal cancers the para-sacral method possesses all the advantages.

In not a few cases in which I have commenced the operation *viâ* the abdominal route and found a cancer at, or just above, the pelvic brim, I have deemed it safer to perform a colotomy by bringing the upper end of the bowel out through the abdominal incision and closing after excision of the cancer the lower segment, without making any attempt to perform an end-to-end anastomosis or to bring the upper segment down to the anal margin. I am convinced that this is a far safer operation than any of the combined methods, and that the disadvantages of an inguinal anus are much exaggerated.

In all cases in which I have performed one of the combined abdominal methods I have considered it prudent to make a stab puncture through the skin just posterior to the anus, and I have recently adopted—I think with success—this method of drainage in my para-sacral excisions.

One point I desire to refer to, viz. the advantage of the removal of the infected glands which lie along the iliac vessels. In my opinion, when the disease has spread to these glands the prospects of radical cure are practically *nil*. Even in a case in which there was reason to suspect involvement of these glands in rectal cancer I would perform a para-sacral excision, and then later on open the abdomen and attempt extirpation of the iliac glands. In my opinion the glands that in all cases call for removal are those between the rectum and the anterior surface of the sacrum, and there can be no question that these can be far more radically removed by the para-sacral incision than by the abdominal method. I have not entered upon the question of the suitability of cases of rectal cancer for excision. It goes without saying that every movable rectal cancer should be extirpated. Firm anterior rectal adhesions are in general a bar to radical excisions, but even the firmest posterior adhesions do not in my view contra-indicate operative treatment. From my remarks it will be seen that I strongly hold the opinion that the great majority of cases of rectal cancer deemed operable should be removed by the para-sacral method.

(*To be continued.*)

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THE WEAK HEART.*

By W. J. FENTON, M.D., F.R.C.P.

LADIES AND GENTLEMEN,—A brief explanation of the title of my lecture to-day is needful; in the first place the word "the" would have been better omitted for the reason that what I wish to consider with you are the conditions under which weakness or insufficiency of the heart may develop, in order that we may arrive if possible at a clearer understanding of what constitutes weakness of this organ; and in the second place heart weakness, insufficiency, and dilatation seem to be either synonymous with failure, or at least different degrees of the same thing, and unless this is borne in mind a good deal of uncertainty upon the subject will result.

I propose to deal with the subject under two headings, first considering what constitutes the normal working of the heart, and second, how a defective working may be brought about, together with its bearing upon diagnosis and treatment.

It is evident, however, that to treat of these in detail would require more space than is afforded by the limits of a single lecture, and therefore it is necessary to put the matter in general terms, emphasising those points which are of clinical interest, and avoiding as far as possible those having a more purely physiological bearing. The heart has so often been compared to a force pump that there has been a tendency to overlook its great power of adaptation to circumstances, and to attach too much importance to the state of the valves, and the gross physical signs to which they give rise when incompetent.

I need not detain you with a minute account of the anatomy of the heart, but it is essential to keep before our eyes the idea that the valves are mechanical contrivances, which play, it is true, an

* A lecture delivered at the Hospital for Consumption and Diseases of the Chest, Brompton.

important part in the cardiac economy, but are, notwithstanding, of much less relative importance than the musculature. This is the foundation upon which the cardiac action rests and to which we must turn when we wish to arrive at a true understanding of the heart's action. When this fails the powers of the individual, or even his life, are endangered, whereas valve defect may be more or less perfectly compensated; in some cases so much is this the case that the ordinary mode of life is little interfered with through many years, and so long as this compensation holds good, the valves are, in a manner of speaking, negligible, the necessary consequence of which is that the conditions under which the muscle performs its function lie at the root of the subject.

You are, no doubt, familiar with the theory of automatic cardiac activity, the theory which assumes that the functions of cardiac muscle are inherent properties of that tissue, and assigns to the nerves the *rôle* of guide, philosopher, and friend, controlling an otherwise insensitive organ and rendering it subservient to the ever-varying needs of the body, an action which has been compared to that of a horseman over his horse, or to bring the simile more up to date, that of a chauffeur over his car.

I have not time to discuss the myogenic theory here, but I wish to point out that it assumes the heart to possess four very important functions, namely, rhythmicity, irritability, conductivity, and contractility, to which must be added a fifth—tonicity—but for the purpose I have in view we need only consider tonicity and contractility, as these are of special importance in the subject before us.

A normal cardiac cycle includes four main phases: During auricular dilatation blood enters from the great veins, and after the opening of the auriculo-ventricular valves passes to the ventricles, but whether passively or under an active suction exerted by the walls of those cavities need not now detain us. Auricular systole follows, driving a variable amount of blood onward in addition to that which has already entered the ventricles, and putting their walls on a certain degree of stretch; these chambers in their turn contract upon their contents, but do not at once open the sigmoid valves, a period of time elapsing before the ventricular pressure is sufficiently raised to overcome the resistance of the column of blood in the aorta. At this time the first sound occurs, and thus marks the beginning of systole; finally the sigmoid valves are forced open by the increasing intra-ventricular pressure, and their contents pass into the aorta, the fourth phase being, of course, ventricular diastole.

So far the matter is comparatively simple; the heart evidently does a certain amount of work, which, other things being equal, can be measured by the amount of blood discharged in a given time. But more than this is needful; the requirements of the body are subject to constant variation, and therefore the demands made upon the heart must vary also, and must be met when they arise. They are met by what is called the reserve power of the heart in two main ways: by an alteration in the rate of the beat and by an increase in its amplitude—that is, by modifying the quantity of blood which the ventricle contains at the end of diastole. It will be convenient to consider these points separately. It is, of course, a matter of every-day observation that exercise, or any condition which makes a call upon the heart for an additional blood supply, is accompanied by an acceleration of the rate of beat, and it is known that this acceleration is accomplished mainly at the expense of diastole, but it is sometimes forgotten that the diastole or rest period is the time when the organ performs its chief nutritive functions, and that therefore any long-continued increased frequency of rhythm not only throws an additional strain upon the organ, but does so at a time when its nutrition is most hampered. Moreover, strain raises the peripheral blood-pressure, at any rate at first, though in athletes this rise is not of long duration, and tends to pass off early. The bearing of this is obvious, but it is modified by the effects of altering the amount of ventricular contents. It has been shown experimentally, by increasing the work done by the heart, such as by compressing the aorta or by temporarily increasing the amount of fluid in the circulation, that an increase in the size of the heart takes place, and observation in the case of athletes shows that after great exertion the heart is greatly increased in size, but that the normal proportions are reproduced after a few hours' rest. Now these observations go to show that the ventricle is not to be looked upon as an inelastic bag having fixed dimensions, any

departure from which constitutes pathological dilatation, but as a contractile receptacle capable of a large degree of delicate adjustment, both as to capacity and rate of beat, to meet the constantly varying demands of the organism. There is a physiological as well as a pathological dilatation, the result of which is to economise the power of the heart to a greater extent than would be the case if great demands could only be met by acceleration. Precisely how this alteration of capacity is governed is uncertain; that it is dependent upon tone is probable, also that it is under the control of the nervous system, but its effect is to vary the amount of blood delivered from the ventricle, provided the muscle is strong enough to expel it. Another way in which tone of the ventricular muscle may assist in augmenting the amount of blood thrown into the aorta is that normally the auricles do not completely empty themselves during their systole, the quantity being determined by the capacity of the ventricles, and where this is increased by a lowered tone the auricular discharge must be proportionately augmented.

The ventricles, thus filled and distended by the contracting auricles, empty themselves more or less completely into the aorta, and since the load is subject to variation, the energy so expended must vary also if the circulation is to be efficiently carried on. This difference between the energy expended by the heart whilst at rest and that during exertion constitutes its reserve power and finally determines its working efficiency, and this, it will be observed, is mainly a question of contractility; so long as it is equal to the load to be dealt with there will be no trouble, but the moment it fails to contract with sufficient energy to expel its contents signs of distress will manifest themselves. Now this reserve is not a fixed amount, but varies with each individual, and also in the same person at different times; it is consequently largely a relative matter determined by several factors, and must be considered accordingly.

It is here, however, needful to insist that the heart is an organ whose normal state is one of rhythmical contraction upon a load, and within limits this is essential for the development of its full activity. The results of experiment will illustrate my meaning. If the gastrocnemius muscle of a frog be taken and stimulated with an electric current of constant strength, it will be found that up to a certain point an increase in load is accompanied by an increased energy of contraction, and that the best result is by no means that associated with the smallest load—there is, in fact, an optimum load at which the best result is obtained. If this be applied to the heart it implies that, so long as the reserve is not unduly encroached upon, work is good for the organ, and that under certain conditions limitation in this respect is harmful, and may, as in other muscles, tend to perpetuate trouble.

I do not propose to deal with the effects of valve lesion, interesting as it would be from the point of view of tone and contractility, beyond briefly referring to that condition. Broadly speaking, the same general principles are applicable owing to the effects of compensation, which, as the term implies, indicates that the effects of the lesion have been neutralised, but there are important modifications which must not be lost sight of: In the healthy heart the reserve power is measured, as I have already said, by the difference between the energy exerted whilst the body is at rest and that of which it is capable during exertion, but where there is valvular disease the work done at rest is necessarily greater than in health, and the reserve is more easily encroached upon to a degree commensurate with the severity of the lesion; moreover, the condition is abnormal, and certain factors are introduced which are not entirely satisfied by the occurrence of hypertrophy, resulting in a greater tendency to failure than is present in the mechanically intact organ. But at the same time, making all allowance for these considerations, the broad principles which I have laid down are applicable; it is the extent to which they may be applied which varies.

The question of cardiac efficiency or weakness is mainly, therefore, one of contractile power, and this seems to lie at the root of the relative strength or weakness of the organ—that power to adapt itself to varying loads without serious disturbance, and which, as in other muscles, is at its best under certain conditions.

We may now inquire under what circumstances these functions may be expected to vary, and then pass on to a consideration of the symptoms which arise. Broadly speaking, the main condition which governs the activity of an organ is adequate nutrition, but if the question be asked, Given suitable

nutritional factors, may failure take place notwithstanding this? it is not easy to answer, and I think one must take into consideration certain general principles. It seems probable that, as in other tissues, a certain hereditary factor plays a part, modifying what may be called the durability of the organ, a condition of things in which stresses which may not greatly exceed the normal produce results altogether incommensurate, such, for instance, as takes place in the vessels or nervous system under like circumstances; this does not imply that breakdowns of this kind should be of frequent occurrence, but that an organ in a state of never-ceasing activity might be expected to show defects from time to time where its constructional material is not of the highest order.

In addition to the personal factor which I have just indicated, there is another which has a relation to treatment: it is the effect of physiological stimulus. I have already mentioned that there is a certain optimum load at which the best results are obtained, and departures from this are likely to be followed by effects more or less harmful, but here, again, the whole of the circumstances must be taken into account. The heart, like other muscles, adapts itself to the conditions of its environment, and the amount of its reserve power will be largely determined by its mean every-day requirements, and will accommodate itself to limits within which it has been called upon to act. The capacity of the trained athlete for effort depends upon this, and the early exhaustion of the man who is out of condition receives its explanation along similar lines; in both instances there is a co-ordination between the work to be done and the power to do it, but the extent to which this can be taken advantage of depends upon what has been the mean requirement before the exertion was undertaken. An instance of my meaning is the following: A man of sedentary habit is called upon for a sudden effort, or deliberately undertakes it under the impression that the greater the vigour of the exercise the greater will be the good he will derive from it, and in this manner throws upon his heart the burden of an altogether unaccustomed load with little or no preparation. Symptoms of distress arise early, warning him that his reserve power is not equal to the demand being made upon it, and provided the warning is not neglected no great harm results in the great majority of instances, but should some factor be present making for muscular defect, injury may arise coincidently with the warning, and the case may be quite otherwise.

On the other hand, a young female patient, or a lanky, growing youth, with sympathetic and injudicious friends, complains of being easily tired, faints, has palpitations, and various pains referred to the præcordium with other symptoms of a neurasthenic type. These cases, which are quite different to the last, are sometimes labelled "weak heart," and a course of treatment laid down for the supposed local trouble in which more or less complete rest plays a very prominent part, or else vigorous exercise may be prescribed without definite adjustment to the circumstances of the case, and the results necessarily fall short of expectations.

The main conditions which govern the nutrition of the heart are those dependent upon the character of the blood supply, its quantity, quality, and the presence of toxic material, any of which may give rise to acute or chronic insufficiency. Instances of alteration in the quantity of nutriment will occur to all. They mainly arise in connection with obstruction of the coronary vessels, and produce results depending upon the rate at which the vascular supply is cut off, sudden blockage causing death, more chronic occlusion being accompanied by organic changes in the muscle, ending in fatty or fibroid degeneration. It is evident that such conditions will react prejudicially upon the heart and will be accompanied by gradual failure of its powers, producing symptoms more or less easily recognisable. There is, however, another circumstance which will give rise to diminution in the nutritive supply—I refer to the fall in blood-pressure which is said to take place when the heart is failing. This no doubt is the case, but probably it does not directly follow upon weak cardiac action until very late, and in most instances the fall in pressure noticed is due to failing tone of the vasomotor centre with consequent dilatation of the vessels, and it is well to bear in mind that all the bearings of cardiac pathology are not to be found in the heart itself; many points of extreme importance must be looked for in the musculo-elastic tubing through which the blood flows.

Alteration in the quality of the blood supply occurs in connection with the various forms of anæmia, but it is here difficult or impossible to

entirely separate this division from the next; at the same time, the heart, owing to the never-ceasing nature of its activities, demands a full supply of oxygen, and whatever tends to curtail this will without doubt react unfavourably upon the organ and induce a tendency to insufficiency.

The presence of toxic material will readily account for certain cases. The various fevers and infections are clearly recognised under this heading. Diphtheria, influenza, pneumonia and typhoid at once are suggested, and in these diseases the possibility of serious cardiac failure is always present.

In addition to poisons of this nature there are others, as tea, coffee, tobacco, and alcohol; all or any of these if indulged in to excess may affect the heart in such a way as to reduce its resistance to adverse events, and turn the scale in the direction of insufficiency more or less marked.

I do not know if I have succeeded in making myself sufficiently clear to you, but what I wish to emphasize is that the healthy heart is possessed of a great amount of reserve power and adaptability, which also admits of cultivation, but is limited by individual considerations, and will in the main be determined by the mean of every-day requirements. At the same time the due performance of its normal functions depends upon an unimpaired nutritive supply, and before cardiac weakness is spoken of as though it denoted a primary defect it is needful to exclude local and general causes, and if these can be reasonably set aside a diagnosis of weakness or insufficiency should be made with great reservation, owing to the physical and often mental disabilities which it imposes upon the patient.

We may now ask what are the signs and symptoms by which we are to be guided, and here it is important not to allow attention to be too much centred upon the heart itself; it is only by a careful survey of all the factors of the case that a right conclusion can be arrived at. And first let me mention a number of symptoms upon which the patient lays great stress, but which are not necessarily significant of cardiac trouble. It should not be forgotten that in an organ like the heart, which is normally outside consciousness, he has little but subjective sensations to guide him, and as he associates with this viscus the idea of an organ peculiarly essential to life, he naturally cannot be expected to take an unbiassed view of sensations of discomfort arising apparently in connection with it, but it is none the less worthy of remark that in a majority of cases, when a patient complains directly of his heart the organ will be found perfectly sound, and the origin of his trouble must be looked for elsewhere.

Palpitation more often owns a toxic cause, such as tea, coffee, or tobacco in excess, or is of reflex origin from the gastro-intestinal canal. It occurs of course associated with true cardiac lesion, but taken alone it is not evidence of disease.

Tachycardia may be of nervous origin, or be associated, as it frequently is, with early tuberculosis.

Fainting, again, except in Stokes-Adams' disease, has not the common association with cardiac insufficiency which it is often supposed to have. It is for the most part a nervous phenomenon, and its cause is not often found in the heart. It is true the subject of morbus cordis may faint, but that is not the same thing: he faints in spite of his heart condition, and not necessarily on account of it.

Præcordial pain is often gastric in causation. How many patients complain of pain over the heart and suppose it to be anginal? Anginal pain is more likely to be sub-sternal, and is associated with other phenomena. But a caution is here needful: many cases of mitral stenosis occur in which cardiac pain more or less localised is the sole complaint, and careful search does not reveal evidence of gastric or other cause.

Evidence of cardiac insufficiency or weakness—or what is a still better term, as it admits of little misconception, failure—is to be looked for in a defect in what one might almost call its sole function, that of delivering a normal amount of blood into the aorta and pulmonary arteries under normal conditions. When it fails two main symptoms will arise, one referred to the pulmonary system, the other to the peripheral—I mean dyspnœa and œdema. There are others of course, but I am speaking of early signs. Whilst everyday stresses can be borne without serious discomfort, and whilst even excessive exertion is recovered from readily, we may be assured that the heart is equal to the demands made upon it, but the moment there is unusual breathlessness, which recurs easily, or when the patient begins to experience a true limitation of his powers in this respect, defect may be assumed,

and what Dr. James Mackenzie calls a "limitation of the field of response" has begun; the organ responds to a demand, but with difficulty, and if the demand is continued it will definitely fail—in other words there is cardiac weakness, or early failure. Along with this there may, or may not, be slight pitting over the tibiæ; if this occurs the failure is the more obvious, in so far as it has shown unmistakable evidence of existence.

In putting the matter in this way I do not wish you to infer that when the symptoms which I have enumerated as being not necessarily cardiac in origin occur they are to be entirely neglected, but when they are complained of by a patient, and evidence is not forthcoming that cardiac function is impaired in the way I have just referred to, great caution is needed before committing oneself to a diagnosis of insufficiency with its attendant prohibitions.

Before passing to a brief review of the more reliable physical signs I wish to digress for one moment. What it is needful to decide in a patient under examination is not whether the heart is equal to its task whilst the patient is at rest, but what is its behaviour under stress. You will have gathered from what has gone before that when cardiac inadequacy sets in it is its reserve power which is the first to feel the strain, and consequently it is to the reserve power that we must look in estimating if the organ has sustained damage. With this object in view it is evidently useless simply to examine the patient in the recumbent position or after he has rested for a time; he must be made to perform some active movement, such as walking up and down stairs, and then examined before a conclusion is arrived at. In this way only can any latent defect be made to show itself, and without it an entirely erroneous conclusion may be drawn.

The physical signs which we should expect to find are those usually associated with disease of this organ, though the degree to which they can be demonstrated necessarily varies in different cases, and the best-laid plan to discover a latent defect may be set at nought by an anatomical or pathological feature of the neighbouring organs.

The first evidence to be looked for is perhaps that of enlargement, as indicated by the position of the apex-beat, or, stated more accurately, the impulse, for I need hardly remind you that this does not of necessity correspond with the true apex-beat. Only when it is supported by the results of careful percussion can the assertion be made that the impulse is due to the left ventricle. On the other hand, it may be difficult or impossible to define the cardiac outline with certainty, and in these cases an X-ray photograph may be of assistance. I might mention here that in percussing out the heart it is advisable to make use of very light percussion, as under these circumstances it is easier to hear small differences of tone than when heavy strokes are employed.

The character of the impulse is of value also, provided the caution referred to above is not lost sight of; an impulse which is diffuse, or can be seen but not felt, is a valuable indication in some instances.

The indications afforded by the pulse are of great value. Frequency, if dissociated from mere excitement, should be carefully ascertained, especially the difference between rest and activity; a moderate rise after exertion is normal, but if it is excessive it is very suggestive of an organ nearing the limit of its reserve. Of pulse irregularity it is not possible to speak definitely. There is no doubt that *per se* it may exist for long periods of time without apparent harm; even great irregularity is not incompatible with apparent health, but when it occurs with valve disease, or where it appears in old people, it is to be regarded with great suspicion; at the same time it must be considered along with factors to which I have already called your attention.

A form, known as pulsus alternans, *i.e.* where beats of unequal strength occur at practically equal intervals, is said to be associated with failure of contractility, and indicates an outlook more or less serious.

Intermittent beats are not to be regarded as necessarily indicative of cardiac mischief; indeed, their precise significance is in great doubt. Where they are met with all other conditions must be taken into account, and the case judged of by the capacity of the heart to perform its function as tested by inquiry as to what takes place under strain, and where there is no faltering the intermissions may generally be disregarded. However, under certain circumstances, such, for instance, as great exhaustion and specific fevers, the occurrence of pulse intermission is to be regarded with suspicion.

The occurrence of symptoms pointing to heart insufficiency in cases where there is also high tension or arterio-sclerosis is of course grave, and generally associated with lesion of the heart-muscle.

Auscultation must not be relied upon too absolutely in the absence of evidence of functional impairment; it is well known that it is extremely doubtful how much reliance is to be placed upon the character alone of the first sound, and if weakness is suspected it should be considered along with the factors I have referred to. The same may be said if an organic murmur is present; in a case of the kind the heart cannot be looked upon as having the same powers as where it does not exist, other things being equal, but at the same time the mere presence of a murmur need not of itself be the signal for rigid curtailment of all activities unless there is also evidence of limitation of function.

Two features, as disclosed by auscultation, are of great value—shortening of the second pause, and so approximating in point of time the second and first sounds, or a gallop rhythm. Either of these conditions must be regarded as evidence of cardiac failure, actual or impending.

March 20th, 1911.

Local Tuberculosis from the Bacillus of Bovine Tubercle.—Heuser, after calling attention to the incompleteness of all extant case reports of this character, describes a personal case which is unobjectionable from any point of view. The patient was a slaughterer, who injured a finger while dressing the carcase of a sick cow. A lesion developed *in situ* severe enough to prevent the man from working at his trade. A lymphadenitis developed in the axilla. When the patient first came to consultation he was also found to have a chronic lymphangitis of the arm, with here and there a nodule. The primary lesion showed all the properties of a tuberculosis verrucosa cutis. General condition quite normal. The tumours along the thickened lymphatics were extirpated or destroyed *in situ* with the cautery. Bacteriological studies and animal inoculations showed conclusively the presence of the bacillus of bovine tuberculosis. Reaction to human tuberculin was negative.—*Med. Rec.*, vol. lxxix, No. 9.

ACUTE PNEUMONIA IN CHILDREN.

A Clinical Lecture delivered at the Polyclinic.

By J. PORTER PARKINSON, M.D.

GENTLEMEN,—In the short time at my disposal I propose to limit myself chiefly to the diagnosis and treatment of acute pneumonia, based on fifteen years' experience as physician at a children's hospital, and especially upon one hundred cases treated there by me during the last year.

Statistics show that broncho-pneumonia is by far the most frequent form of pneumonia. It is usually divided into two varieties—primary and secondary; but the first is considered by many to be the same as lobar pneumonia, only differing slightly from that disease as seen in the adult owing to the peculiarities of structure of the lung in the young child. These peculiarities allow very slight causes to produce local defects of expansion in the lungs, and predispose to collapse, catarrhal inflammation and consolidation. At least one third of the cases of pneumonia in children under three years is of the primary type.

Secondary broncho-pneumonia.—I need not describe in detail the symptoms of this well-known disease, but the physical signs differ considerably, and it is worth while to spend a few minutes on them. The patches of consolidation vary in size and position, and may lie too deeply below the surface to give physical signs, or they may mass together into large portions of solid lung. Then, too, these areas are frequently surrounded by collapsed or over-distended lung, so it is not surprising if the signs vary considerably.

For practical purposes they may be divided into three classes:

(1) Where they are those of bronchitis only. Here the rapid breathing and pulse and high fever will, as a rule, make the physician suspect broncho-pneumonia.

(2) Where the patches of consolidation give signs. Among the general râles of bronchitis there will be found areas where there are numerous inspiratory crepitations with a sharp, clear, ringing quality which is very suggestive; bronchial breathing may be superadded, and if the patches are

superficial enough some dulness to percussion may be obtained if it is not masked by surrounding emphysema.

(3) There may be signs of consolidation over a large area of one or both lungs. Here the diagnosis is clear in the majority of cases.

The physical signs vary greatly from time to time, partly owing to fresh additions of broncho-pneumonia or its disappearance, but largely to variations in the accompanying collapse, especially in rickety weakly infants. Broncho-pneumonia is very variable in its onset and course. The acute congestive form is usually seen in very young infants, and may lead to death in twenty-four hours or so before the physical signs are at all definite. The onset is sudden, with high fever and great prostration, cyanosis, and rapid breathing. There are severe cerebral symptoms. The signs are few and inconclusive; breath-sounds are weak on one side of the chest, or may be unusually harsh, or there may be râles. There is no dulness except from collapse. The diagnosis is rarely made during life, an acute specific fever or meningitis being suspected, and even the post-mortem evidence is impossible without microscopical examination.

In the prolonged type, between periods of high fever there are intervals when the temperature is normal or nearly so, but the fever returns accompanied by fresh chest signs. Sometimes the fever abates while the consolidation remains, and after a considerable time a fresh attack comes on, which may be fatal. Occasionally, however, cases which last as long as three months recover.

Primary broncho-pneumonia.—I need not trouble you with an ordinary account of this disease, but will point out certain difficulties which we all encounter from time to time. It must be remembered that the consolidation is as frequent at the apex as at the base, so the whole chest should be carefully examined, not omitting the axillary region. There are some cases, however, where the physical signs are absent, either at the commencement or during the whole course. The diagnosis is then a matter of symptoms: if these are of a pronounced nervous type meningitis may be suspected. Indeed, convulsions, severe headache, squint, head-retraction, etc., are not uncommon. In pneumonia the onset is more sudden, the temperature higher, and the respirations more frequent than in meningitis. In doubtful cases lumbar puncture may be done, and the fluid examined for albumen, cells, and organisms.

Voisin and Nobecourt have published the following results of lumbar puncture in children suffering from pneumonia:

(1) In cases without nervous symptoms the fluid was scanty, clear, and sterile. Occasionally a trace of albumen was present and a few lymphocytes.

(2) In twenty-four cases with cerebral symptoms the liquid was more abundant (10 to 35 cm.), generally clear, and in only eight cases was albumen found. In half the cases there were no cells, in the other half lymphocytes and polymorphs were found in about equal numbers; all but two were sterile; these contained pneumococci, and there was purulent œdema of the brain.

These results coincide with my own experience that reliance may be placed on this method of diagnosis.

I need not go into detail into the diagnosis between pneumonia and pleural effusion, but it must be remembered that over pleural effusion in children the breath-sounds may be very loud and bronchial, but the percussion note is much duller and flatter, and pulmonary râles are not conducted through a pleural effusion.

I have seen otitis media complicating pneumonia cause prolonged fever and cerebral symptoms, which, in the absence of ear discharge, has been difficult to explain; in doubtful cases the tympanic membrane should be examined, and, if necessary, punctured.

Without going into all the complications of pneumonia, two must be mentioned which are frequent, viz. empyema and pericarditis. The former may be suspected if the fever lasts an unusual time and becomes more irregular, or rises again after a period of apyrexia. It causes a great increase of dulness in the chest. Pericarditis is easily overlooked if examination of the chest be not frequently made, and the purulent form may show no sign of its presence.

About 25 per cent. of cases of pneumococcal pneumonia show protracted pyrexia; such cases can be classified under the following headings:

(1) Those that do not differ from the ordinary except in duration.

(2) Where pneumonia spreads in the lung from one focus to another. In between the attacks the temperature may be normal; this variety is most

often seen in connection with whooping cough or measles.

(3) Protracted pyrexia with imperfect resolution or persistence of physical signs in the chest. The temperature is often irregular or intermittent.

(4) Occasionally there are other bacteriological complications, such as streptococcal infections. These usually arise early and cause the death of the patient in a short time.

(5) Toxæmic complications such as enteritis, acute nephritis, etc.

In considering the treatment of acute pneumonia, it must be remembered that we are dealing with a general systemic infection by one or more micro-organisms, in which the organs of the body in general are involved in addition to the local disease in the lungs. The pulmonary lesion may be of secondary importance to the general infection; so in treatment we should try to combat the toxæmia and get rid of the poisons by elimination. Serums and vaccine therapy have been tried, but, as a rule, with but little advantage; the only cases in which I have found the latter to be useful are relapsing pneumonia, or empyæmata which refuse to heal by long drainage; but it is difficult to be sure that the good effect is due to the injection.

The poison of pneumonia is said to consist of a toxic albuminate which tends to paralyse the vasomotor centre, thus diminishing the supply of blood to the heart; the blood accumulates more and more in the splanchnic area till the intra-cardiac pressure becomes so reduced that the muscle ceases to contract.

That some poison is present in the blood of a patient suffering from pneumonia has been shown in the following way. The heart of a turtle placed in saline solution and fed with normal human blood will contract almost indefinitely, but fed with blood from a pneumonic patient the heart ceases to contract in a few minutes. It follows from this that attempts should be made to eliminate the poisons and to counteract them.

A dose of calomel should be given at the outset. The action of the skin may be increased by sponging with warm water, and water may be given freely to help diuresis. The best drug to counteract the vasomotor paralysis due to the poison is caffeine. There is no other vaso-contractor known whose effect is so certain or so lasting. I advise the sodio-salicylate which is made by dissolving 20 grains of caffeine and 17½ grains of salicylate of soda in 1 drachm of distilled water. One to six minims of this may be injected or given by the mouth every four hours. The vaso-dilatation due to the action of the poison on the nervous centres may be estimated by taking frequent observations of the blood-pressure by the sphygmo manometer.

As a rule there is a rise of blood-pressure during the acme of the fever, and a rapid diminution at, or just preceding, the crisis. A rapid fall in the course of pneumonia may afford an early indication of heart weakness, and so is a valuable indication for treatment. Here is a table showing the normal systolic blood-pressure in healthy children at different ages:

Years.	Minimum.	Maximum.
2 to 3	70 mm.	80 mm.
4 „ 8	75 „	90 „
8 „ 12	85 „	95 „
12 „ 15	90 „	105 „
16 „ 17	95 „	120 „

Cardiac failure in pneumonia may be due to one of three causes: Anatomical changes in the heart; functional disability of the heart with or without anatomical change; vascular paralysis due to nervous influence.

Experiments on animals show that the latter is the chief cause of death, and observations on children show that caffeine gives the most lasting results in raising the blood-pressure, its effect being evident for at least two hours. Adrenalin or pituitary extract have a very evanescent effect, and camphor, though more lasting, is not as certain as caffeine. Cold air has the effect of raising the blood-pressure, and cases of acute pneumonia have recently been treated in the United States by the open-air method, the bed being placed on a balcony in the open-air even during the winter months, heat being kept up by plenty of bed-covering and hot bottles. Statistics seem to show that the symptoms and course of the disease are favourably influenced thereby, but it seems unlikely that this spartan method will come into general use at present; still, it is important to remember that abundance of fresh air is indicated.

Local constriction of the vessels of the lung may be produced by cold thoracic packs. These may be renewed each quarter or half an hour for two or three applications.

Digitalis, owing to the slowness of its action, is of more service in preventing cardiac failure than in treating it when it has come on; in cases where heart weakness is known to exist it should be given from the commencement, and is then a useful drug.

With regard to the general treatment of the disease, the child should be put to bed, and frequently moved from side to side to allow collapsed areas of lung to expand, for the same reason a fit of crying now and then is useful. The room should be well ventilated so that oxygen is given in a natural way, though oxygen from a cylinder may be useful in cardiac failure with cyanosis. Bronchitis has to be treated on the ordinary lines, and if there be much secretion an emetic may give much relief. Ipecacuanha often fails to produce vomiting, and a small dose of apomorphine is quite safe and efficacious. Ipecacuanha is much too frequently prescribed, for it is useless where there is no bronchitis, and where there is, it is by no means sure that it acts favourably. It is also certain that it upsets the digestion; it is better, therefore, if the child seems to be getting rid of the bronchial secretion without drugs not to give them unless there is definite indication for their employment. A hot mustard bath is often useful in cases of collapse in young infants; it also relieves congestion of the lungs and heart by dilating the peripheral arterioles as well as helping the action of the skin.

Disinfection of the mouth and teeth should be carefully carried out, to prevent further infection of the lungs.

Alcohol is not necessary as a routine treatment, but may be very useful, especially in long continued broncho-pneumonias, and in cardiac failure. Brandy well matured has an action far superior to that possessed by the cheaper varieties.

The feeding in acute pneumonia is of the greatest importance, and one generally notices that as long as a child takes its food all goes well; one of the earliest signs that cause anxiety is failure of the appetite. An important rule is, more water and less food than in health.

It has been found from examination of the digestive ferments in children suffering from acute disease that the peptonising power of the stomach and pancreas is very weak. Emulsifying powers are weak or absent. The greatest activity is in the diastatic power of the pancreas. The practical inference of this is that the food should consist chiefly of carbohydrates, such as starches and sugars; fats should be given in very small amounts and proteids may be pre-digested. If curds are passed in the stools the milk should be diluted or peptonised.

[Dr. Parkinson concluded by discussing the treatment of the complications.]

March 20th, 1911.

ON THE

TREATMENT OF CANCER OF THE RECTUM.

A DISCUSSION* BY

Mr. Swinford Edwards. Mr. Godlee.
Sir Frederick Eve. Mr. F. C. Wallis.
Mr. Waterhouse. Mr. Lockhart Mummery.
Mr. Boyce Barrow. Mr. Fagge.
Mr. Sampson Handley. Mr. Gordon Watson.

(*Continued from p.* 368.)

Mr. A. Boyce Barrow said he had intended to bring some patients, but they had disappointed him. His object in bringing them would have been to demonstrate how a colostomy worked, and how comfortable it was to the patient. The patient got used to it, so that he came to almost disregard its presence. He had interviewed recently a man on whom he had operated three years ago, and he said he had kept very well, and had had no great discomfort. The patient reminded him that he (Mr. Barrow) had told him to take castor oil if he had trouble, but he had only to take three spoonfuls of that oil in three years, for his bowels acted twice a day, and he had no surprises at all. One patient, who had been unable to come, was operated upon by Mr. Barrow eight and a half years ago—September, 1902. He said he had kept fairly well and led a regular life; he could cycle and walk as well as of yore. He met that man after he had ridden thirty miles on his bicycle which showed he was not much inconvenienced thereby. He was an enthusiastic advocate for colostomy in association with any operation for the removal of cancer of the rectum. He began operating for cancer of the rectum through the anus many years ago; he believed he commenced in 1880. He used to operate through the rectum, and the results were not satisfactory. The rectum became contracted, and he had to do colostomy afterwards. It occurred to him that it would be wise to do a colostomy first, and so he established a colostomy opening first, as a preliminary to the operation, still removing the disease *per anum*. After a little time he did the parasacral operation, but always with a colostomy first, and the para-sacral operation he found a highly satisfactory proceeding;

* At the Royal Society of Medicine.

but, of course, he left a blind piece of bowel above, between the colostomy opening and where the disease was removed through the sacrum. That always required washing out. It must be twenty years since he recommended the complete operation. The first complete operation he had performed he did in two parts, still doing his old colostomy first, then waiting for a week, and completing the operation by removing the whole diseased bowel, anus included. The patients did well and he was much encouraged. He therefore ventured to do the complete operation at one sitting. He thought he was the first to do that operation, but when he began to read more widely he found that was not so; that the operation had been already described, although then he had not known it. The operation when associated with colostomy was much simplified. There was not much stitching required to fix the colostomy, and, moreover, it enabled the surgeon to proceed immediately with the lower end and remove the disease most completely. He stitched up the upper end, and stitched up the anus, and he could remove the whole diseased bowel without consideration or thought of any junction afterwards. He was comparing the operation, with junction of the bowel, with that when colostomy was done. Another point in favour of colostomy was the fact that it was a much more aseptic proceeding. Once the upper end of the bowel was closed, and the anus closed and plugged, the whole proceeding was aseptic, and the course of the case afterwards was aseptic. He thought the asepticity of the operation, its completeness and its simplicity were very good points, and they made one advocate colostomy in association with the operation; but the patients objected very much to it. He thought, however, that everyone ought to make a sacrifice in order to get rid of a cancerous disease. He did not think many of those present would hesitate to have a colostomy associated with an operation if they were unfortunate enough to have cancer of the rectum, and if the dangers of the operation, and the liability to return, were diminished thereby.

In any operation for cancer, in any situation, there were two elements requiring removal—that which was *recognisable* and that which was *unrecognisable* (the spread or extension of the disease, which might be called the lymphatic extension). Those two features ought to be well considered in any operation done for cancer. An operation which did not take count of both features was behind the times. Everyone would allow that it was the wide removal of glands, etc., which produced so much better results in dealing with the disease—for instance, the removal of the axillary glands in cancer of the breast.

He had said that the abdomino-perinæal operation was a very complete one, and that it was aseptic. He also contended that it was a very safe operation. He could not say he had not had a death, but that death was not exactly attributable to the operation. The fact of the operation being aseptic robbed it of many dangers; but it would be admitted that the greatest danger was that it was a long operation and a severe one, and so liable to cause much shock. Still, he had not found that shock. One of the greatest causes of shock was hæmorrhage, and in no operation was the hæmorrhage more under control than in the abdominal part of this operation for the removal of the rectum. In the abdominal part of the operation the vessels could be ligatured before they were divided. In the second part of the operation he trussed the patient up into the lithotomy position, and sat down and cut from the central point of the perinæum to the coccyx. He did not find it necessary to take out the coccyx. He cleared out the whole pelvis behind the bladder and prostate, and it was surprising how little bleeding there was. The inferior hæmorrhoidals probably might bleed a little, but it could be easily controlled, and the condition of the vessels in the abdomen had so restricted the bleeding that there was scarcely any in the perinæal part of the operation.

He regarded the anæsthetic as the great element in causing shock, especially in very prolonged operations. Possibly some of those present might dissent from that view, alleging that the anæsthetic was given to prevent shock. If the anæsthetic was given very sparingly there would be very little shock, and for a long operation it was desirable to give the anæsthetic quite sparingly. He had put that matter to the test. He operated upon a patient, who was a very feeble person, for cancer of the stomach. It was found desirable to do gastro-enterostomy, but he said that if much anæs-

thetic were given him he would die. After the preliminary stages the anæsthetic was stopped, and the operation proceeded with Mr. Barrow talking to him the time. The operation took one and three-quarter hours, because the Y-shaped operation was done. When the junctions were finished he asked the patient if he would like a drink, and at his request one was given to him. Having drunk the glass of water a little more anæsthetic was given, and the abdominal wound was then stitched up. The patient had no shock, and he did perfectly well after the operation. He asked the patient during the operation whether he was hurting him, and he replied that it was nothing of any consequence. He thought the case showed how much shock depended upon the anæsthetic.

Mr. Barrow therefore contended that the abdomino-perinæal operation associated with colostomy was the safest and most satisfactory operation so far attempted for this disease.

Mr. Sampson Handley showed two patients from whom the cancer specimens shown at the meeting were removed. The first was a man who was operated upon on December 1st, 1910. He was a lead-worker, and the operation was done by combined method, with removal of the anus and its muscles, and the man had now a permanent colostomy opening. It was acting about twice a day. This was a lateral colotomy. He stabbed through the abdominal wall, so as to make only a small opening, a method which obviated subsequent weakness and bulging of the colostomy scar. The other patient was a lady upon whom excision of the rectum was done two years ago. She was now very well, and was able to travel about; in fact, she had since been to the continent several times. The bowels were opened once or twice a day. Mr. Handley continued as follows:

Two of the most respected surgeons of the day consider that it is sufficient in rectal cancer to remove the growth as seen by the naked eye, together with an inch margin of the apparently healthy mucosa above and below. That is to say, they perform an operation which would suggest itself by the light of Nature to a surgeon entirely unacquainted with the pathology of carcinoma. Years ago a similar operation was habitually done in the case of breast cancer; the growth was excised from the breast, and the axillary glands were left untouched. It is strange to remember that as late as 1887, when Banks advocated the routine removal of the glands in breast cancer before the Harveian Society, only a single speaker supported him. Step by step, as pathological knowledge has increased, the scope of the operation has gradually widened in some directions and restricted itself in others, until the modern operation for breast cancer described by a surgeon with equal vigour and inaccuracy as "removing all but the ribs and crippling the patient for life," has established itself. And at each step of this progress there has been a marked improvement in results, so that local recurrence in the field of operation is now a rare event, and functional disability is practically absent.

In the operative treatment of rectal carcinoma we may anticipate with confidence a similar process of evolution. The necessity of removing a considerable length of bowel is shown not only by my own researches, but by the clinical results of Mr. Swinford Edwards, which are exactly in accordance with my observations. In all his cases of recurrence with one exception the growth returned locally at the level of the bowel section. It would be difficult to bring forward a stronger argument for the removal of an increased length of bowel both above and below.

Mr. Harrison Cripps argues that my observations on the spread of rectal cancer must be incorrect, because recurrence, when it takes place, involves only a small area at the site of operation. He implied that if permeation is so extensive the obvious recurrence must be equally extensive. To this I would reply that I have seen a primary sigmoid growth extending along the wall of the bowel nearly to the splenic flexure. I show a specimen of rectal cancer with an isolated oval patch of secondary growth extending up the bowel 3 in. above the upper border of the primary growth. As a rule, however, the obvious recurrence is very local for two reasons, first, because permeation is a slow process which only produces naked-eye phenomena after a considerable period. It is only in those regions where permeation has been long present, that is to say, in the region close to the primary growth, that naked-eye recurrence is to be expected. Secondly, the area of the obvious recurrence is reduced by shortening of the affected length of bowel owing to contraction of the growth

in a longitudinal direction. To remove an adequate length of bowel, to control hæmorrhage at the beginning of the operation, and above all, for the careful removal with visual aid of the fat and glands of the meso-rectum, the perinæal and sacral operations must be replaced by the abdomino-perinæal methods.

The attempt to preserve the natural anus by bringing down the cut end of the sigmoid leads, as continental experience has shown, to a largely increased operation mortality. Moreover, it involves the risk of recurrence in the anal region—a calamity which took place in the last case I operated upon by the perinæal method. For these reasons, speaking generally, I advocate an operation on the lines described last year by Mr. Miles, and I believe his recent paper will lead to a great advance in the treatment of rectal cancer in this country. I have myself practised a similar operation for two years, and in three cases. All of these are living and have no signs of recurrence. Had the operation been the dangerous adventure depicted by Mr. Harrison Cripps I can hardly believe that fortune would allow me an initial sequence of successes. Gonlliond has published a series of eleven cases in women, all of whom recovered.

It is necessary to recognise that the radical abdomino-perinæal operation is a severe one, and that its mortality is higher in men than in women. In old or feeble patients of either sex, but more especially in men, I should still employ a limited perinæal operation if the growth were situated low down, or should content myself with a colotomy.

Mr. Harrison Cripps has stated in reference to a case of mine operated upon by the combined method that according to my own statement I had done a perfectly useless operation, because, on examining the specimen, I found evidence of permeation of the bowel as far as the line of section. I am quite ready to admit that I failed to obtain the surgical ideal of complete removal, but my operation secured a better approximation to it than the limited operation which Mr. Cripps advocates, and in that way I believe that I minimised, so far as was possible in that particular case, the chance of recurrence.

Surgeons often claim far too much credit for their successes in dealing with cancer. An absolutely clean removal of every cancer-cell present in the body is probably a rare event. Freedom from recurrence is common because natural processes of cure destroy or inhibit the few remaining cancer-cells. These facts explain the not inconsiderable amount of success attainable by very incomplete operations. They in no way justify the abandonment of the surgical ideal of complete removal, for experience has shown, more especially in breast cancer, that the more extensive operations, if guided by pathological knowledge, produce greatly improved results. The ideal may be unattainable, but the struggle to reach it finds its full reward. Mr. Harrison Cripps's results have been attained by a very rigid selection of cases. I gather that only in one case in every three or four seen by him does he advise excision. The vast majority are left not only with their disease, but often, I presume, with the colotomy opening which Mr. Cripps so much deprecates in his criticism of Mr. Miles's and my own methods. One advantage of the combined methods is that they widen the limit of operability and enable us to aim more often at cure instead of merely at palliation. If the perinæal method is applicable only to one case in four of rectal cancer, surely it cannot be the ultimate resource of surgery. It is the duty of surgeons, in dealing with rectal cancer, to advance along the lines laid down by pathology so far as they are able without subjecting the patient to undue risk.

It is, of course, to be much regretted that a reasonably safe, and at the same time an adequate, operation can only be done at the price of a colostomy opening. But the disability, though real, is not inconsistent with a normal life of work and social intercourse. The methods which aim at the restoration of the normal function of the anus are only successful in a small percentage of cases. The ideal result of these cases is dearly bought at the cost of many operative fatalities, or at the cost of preventable recurrences. Other cases remain with a permanent sacral anus, or suffer from incontinence or stricture. If the surgery of rectal cancer is to advance, the ideal of restoring anal function must be abandoned as inconsistent with the other and far more important ideal of complete and safe removal of the disease on the lines laid down by pathology.

It must, however, be admitted that the pathology of rectal cancer is not yet fully worked out. For some years I have been endeavouring to trace the

spread of cancer of the rectum, and until recently without success. Owing to the proneness of the carcinoma-cells to mucoid degeneration and the consequent difficulty of recognising them in microscopic sections, about two years ago it occurred to me to employ muci-carmine, a specific stain for mucus, and by the aid of this stain I thought I was able to demonstrate the spread of permeation along the mucous lymphatic plexus of the bowel at a distance of six inches from the primary growth. Mr. Cripps has no doubt that I was mistaken. I am able to-night to demonstrate another case showing similar appearances, extending to a distance of four inches from the obvious or naked-eye edge of the growth. It rests with Mr. Cripps to explain what these appearances are, if they are not those of permeation of the mucous lymphatic plexus by cancer-cells. I have been unable to find anything similar in a small series of normal rectums. Though an occasional isolated element may take on the muci-carmine stain, nothing can be seen in the normal bowel in the least comparable to the small groups of sharply defined mucoid cells, irregular in size and shape, which may occur at frequent intervals in the cancerous rectum to a variable distance from the macroscopic edge of the growth in the plane of the mucous lymphatic plexus. Beyond a certain point they cease to be visible, and that point is, in my opinion, the true microscopic growing edge of a rectal carcinoma, within which the surgeon's knife must not willingly intrude.

Mr. Rickman J. Godlee asked Mr. Handley at what distance from the original growth the last section shown was taken. Also how far from the original growth had he found cancerous cells?

Mr. Sampson Handley said the section was taken from fairly near the growth because, wishing to demonstrate the process clearly, he took one of the best fields he could. In one case he had found scattered foci 6 in. from the growth, and in a second case 4 in. away.

Mr. F. C. Wallis said that during the last ten years the surgery of the rectum had made great progress, and this was more particularly the case in other countries. But in this country also in the last few years a much more intelligent interest had been taken in this branch of surgery by practitioners at large, who more and more appreciated what a valuable asset a proper knowledge was of this subject, and what before were looked upon as more or less mysteries were now treated in a rational manner, and thus the relief of pain for the patient was much earlier and of a more practical character. The first essential for good results was early diagnosis, and that could only be obtained by a proper examination being made at the earliest moment the symptoms were complained of, instead of the whole thing being shirked as long as possible by both practitioner and patient. This was a grave responsibility which rested with the practitioner, and it behoved those who had the opportunity of early examination to do this as early and as thoroughly as possible. If this was made a routine practice the diagnosis would be made much earlier, and so the results of the operation would be greatly improved. Before deciding if any operation shall be done in cases of rectal carcinoma, one of the most important matters to decide was whether the growth was a fixed one or not. This decision, as a rule, should not be made until the patient had been examined under an anæsthetic, and even then the decision might be of some difficulty, especially if the growth was adherent in front to the bladder, vagina, or uterus. In such cases the growth might easily be pulled down with the viscus to which it was adherent, and thus give a false idea of free mobility. When any such possibility as this occurred during diagnosis, it would be obviously wrong and might also lead to serious disaster if an operation was undertaken either from the perinæum, or by one of the posterior routes, and when the growth was arrived at it was found to be fixed. Also other parts of the colon and even the small intestines might adhere to the growth. To obviate any such catastrophe, and at the same time to obtain a more thorough knowledge of the growth and its surroundings, an exploratory laparotomy should be made in the mid-line. This was an expediency which he suggested should be made much more use of than was at present the case. The question as to what particular operation should be undertaken was one which was almost acute in its present interest. Any new operation which involved a decided alteration of technique was usually commenced with enthusiasm. The enthusiasts often overdid it, and operated on

all sorts of cases which were in one way or another unfitted for such a procedure, with the result that the operation was discredited by those whose practical experience, having extended over some years, had settled into a groove in which work might run smoothly, but perhaps not so much progress was made, and thus an important advance had often been checked for a considerable time. It was to be sincerely hoped that such would not be the fate of the abdomino-anal operation, which was now so much advocated, and was a good one in suitable cases. The technique of this operation had been thoroughly described, and the main details of it could soon be learnt by one or two operations on the cadaver. But this was the least difficult part of the work. The age of the patient, the character and size of the growth, the situation, particularly with regard to its surroundings, the length of the sigmoid loop, the size of the pelvis, and the amount of fat generally in the abdominal cavity were all matters of clinical importance, which it was essential to consider before undertaking this operation. But it could be truly said that most of these points have to be considered in any operation for a high-lying cancer of the rectum. But it was probably for the low-lying carcinomata that this operation would obtain the best results. The involvement of the lymphatics in the submucous plexus in certain cases of cancer of the rectum was admirably demonstrated by Mr. W. Sampson Handley, but whether this was satisfactory to everybody was not certain. Personally, on reading Mr. Handley's views, and thinking back over his own cases, he was much impressed with the fact that a certain proportion of such cases which had done extremely well had had a return of the growth in the lower segment after an interval of a year or eighteen months. And he (Mr. Wallis) thought that in such a case if the abdomino-anal operation had been performed, and the bowel above the growth more freely removed, there would not have been recurrence. It was for such cases that the abdomino-anal method was desirable, and from which probably the best results would be obtained.

He could not feel enthusiastic as to the ultimate fate of the bigger operation which had been performed—namely, excision of the whole of the rectum and possibly a portion of the sigmoid, with the establishment of an iliac anus. It could only be very few cases in which such a measure was justifiable, and even amongst the few that were justifiable it would hardly be expected that anything but a high percentage of mortality would obtain. When a rectal carcinoma required such severe measures it was probably far better treated by an ordinary colotomy, and he could give various instances of people who had lived comfortably with a colotomy and a carcinoma for many years, the longest he knew of being ten.

With regard to the other operations, he was strongly of opinion that the best method of dealing with rectal growths not involving the sphincters was by the sacro-anal method. The great point which he claimed in its favour was that instead of suturing the last two or three inches of the bowel to that which was brought down, the anal mucous membrane was excised and the cut end of the bowel above the growth brought down and stitched to the anus. This was quite a simple matter, and all possibilities of a sacral anus forming and the almost certainty of a stricture were done away with. This was a method which he had adopted for the last twelve years, and which could be well combined with the abdominal incision when necessary. When the growth involved the sphincters, then a wide excision of these muscles was necessary, and this operation could usually be performed through the perinæum, especially if a preliminary colostomy had been performed, and, indeed, in such cases this was essential.

When a growth was inoperable there was no doubt that great comfort was given to the patient by a colostomy. He (Mr. Wallis) had already mentioned that patients lived in comparative comfort with good general health for many years after this operation had been performed, and he was constrained to mention it again, because he remembered a few years ago, at the Medical Society of London, when a discussion took place on this very subject, that grave doubts were passed upon any possible benefit being derived from such a measure. There was no question as to the immense benefit derived, especially if the toilet of the colon, which was well known now to most good surgical nurses, was daily carried out, and if the growth itself was prevented as much as possible from becoming septic by appropriate treatment.

He could not advocate any of the various fancy operations for colostomy, which in his experience

only caused trouble, and rarely, if ever, possessed those particular virtues which were claimed for the operation. One other point he would suggest in the operation, and that was that the bowel should be opened at the time of the operation. This saved much trouble, was much more comfortable for the patient, and, with ordinary care, was entirely free from all risks. He had no knowledge of any good effects which had been produced by the action of radium upon a rectal carcinoma.

(*To be concluded.*)

Nose and Throat in General Diagnosis.—H. Z. Giffin considers that the patient's general condition affects the mucous membrane of the nose and throat, and that abnormal nasal conditions affect the general health. Acute headaches with fever, no matter to what part of the head the pain may be referred, are often due to sinusitis. Dull, heavy headaches of a more constant and chronic type, sometimes confined to one side of the head, are often due to nasal obstruction or sinusitis. Periodical headaches are generally of nervous origin. The association of headache or nasal obstruction with the onset of the patient's complaint, particularly if the complaint be a periodical one, makes us suspect nasal disorder. The effect of chronic fibroid tonsils upon the recurrence of articular rheumatism and other disease of the rheumatic cycle should be considered. Suppuration in the antrum is sometimes at the basis of certain obscure cases of anæmia. Tumours of the face and neck, including goitre, should have nose and throat examinations for diagnostic purposes. Irritative cough, especially night cough, may be entirely a reflex matter, and sinusitis may be found. Certain forms of asthma are apparently influenced by nasal conditions. In 66 cases of asthma that were operated upon at St. Mary's Hospital, Rochester, during 1909, definite improvement followed in 57 per cent. and cure in about 10 per cent. On tabulating the results one finds that, in general, when the attacks become less frequent they also become less severe. The general health had improved in 33 of the 52 patients. Headache had become a symptom in 31 cases and had improved in 24. Mild stomach symptoms were elicited in 25 and had improved in 20. Of the operations upon the five patients who were "cured," four were turbinectomies and one the removal of polyps. Improvement followed in three of the four cases in children in which adenoids and tonsils were removed. The most unusual things will occasionally cure asthma. —*Med. Rec.*, vol. lxxix, No. 9.

Persistence of Strychnine in a Corpse.—On November 15th, 1909, a man, æt. 53 years, died under suspicious circumstances. The body was buried on November 19th, 1909; on November 29th of the same year it was disinterred, and certain organs removed and sent to the late Franklin G. Robinson for analysis. Prof. Robinson's notes record that he found strychnine, but, owing to illness, he was unable to appear personally at the trial. The body was exhumed a second time on March 18th, 1910, the other organs removed, which were sent to Cram. The body had been frozen most of the time, but the grave when opened was full of water, which was allowed to drain off. An embalming fluid of acid reaction had been used when the body was first buried, which made it appear likely that any strychnine would be dissolved out. The method used for determining strychnine was the usual one of extracting with alcohol made acid with acetic acid, evaporating the extract to soft dryness, extracting the residue with water, extracting the acid water solution with chloroform, and then the same solution made alkaline with chloroform, the strychnine coming out in the last chloroform extract. The strychnine was then purified by absolute alcohol. From 454 grm. of the lung, 133 grm. of kidney, 446 grm. of muscle, 850 grm. of small intestine, and 560 grm. of brain, each being tested separately, no strychnine was obtained. From 803 grm. of liver was obtained 0·0015 grm. of strychnine, and from the spinal cord 0·0033 grm. The spinal cord itself weighed 25 grm. The note-books of Prof. Robinson record that in 290 grm. of stomach contents he found 0·0277 grm. of strychnine, and in 45 grm. of kidney 0·0003 grm. of strychnine. The spinal cord, being better protected from the action of the water in the grave, which was acid from the embalming fluid, had retained the strychnine better than the other parts.—*Journ. A.M.A.*, vol. lvi, No. 9.

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A LECTURE ON BRONCHITIS.*

By J. EDWARD SQUIRE, C.B., M.D., F.R.C.P.,
Senior Physician, Mount Vernon Hospital for Consumption.

LADIES AND GENTLEMEN,—In speaking to you about such a common condition as bronchitis, which in this clime is so familiar to all of us that we get in the way of treating it as we are apt to treat familiar things—being content with recognising the general appearance without criticising details—it is somewhat difficult to make the subject interesting, and more difficult to bring forward anything which is not known to all of you. But it is just because of this familiarity with the disease that I was tempted to select it as my subject; because one knows that the things which we are meeting with every day become so familiar that, recognising the general appearance, there is a tendency to be content with general impressions and not to examine details as we should in the case of something which was strange. Where the diagnosis seems to be obvious there is always the possibility that some modification of usual symptoms may escape notice, and those modifications may occasionally be of very great importance.

In the first place let me say a few words about causation. The importance of studying causation is chiefly that we may learn what to avoid, in other words it directs our attention towards prevention. We should probably satisfy our patients if we were to say that the attack of bronchitis is the result of catching a chill: and of course to a certain extent that is correct. But we must realise that this chill is not the actual cause of the disease. The chill is merely the incident in the case which has broken down the protective power of the mucosa of the bronchial tubes; it is merely the means of laying

* Delivered at the Polyclinic.

the mucous membrane open to microbic invasion. It is evident that, the air-passages being constantly exposed to the outer air, microbes and foreign matter are constantly being breathed in or inhaled, and that some of these materials may stick on to the mucous membrane. Fortunately for us this foreign matter is also as constantly being expelled. When we come to look for the microbe which is the cause of bronchitis, we find ourselves in the same difficulty which we encounter in connection with many other diseases—that we cannot isolate one particular microbe which we can label as the distinct cause of bronchitis in the majority of cases. It is a point which is becoming more and more familiar to us that common diseases, especially those affecting the air-passages and the lungs, may be due to a variety of different micro-organisms. If we were to take as an example inflammation of the lungs—I use that term of set purpose instead of pneumonia, and perhaps a more useful term still would be one which we may borrow from the other side of the Atlantic, namely, pneumonitis—we should find that the pathological condition of the lungs which is familiar to us all in pneumonia may be produced by many other microbes besides the pneumococcus. If you were to take a microscopical section of a lung, with consolidation due to the tubercle bacillus in the early stages of the disease, and a preparation of another lung which had become consolidated in influenza, and compare these under the microscope with the consolidation of pneumonia, you would not be able to tell which was the result of the pneumonic, which of the influenzal, and which of the tuberculous infection. It is when we observe the result of the toxæmia which is produced by one or other of these microbes that we begin to get the distinction between the varieties of infection brought about by the different diseases. The same sort of thing happens in the case of bronchitis. We may get bronchitis from the invasion of the pneumococcus, just as we may get pneumonia from the invasion of the pneumococcus. We may get bronchitis resulting from the invasion of the *Micrococcus catarrhalis*, the microbe which produces the common cold in the head. We may also get it from an ordinary streptococcus (if there is such a thing as an *ordinary* streptococcus), or it may be also due to various other organisms. Speaking of the *Micrococcus catarrhalis*, which is a frequent cause of a common cold in the head, it is worth remembering that a common cold in the head and bronchitis have a close connection one with the other. Frequently, indeed, bronchitis is an extension of an ordinary cold in the head. Of course, we now recognise that the cold in the head is due to infection, but I am afraid that even yet the public does not entirely realise that, and still persists in attributing a cold to some change in the weather, or to the leaving off of a scarf which the individual has been wearing, or something of that sort. There are two observations which are very interesting in this connection, which you may have heard of already. I believe it to be the fact that in the islands off the north of Scotland, which, during the inclement season, are cut off from communication with the mainland—that is to say at just the time of year when the weather would be thought to be most likely to produce colds—the inhabitants of those islands get through without any colds at all; but when the weather breaks and gets better and the boats from the mainland resume their journeys, colds begin again in the seaports and spread over the islands, so that everyone seems to have a cold before the season is far advanced. The other fact I referred to was that in one of the recent Antarctic expeditions, in which I suppose the weather was inclement enough for anything, there were no colds among any of the crew until a bale of clothing which had not been opened since it had been packed on board in England was undone, and then those members of the expedition who unpacked that clothing developed severe colds. Those two facts should, I think, convince the most sceptical that the ordinary cold at least is due to some infection, and not due merely to the weather. We see the same sort of thing closer at home. In the hospital to which I am attached it has happened more than once that we have had to forbid visitors, because there has been a scare of infectious disease, and during the period that visitors were excluded the patients remained free from colds, even if the weather was bad, and even in the case of patients who have spent most of their time on the balconies. As soon as the hospital was reopened to visitors the colds recommenced. We have to remember, then, that bronchitis is due to some microbic infection. My reason for insisting upon this is because it may have some bearing on treatment. When we are asked what the particular germ is, we have to reply, as I have already

said, that there are several germs which may cause an attack of bronchitis. From the point of view of prevention it may be worth while bearing in mind that septic organisms are to be found in the mouths of most people, and especially in those people who do not take sufficient care of their teeth. Septic trouble is very common in people who have not cultivated the use of the tooth-brush. Possibly an attack of bronchitis may, in certain cases, be attributable to some of the organisms from the mouth being inhaled. Another point worthy of note is, that whilst undoubtedly chill may be the determining cause of the attack of bronchitis, in that it opens the road to microbic invasion, we find that the people who are most liable to get colds are not those who live most in the open air and who are most exposed to climatic influences, but on the other hand, those who are accustomed to live in close, over-heated rooms, or at least in ill-ventilated rooms, and who go from such an atmosphere into the cold air outside. In a general way one may say it is those who take most care of themselves who are most liable to get colds. By taking most care of themselves, I mean those who wear too much clothing and wrap themselves up too warmly, and live and sleep in rooms where they will not have the windows open. The open window is one of the preventives of bronchitis. We should accustom ourselves to live as far as possible in a pure atmosphere. Now, since there are a variety of microbes which might produce bronchitis, one is not surprised to find that bronchitis is often a symptomatic disease. We know that the first stage of whooping-cough is apparently nothing more than ordinary bronchitis. We know that bronchitis is one of the commonest concomitants of an attack of measles. Most of us know also that the early stage of enteric fever may be masked by the bronchitis which is associated with it. We are also all familiar with the fact that the tubercle bacillus produces bronchitis, and we may occasionally have dismissed as an attack of bronchitis a condition which turns out afterwards to be tuberculosis of the lung. Everyone knows that in unmistakable tuberculosis bronchitis may be an accident. That is an important matter, because in these days of open-air treatment of tuberculosis of the lung we have to be very careful about those people who are subject to bronchial attacks in the course of their disease. At this time of the year, for example, we find there are patients who have early tuberculosis of the lung who cannot be put into the open-air balconies, or made to sleep out of doors, or even in a ward which is very well ventilated, because of their liability to get bronchitis. Even if the bronchitis does not delay their recovery, it certainly adds considerably to their discomfort while its lasts.

Coming to the evidences of the disease itself: with the ordinary symptoms of a simple attack of bronchitis you are of course thoroughly familiar. The person, after perhaps a few days of an ordinary cold in the head, gets a certain tightness of the chest, some pain behind the upper part of the sternum, which becomes worse on taking a breath, and a cough which is all the more troublesome because it seems to be ineffectual. The amount of expectoration in the initial stage of bronchitis is very slight indeed, or practically none at all. Nevertheless the cough is very insistent. After a couple of days, during which the patient is feeling out of sorts, and the temperature may be raised, the cough becomes looser, the resultant phlegm now being frothy, with perhaps mucus added, in lumps it may be. Two or three days after that the cough again alters, the phlegm being much looser. And now the lumps that come up have a greyish tint, with perhaps black specks in them. The yellowish phlegm has disappeared by now, and in a few days the attack may be over. On examining the chest during the first stage we find the musical râles or rhonchi, dry sounds, high-pitched and whistling, or perhaps deep-toned and roaring. In the later stages we have the large bubbling râles which are easily modified by cough. A point I want to insist upon here is that in the ordinary attack of bronchitis these signs are found throughout the whole chest, both sides being equally affected. It is of very considerable importance to bear this in mind, because if you have an attack of bronchitis in which the physical signs are confined to one side of the chest, to one lung, or to one part of the lung, or in which the signs are patchy, you have probably to do with something which is not ordinary bronchitis. In my experience, one-sided bronchitis is almost always due to tuberculosis: but you must make the reservation, that in influenza you also get those one-sided or patchy attacks of bronchitis. You will remember that the pneumonia of influenza is

very often of that patchy nature; there is a patch of pneumonia at one base; as that is resolving there is another patch starting at the apex of the lower lobe on the opposite side, and when or before that resolves you will find another part of the other lung has become consolidated. Bronchitis is a very common accompaniment of influenza, and, as I have said, the bronchitis may be one-sided or patchy. But in my experience the most common association of one-sided patchy bronchitis is tuberculosis. The nature of the expectoration, as a rule, does not call for any very great amount of attention, except that you will tell fairly well by the character of the expectoration what stage the disease has reached. But there is one character of the expectoration which, when it occurs, requires to be carefully noted, because it is a sign of very extreme danger: I mean the "prune juice" expectoration which sometimes occurs especially in older people towards the end of an attack of bronchitis. That prune juice expectoration nearly always means that the right side of the heart has failed and you should regard it as a sign of urgent danger. That was very forcibly brought to my mind in a way which made a considerable impression upon me, because it was in the person of a close relative. There was an ordinary attack of bronchitis, and the doctor in attendance kept me more or less informed as to what was going on. But this prune juice expectoration showed itself one day, and being looked upon by the medical man as a mere interesting incident and not worthy of particular notice, no note of it was sent to me. The next morning I received a letter from a relative, who was in the house, saying things were going on very nicely indeed, but there had been a curious sort of phlegm brought up, which was described in terms which left no doubt in my mind as to its nature. The effect of this information was to cause me to put my things together immediately and start for a train. Just as I was leaving I had a telegram asking me to come at once, and I reached the patient only ten minutes before the end. That sign not having been given its due importance, I did not get the information at once which might have made some difference, at all events to my peace of mind. That is an indication of one of the common dangers of a severe attack of bronchitis, and particularly in a chronic bronchitic. The right heart gets overtaxed and fails, and if that should happen there is not much time left to apply remedies.

We may pass to the complications of the complaint. As a rule, an ordinary attack, such as I have spoken of, passes over in a few days, and in a fairly strong adult practically nothing is needed in the way of treatment, and the disease gets well without there being complications. But when bronchitis occurs in a very young individual, or on the other hand, in one who is very old, the disease is one of very considerable gravity, and complications of some kind are almost sure to occur if the strength of the patient enables him or her to stand the disease long enough to allow of complications. In very young children, the two risks which seem to me to be the most important are, first of all, that the disease may spread to the smallest tubes, so causing capillary bronchitis, and secondly, that it may pass to the lung tissue or alveoli and produce broncho-pneumonia. In older persons the chief danger to be looked for is from the heart, and I would say that the treatment of bronchitis in the old person is the treatment of the heart condition rather than of the lung condition. In young persons the dangers that follow the disease are chiefly the result of the straining and the cough. The ribs may be slightly pulled in, but that is not so likely as with the spasmodic cough of whooping-cough. What you get in young children after a few attacks of bronchitis is distension and dilatation of the tubes—bronchiectasis. We know that the mere straining of a cough will not distend the bronchi. There must be first of all some disorganisation of the bronchial mucous membrane and the walls of the tube before there is distension from the strain of even a severe cough. In bronchitis there is this alteration of the mucous membrane which may cause sufficient disorganisation to allow of distension of the tubes. There may be, resulting from bronchitis, an amount of distension of these tubes in a young person which may lead to the belief, when examining the person later on, that he has had an attack of pulmonary tuberculosis which has caused excavation and left him with large cavities. Remember, then, that there may be considerable cavities as a result of bronchietasis from attacks of bronchitis in young children. In the older people the risk is rather in causing emphysema, which hampers the individual considerably for the remainder of life.

We will now turn to the question of treatment. I have already told you that bronchitis is due to microbial invasion. And one would naturally infer from that that antiseptic inhalations would be of some value, if not in preventing, at least in curing the disease. I do not know that experience will bear out theory there. I do not think antiseptic inhalations have any great curative effect on bronchitis when the bronchitis is once established. A treatment which has been employed with some success is making a vaccine from cultures derived from the person's own phlegm, and inoculating the patient with this. That method has been used as a curative means, and also as a preventive, and I think it is worth a little further consideration. The actual treatment of an attack of bronchitis by a special vaccine does not appear to me to be any more rapid in its action than the treatment that we used to adopt before we thought so much about antitoxins. But in the case of people who have become specially liable to bronchitis, and who are constantly having attacks of bronchitis, it has been found that a series of inoculations may have a protective effect which is extremely valuable. There are cases, for example, where people have been for years obliged to go abroad for a change of climate in the spring and autumn, but who, after the use of these vaccines, have been able to withstand the changes of climate in England and continue their work. So it is reasonable to infer that they must have been protected against their attacks of bronchitis by being inoculated for some weeks or months with the particular toxin which generally caused their bronchitis. In these cases it is necessary always to find out which particular micro-organism is the one which general affects a given individual. That can only be done by examining the sputum or the blood as the case may be, and finding which organism is present. In other words, you must make the vaccine for each case. That, of course, is more than we can do for everybody, but in certain instances it may be a thing which is worth consideration.

Now with regard to the present treatment of an attack of bronchitis, the first thing that I think is essential for an acute or severe attack is two or three days' rest. You should get the person to spend two or three days at home in a warm atmosphere; the important matter is that the air in which he lives should be always of the same temperature. At the same time some warm drinks, not necessarily alcoholic, should be given occasionally, and if you do this you can generally get him through the attack without the slightest difficulty and without physic. In older people and in young children the first signs of an attack should lead you at once to order the person to bed and make him stay there for a few days. When we come to remedies, the first thing which seems to come before one's mind prominently in nearly every case is the bronchitis kettle. I heard it said years ago that more people are killed by bronchitis kettles than by the bronchitis. And although that, like many other statements, is beyond what we can substantiate, I feel certain that in many cases the bronchitis kettle is a mistake. The moist atmosphere may be soothing to the dry mucous membrane in the early stage of the bronchitis, and so help the onset of the expectoration, which gives much relief to the patient. But to keep the atmosphere of the room saturated with moisture is, I am convinced, in many cases not an advisable procedure, and I certainly would not put down the bronchitis kettle as being a remedy for every case. I am disposed to think that in many cases if you can keep the atmosphere dry you will do much more for your patient. Still, there are a few cases where the bronchitis kettle gives such relief that it may be employed. I wish, however, to express a warning against the use of it in all cases. In ordinary simple cases all that is wanted is the external application of some counter-irritant, which I believe to be of the very greatest value, and internally some simple expectorant. If you can get the person to perspire thoroughly at the beginning it seems to help matters and to cut the complaint short, or, at all events, to diminish its severity. It has been suggested—and I think it is not a bad plan—that a person, when commencing to have an attack of bronchitis, should have an extremely hot bath before the fire in his room, have the bed warmed and ready for him, and, having practically boiled himself in this hot bath, to get quickly dry and into bed, afterwards taking a hot drink, and remaining in bed for the following twenty-four hours. To take a hot bath in the ordinary bathroom, and then walk through a cold corridor or through cold passages to bed is unreasonable, and may do no

good. Worse still is it to take a Turkish bath perhaps a mile away from home, and then come home to bed. It is the height of absurdity to do this. With regard to the counter-irritants, which are so useful, the one I chiefly use is the acetic turpentine liniment (or you may use Elliman's embrocation). Later on you may apply poultices, not necessarily counter-irritant; the ordinary linseed poultice is not only soothing, but I think also useful. It is one of the old remedies, but I think we cannot ignore it in our treatment, even at the present day. Of course a little mustard added to that makes it a counter-irritant. Still, I do not think the mustard is wanted after the first stage. If the mucous membrane is dry, and the discomfort behind the sternum is sufficient to require special treatment, the inhalation of some compound tincture of benzoin in steam is soothing and comforting and apparently useful. You use a teaspoonful of Friar's balsam to a pint of boiling water, and the steam of that is inhaled for five minutes night and morning. Small doses of iodide of potassium (one to three grains) are often most useful in loosening the phlegm and aiding its expulsion in older patients, especially in those with high arterial tension. Another simple remedy which I regard as very useful is the morphine and ipecacuanha lozenge of the Pharmacopœia. There is in it a small amount of sedative with some expectorant. I use it for what may be called the ambulant cases, for the person who is not obliged to lay up altogether, but who is still bothered with his cough. If he carries one or two of these lozenges about with him broken up into small pieces and when the cough is troublesome he takes a small piece and swallows it, he will not require more than two or three lozenges in twenty-four hours and yet the cough is kept in subjection. These lozenges are also very useful to check the cough during the night. This brings me to a point which I think requires some emphasis, and that is that sedatives may be extremely dangerous in bronchitis. There is no easier thing than to stop a cough, and perhaps there is no greater temptation than to do so. A person comes to you saying he is worried by a cough; that is the only thing which troubles him and he wants to be rid of it. It is a great temptation to you to try to stop that cough, the only question being how much sedative it will take to do it. But when we remember that the cough is Nature's way of trying to get rid of foreign substances we have to hesitate before we give sedatives which will permit of the accumulation of these foreign matters in the air-tubes. The cough at an early stage of bronchitis, where it is a dry cough, is a reflex cough, and may be quieted by a little sedative medicine. But as soon as there is any sign of any expectoration, the amount of expectoration should be increased and the cough encouraged rather than checked. Though we remember that a cough is Nature's effort to get rid of the phlegm, we know that Nature is not always infallible in her judgments. Just as a man who has had a leg amputated will have persistence of feeling referred to the toes, although he knows there are no toes to feel in, and this feeling is not altered until the brain gets used to referring the sensation to the proper place, namely, to the stump, so when there is increased vascularity in the mucous membrane, the sensations will be attributed by our mental processes to some irritant on the mucous membrane which we try to get rid of by coughing. Here, of course, it is much better to soothe the irritations by some sedative and diminish the cough, which is increasing the mischief and doing no good. But later on, when there is something to expel, our aim should be, as I have said, to increase the efficacy of the cough, and that can be done by giving expectorants. As to what particular expectorants should be given, we can very well leave that to the individual judgment of the practitioner. Some of us find one particular form is always useful, whereas others say they have used that and it has produced no particular good effect, just as some people say lobelia, for example, is a most useful drug under certain circumstances, whereas others will say they have not seen the slightest therapeutic action from it. I think most of us will agree that ipecacuanha is a useful expectorant and it is the one which most of us try first of all, and I think generally its use is attended with success. When the tubes get choked, then, of course, the difficulty is as to how to get them free again. Your expectorants may help, but any choking of the tubes adds to the danger of the patient and certainly adds to his discomforts and to his shortness of breath. Therefore it is best to endeavour to prevent the tubes becoming choked. We must remember that if the cough is at all per-

sistent the patient may be worn out by not having sufficient sleep at night. Though we give expectorants to try to make him cough, the patient cannot continue coughing night and day. So that though you may give expectorants during the day you must give a sedative at night, to provide the patient with proper rest. You will find a hot drink at night, even if is only warm water, but still better, hot milk—and sometimes hot whiskey will be preferred—will do much towards providing a reasonable night's rest. You should also have a sedative close at hand, so that if the hot drink is not effectual the sedative may be given. But you have also to remember that if you do that you are doing it at the expense of considerable difficulty next morning. You must therefore encourage the cough first thing in the morning when the patient wakes up, so as to get rid of the night's accumulation in the tubes. Another point which has probably occurred to many of us is that there are certain individuals who tend to get their tubes choked when there is much secretion because of their lethargic temperament. You often find very stout individuals will allow a considerable amount of secretion to collect in their bronchial tubes without making the slightest effort to get rid of it, an amount which a thin person would not tolerate at all. In my experience, very stout people are difficult to manage in their attacks of bronchitis because of their allowing the secretion to remain in the tubes. Stout people are apt to be lethargic in their movements and to take things easily—perhaps that is the reason they have become so stout—and when they are ill we notice the same peculiarity: they take a long time before they recognise that they are ill, and they stand a large amount of irritation, whether in the tubes or anywhere else, without taking any steps about it; similarly when they are well or nearly so they take some time in realising the fact. This sort of person wants assistance in getting rid of the secretion. Therefore, remember that for stout people and young children sedatives are dangerous in attacks of bronchitis. One thing which there has been a great deal of difference of opinion about is whether inhalation of oxygen is good in acute bronchitis. Personally I think it is; in severe cases I have seen advantage from having a cylinder of oxygen handy. An occasional whiff of oxygen will at all events make the patient feel as if he is getting enough air. When a person with bronchitis begins to get blue because of his difficulty in getting enough air into the lungs or because he finds difficulty in pumping the blood through the lungs, the occasional administration of oxygen seems to bring great comfort. I think beyond that it acts as a stimulant and helps recovery. It seems to me to tide the patient over an acute crisis and to give him a further chance of recovery. Certainly I do not agree with those who say that the inhalation of oxygen is useless in acute bronchitis. I have said that sedatives should be avoided as far as possible in the case of young children and phlegmatic people. Those cases in which there is kidney disease or heart disease are also instances in which sedatives must be used with great care.

One last word, and that is that the chronic bronchitic may easily become infected with the tubercle bacillus when he is living in a tubercle-bacillus-laden atmosphere such as that of London. In large towns it is important to remember that many of the chronic bronchitics who are going about are also tuberculous. The person has been bronchitic or "asthmatic" for a number of years; he has been short of breath for a long time, and has also had a cough whenever there has been a change of weather. If he gets some addition to these discomforts as a result of tubercle bacilli settling in his lungs, it only means that the cough becomes worse, and the shortness of breath is a little worse, and that all his symptoms may be a little increased. But the patient has become so accustomed to having these conditions that he expects them to get worse as he grows older. And unfortunately in many cases the doctor agrees to that proposition, and the fact that there is this extra infection is overlooked. I do not think it matters very much to the patient whether it is overlooked or not: no amount of treatment will make very much difference; these cases remain tuberculous. In my experience the onset of tuberculosis does not make any material difference to the progress of the chronic bronchitis. The lungs have already become somewhat fibroid by the repeated attacks of bronchitis, and the spread of the tuberculosis is extremely slow; but though it will not heal, neither will it spread much. The main reason why it is important to recognise that these chronic bronchitics may have in addition tuberculosis is that many of them cough up a good deal of phlegm which contains tubercle bacilli, and if the danger

is not recognised there are no precautions taken to properly dispose of the phlegm, so that it shall not become a source of infection to others. These persons thus, quite unintentionally and unknowingly, become sources of danger to others. In my out-patient work years ago I took the trouble to examine the phlegm of many chronic bronchitics, who come in such large numbers to hospitals, and I found that more than I had suspected had tuberculous disease in their lungs, though the symptoms would not have led one to think there was anything specially to be dealt with except ordinary bronchitis and emphysema. It is true that on careful examination you may find consolidation of one apex sufficiently obvious, but it is more or less masked by the emphysema round it. But as a matter of precaution I suggest that chronic bronchitics should have their phlegm examined occasionally, for the sake of the people with whom they are brought into contact.

I am afraid, ladies and gentlemen, that these remarks have been somewhat fragmentary and sketchy. I may not have told you anything new, or put anything which you already knew in a new way; but if what I have said serves to remind you again of things already known, but liable to be overlooked, perhaps it will not have been altogether useless.

March 27th, 1911.

LICHTWITZ discusses the mechanism of the acid intoxication in diabetes, and shows that sodium salts with anions capable of combustion are preferable to the bicarbonate. Sodium citrate is advantageous, as its taste permits it to be added to certain dishes or given with lemon-juice as lemonade. It does not cause disturbance in the stomach like the bicarbonate; the appetite does not suffer, and he has never observed diarrhœa even with 50 grm. (750 gr.) a day. In a typical case 20 grm. a day of the bicarbonate caused diarrhœa; it was then suspended and 30 grm. of the citrate given daily, which seemed to have an equally good effect on the metabolism, while no further bowel trouble was noticed. The elimination of nitrogen also increased under the citrate. The citrate might be injected intra-venously in emergencies; the bicarbonate is less adapted for this method of administration on account of its strong alkaline reaction. The citrate has only a weak alkaline reaction, and this can be neutralised by adding a little citric acid.—*Journ. A.M.A.*, vol. lvi, No. 10.

SOME COMMON FORMS OF DEAFNESS.*

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Patients illustrating the various kinds of deafness were first shown.

GENTLEMEN,—I do not propose this afternoon to bother you with an account of any of the rarer forms of deafness, nor the abstruse problems connected therewith, but just to run through a short description of some of the commoner kinds with a few hints as to diagnosis and treatment, which may, I hope, be useful in practice.

Commencing at the external auditory meatus, and working from without inwards, I must first mention our old friend, cerumen, which, like the poor, is ever with us, and which, though in many cases it may be quite a simple matter for diagnosis and treatment, has, nevertheless, sundry traps and pitfalls connected with it.

In the first place, although wax generally appears as a yellowish or brownish, greasy mass, this is by no means always the case, for it may be hard, horny, and black, and though quite insensitive as a rule, it may be surrounded by such an amount of dermatitis, or be so closely in contact with the membrana tympani (forming a mould for it in some cases), as to give decided pain on one's making any attempt to move it with the probe. And then, again, one must remember that, particularly where dust and dirt abound, a deceptive appearance is likely to be produced, and it may not be wax at all, or only in part, the bulk consisting of dry blood, pus, cholesteatomatic matter, epithelial *débris*, granulations, polypi, or foreign bodies.

And just one word of warning here: If your patient is markedly deaf, say, for instance, if he cannot hear your watch at all, although you may be certain that cerumen is blocking his meatus, be careful to avoid saying, "Oh, it is only wax, and I can soon put you right," as is often said, for in all probability he has an otitis media or some other disease of the ear in addition to the cerumen, and after removing this latter you will find that you

* A lecture delivered at the Polyclinic.

have not cured your patient's deafness, and will then have to climb down somewhat ignominiously.

Having carefully examined your plug with mirror, speculum, and probe, and having come to the conclusion that you have a fair chance of success as regards removing by syringing, without previous preparation try to separate the plug from the meatal wall a little with a probe so as to let a stream of water more easily detach it, and then straightening the meatus by drawing the ear somewhat backwards and outwards, direct the stream of water, at a temperature of about 38° C., into the crack which you have made or found, between the plug and the meatal wall. If your efforts are not successful it will be better to desist and dry the ear and prescribe some drops, remembering that you are dealing with a delicate organ, and that anything like rough or violent syringing is strictly to be avoided.

Now, as to the drops, they may be divided into three varieties: (1) Those which contain things which fizz and bubble, and so tend to disintegrate the mass, as peroxide of hydrogen; (2) those having a saponifying effect on the fatty material, such as bicarbonate of soda; (3) those having a softening effect, such as oil and glycerine. Each of these has its supporters and any will serve. In some cases, especially where the wax is mixed up with hair and epithelial *débris*, repeated soakings and syringings have to be performed before the meatus can be thoroughly cleared. The after-effects may be somewhat noteworthy. Either the hearing may be hyper- or even painfully acute, though this gradually planes down to normal, or there may be a comfortable feeling of an obstruction having been removed, with a return of normal hearing. Or (and this is especially so if the case has been an obstinate one, and too much force has been used in syringing) there may be dulness of hearing, and tinnitus, which, however, in most cases gradually wears off.

Again, say a patient comes complaining of some dulness of hearing, which has come on recently, especially with a feeling of fulness and throbbing in the ear, with a good deal of pain and tenderness on manipulation of the external ear, and especially on making pressure on the tragus, one may suspect furunculosis—in other words, boils in the meatus. The pain is considerable, and quite sufficient often to prevent the patient sleeping, although it is not so deep seated as in acute otitis media. On examination, which in the first instance must be made by concentrating a strong light on the meatus but without a speculum, as the introduction of this instrument might prove extremely painful, the walls will be found swollen and perhaps in contact, so that no view of the drum can be obtained. The colour may be yellowish or red, varying with the stage of the disease, and also as to whether it is superficial or deep-seated. After an examination, which should be very gently made with a probe, the parts will generally be found to be exquisitely tender, and if suppuration has come to the surface pus will exude. The best treatment will be free incision under nitrous oxide gas, preceded by syringing with carbolic acid 1 in 20. If this operation is declined, or the furuncle is freely discharging, and there is no tension whatever, hot applications together with the use of glycerine of carbolic acid will be best. It is well to remember that this disease has a marked relapsing tendency, and that therefore treatment should be kept up and the ear kept under observation for a considerable time after acute symptoms have subsided. Also the general health should be attended to and the urine examined. The amount of deafness varies very much in furuncle of the meatus. It may hardly be noticeable, but if the meatus is quite closed by the swelling it is of course much more marked.

Before leaving the meatus one must not forget to note infrequently in children and occasionally in adults deafness may be due to the presence of a foreign body, when the question, of course, is how to extract this same without damage to the ear, remembering that abscess of the brain, even death have been caused by rough treatment in endeavouring to extricate a foreign body.

The first step is to carefully inquire and examine as to the nature and position of the body, and it is well at the outset to instil into the meatus a few drops of a 20 per cent. solution of cocaine or adrenalin.

In the vast majority of cases gentle syringing, after a suitable interval, will be the best treatment; probably 90 per cent. of all foreign bodies can be removed in this way. If this fails and the patient is restless and the ear tender, especially in the case of young children, a general anæsthetic will be necessary. This having been administered, the

syringing should be tried again, and if it fails one must try one of the numerous hooks, forceps, bent wires or probes which have been devised with a view to getting hold of, or getting behind the body, and so extracting it. Such ideas as those of gluing on a wooden probe to the foreign body and so extracting it are more ingenious than practical. In very bad cases with a rising temperature and signs of deep-seated mischief, the foreign body being hopelessly jammed in the ear, it will be necessary to operate, turning the ear forwards and getting at the deeper parts from behind, but these cases are rare.

Passing on now to deafness due to affections of the middle ear, together with which we put those of the membrana tympani, we first of all note suppuration and its sequelæ. This may begin either in an acute otitis media, such as one commonly gets in scarlet fever and other such diseases, which may either entirely dry up with but little permanent damage to the ear, or destroy the ear as a hearing organ altogether, or become chronic with the formation of granulations, polypi, and carious bone, etc.; or it may never pass through the acute stage at all, but be due to some chronic disease, such as tuberculosis.

On examining a suppurating ear one may find, after cleansing, all sorts and various conditions, varying with the chronicity of the disease, such as the destruction in whole or part of the membrana tympani and ossicles, caries of the walls, perforations, adhesions, cicatrices, polypi, granulations, and cholesteatomatous matter, and our lines of treatment must be:

(1) To see that there is sufficient drainage.

(2) To keep the ear clean and remove all dead matter.

(3) To preserve and possibly improve the hearing.

(4) Application of antiseptics, those most generally used being boric acid and alcohol and hydrogen peroxide.

Whether the ear should be syringed or not is rather a moot point, some holding that it tends to diffuse the disease and in some cases is not necessary. But I certainly hold that the ear cannot always be cleansed without it, but in any case it should be carefully dried afterwards. Suction by means of Siegle's speculum is useful in some cases to extract the discharge.

When symptoms indicate extension of the disease to deeper points, of course more severe measures, such as the radical mastoid operation, have to be considered. One important point must here be noted as regards the hearing, which is not, I think, sufficiently emphasised in books on the subject, as the practitioner should be warned and prepared for it, namely, that as the patient's ear heals and becomes dry, suppuration ceasing, the hearing, much to his annoyance, may get worse, and although later on you may be able to improve this somewhat by inflation, you had better console him by saying that at any rate a dry ear is infinitely preferable to a suppurating one.

Operations for improvement of hearing by division of post-suppurative adhesions unfortunately have given only very poor results; occasionally, however, in such cases where, after suppuration, the hearing is very defective, one may get improvement by means of an artificial drum, and it is certainly worth while making the trial. No elaborate machine or instrument is needed, in fact, these should be avoided. A piece of cotton-wool moulded into a small ball, a good pair of forceps, and an ear speculum and a good light are all that are required.

With patience and perseverence you may perhaps be able to place the ball of wool in such a position, probably over the head of the stapes, as to markedly improve the patient's hearing, and having found that there is such a spot, he will be able himself, with care, to place the so-called artificial drum in correct position.

Coming now to non-suppurative affections, we may look upon chronic middle-ear catarrh as one of the commonest causes of deafness. It may be the result of repeated colds in the head, or sore throat, or undue indulgence in tobacco or alcohol, in fact anything likely to set up catarrh, such as adenoids, hypertrophic rhinitis, etc. The deafness is often insidious in onset, and varies in severity, being worse when the patient has a cold, and in damp weather. The function of the Eustachian tube is in abeyance, and the membrana tympani becomes thick and indrawn, and often more or less immobile and adherent to the deep parts. Inflation *per* the Eustachian tube may be difficult owing to the swelling of the mucous membrane, and is in the early stages accompanied by squeaking noises and moist sounds. On testing with a tuning-fork,

say of middle tone, giving 128 double vibrations per second (C) it will be found that when the fork is placed on the forehead of the patient he hears it longer in the deafer ear (Weber's test); and if the fork is applied to the mastoid process, and when its vibrations cease to be heard by the patient, if it is placed at his meatus, on the same side, but not touching the ear, he will be unable to hear it (Rinne's test, negative); or if placed on the vertex he hears it longer than a normal person (Schwabach's test). The patient may or may not hear better in a noise (paracusis Willisii). As before said inflation by Politzer's bag or Eustachian catheter improves the hearing.

Treatment.—In the first place, you must inquire into the habits of the patient with a view to prevention of catarrh, and deal with any post-nasal catarrh and hypertrophic rhinitis there may be present. Alkaline washes to the nose and iodine paint to the post-nasal space are very useful, but above all keep the ear well inflated by Politzer's bag or the catheter. In cases of a particularly obstinate character with Eustachian obstruction Eustachian bougies are sometimes used, and some surgeons are fond of injecting drugs such as paroline, menthol, etc., into the middle ear through a Eustachian catheter, or through a cannula passed through it, but the results are apt to be disappointing. The injection of hot air I have several times found to produce marked improvement. Electric massage has also been recommended.

Another unfortunately common cause of deafness is the disease known as otosclerosis. This is not an affection of the mucous membrane, but a disease of the capsule of the labyrinth consisting of the formation of new bone in some parts and absorption in others, resulting in more or less fixation of the stapes, and often blocking up of the round and oval windows with bony growths. This disease is more common in middle age, but may occur in young or old persons. The cause may be said to be unknown, though the blame has been laid on gout and syphilis. Certainly there seems to be an hereditary tendency in the disease. Tinnitus often precedes the deafness, and is a notable symptom. The deafness is apt to be steadily progressive. The membrana tympani appears practically normal in most cases. Inflation is easily accomplished, but does not improve the hearing. Rinne's test negative, paracusis well marked. All the above taken together give fairly conclusive evidence that we have present a case of otosclerosis, the treatment of which disease has so far been very unsatisfactory. Numerous drugs, such as iodide of potassium, phosphorus and arsenic have been tried, with but little success.

As a less common cause of deafness there is disease of the internal ear. This may be due to syphilis, malaria, gout, and other diseases tending to produce variations in the intra-labyrinthine pressure. The signs are marked deafness, especially for high notes. There is also bad bone-conduction. Rinne's test is positive; by Weber's test the sound is referred to the better ear, thus giving results opposite to what are found in middle-ear catarrh. The prognosis is bad in these cases, but iodide of potassium and blistering should be tried, and the cause, if possible, should be found and treated.

March 27th, 1911.

Iodism and Thyroidism.—The correlation between these conditions is at present being freely discussed. Berg traces the history of our knowledge of the use of potassium iodide, with the discovery of iodism, which occurs not only when the drug is pushed beyond tolerance, but also in some individuals from minute doses (iodine idiosyncrasy). The phenomena of iodism show the greatest variety. Berg seems unaware that severe iodism from small doses of the drug is due, in some cases at least, not to supersensitiveness *per se*, but to defective elimination by the kidneys. Thus some of the worst recorded cases of iododerma tuberosum have occurred in subjects with nephritis or temporary obstruction of the kidneys. Iodine in certain cases can set up the picture of thyroidism, or of Graves's disease. In individuals with goitre it can reduce the size of the latter, although goitrous subjects are often more sensitive to the drug than are normal individuals. On the other hand, the use of iodine may cause a marked engorgement of the gland in connection with the picture of thyroidism, although the latter readily occurs without the former. Iodides given to a syphilitic girl set up a typical Graves's syndrome (no exophthalmus) with a veritable thyroiditis.—*Med. Rec.*, vol. lxxix, No. 10.

ON THE

TREATMENT OF CANCER OF THE RECTUM.

A DISCUSSION* BY

Mr. Swinford Edwards.
Sir Frederic Eve.
Mr. Waterhouse.
Mr. Boyce Barrow.
Mr. Sampson Handley.
Mr. Godlee.
Mr. F. C. Wallis.
Mr. Lockhart Mummery.
Mr. Fagge.
Mr. Gordon Watson.

(*Concluded.*)

Mr. P. Lockhart Mummery said he thought it necessary to insist very strongly upon one point which Mr. Wallis also touched upon—namely, the large proportion of inoperable cases of cancer of the rectum which were seen. Mr. Harrison Cripps stated that only one case in four of those which he saw was operable. In his (Mr. Mummery's) own work, out of 100 consecutive cases from his note-books he obtained the following figures: Of the total, 35 were operable, 57 inoperable; 7 refused operation. His definition of an inoperable case was that it was not possible to remove the growth, even by an extensive abdomino-perinæal operation, either because the growth involved structures which could not be removed, such as the bladder, or because there were secondary deposits; or, in a few cases, because the patient's condition did not warrant the risking of operation. The larger proportion in Mr. Cripps's figures was easily accounted for by the different view which that gentleman took on a question of what was and what was not operable. He thought he (Mr. Mummery) could claim to be as optimistic as most surgeons with regard to removal of growths from the rectum, and there was no doubt that 57 per cent. of inoperable cases was too large, and that such a proportion was preventable. There were two causes for that: First, that the patients often did not seek advice until too late. But what he believed accounted for the larger proportion was that the medical man who first saw the patient did not make a local examination when there were symptoms in the rectum. That omission was exceedingly common. He was continually seeing, at St. Mark's Hospital, cases in patients who said they had been treated by their doctor for piles for six months, and that no local examination had been made. On examination at the hospital a large carcinomatous growth was found. Such cases were seen almost every week. It was impossible to produce statistics on the point, but his experience led him to say that from the time carcinoma began to when it became inoperable varied from six to nine months. At St. Mark's a common period for the cases to be seen was at the seventh month—*i.e.* just before they became inoperable. If such cases could be seen quite early there would be an immense advance in the successful treatment of cancer of the rectum. At present there were means, by using the sigmoidoscope, of diagnosing with absolute certainty any growth in the bowel at its earliest stage, when it was little more than a papule, at any point below the middle of the sigmoid flexure. He would exhibit a drawing of the earliest case of carcinoma he had ever come across, the growth having been detected through the sigmoidoscope. He removed the growth. If the condition of the patient was favourable, and he was not subject to any other disease, operation should be done for removal of growth in the bowel whenever there was a reasonable chance of saving the patient's life, because the result was so bad if no operation were done; moreover, even in the cases which were apparently so hopeless, he had frequently seen most brilliant results. What was apparently one of the most hopeless cases he ever saw, and just operable, was that of a patient upon whom he operated eight and a half years ago. The case had been declared by other surgeons in London to be inoperable. He removed the rectum with half the sigmoid flexure and restored the bowel, and the patient was alive and in the enjoyment of perfect health to-day. He was thirty-three years of age at the time of operation. It did not do to pick one's cases too carefully, and it was not justifiable to try to get statistics which would yield a small percentage of recurrences. The duty of the surgeon was to do his best for the patient, and not to try to get satisfactory statistics. The surgeon should tackle border-line cases whenever they presented themselves, and the fact of doing that would spoil good statistics. Though there had been some failures from following that policy, the number of successes in border-line cases was sufficient to justify a continuance of that attitude. He was one of the earliest to advocate in England the removal of the entire rectum in every single case

* At the Royal Society of Medicine.

of carcinoma of that part, and he still believed that that was correct. And, whenever possible, the entire rectum should be removed, not only because it gave a better chance of removing the whole growth, but also—and there he differed from Mr. Handley and Mr. Miles—because it gave the possibility of restoring the bowel to the condition it was in before. It was possible, by removing the entire rectum, to bring the sigmoid to the anus and give the patient perfect sphincteric control. He had several such patients now with their sigmoid flexure opening at the anus, and their sphincters were normal. He admitted that if the growths were large and near the anus one was running greater risk of recurrence, but no absolute rule could be laid down as to what should and what should not be done in rectal cancer. The surgeon must be guided by the circumstances, and where there was a small growth low down the means must be modified. One could get an excellent result by removing the rectum and bringing the sigmoid to the anus. One advantage of the perinæal operation, which Mr. Cripps had not given it credit for, was that it enabled one to resect the growth in many cases of carcinoma of the rectum which otherwise would be inoperable. There were many cases at the recto-sigmoidal junction which he would defy any surgeon to get at by any other operation, while many of those cases could be satisfactorily treated by the abdomino-perinæal operation. The ideal operation for cancer of the rectum must be one in which aseptic healing must be the rule, in which the patient would not have a large sloughing, suppurating wound; it would also be one in which the bowel could be restored. It was possible to remove a part of bowel as large as Mr. Handley's specimen, and bring the sigmoid down to the anus as well. The only difference was that it was necessary to leave the sphincters. By carrying the incision to the right side of the sigmoid colon one could almost bring the splenic flexure to the anus. The ideal operation was not one which left a colostomy behind. After extensive removal the bowel should be restored. The exception would have to be in those cases where the growth was near the anus; then, of course, the sphincters could not be preserved. The point upon which surgeons ought to concentrate their attention was aseptic healing. He had had only three cases in which that had occurred after removal of the entire rectum. A recent one, in which he did the abdomino-perinæal operation, was that in which he took away everything from the sigmoid to the anus, and the patient was out of bed in eleven days, and in three weeks returned to his home in the country unaccompanied. Another case was that of a lady who was out of bed in ten days, and she also went away in three weeks. In neither of these cases was there any temperature or any anxiety. The third patient was one in hospital, and he also went out in three weeks. They showed that such could occur, and the aim should be to get such cases frequently. He believed that most of the mortality from excision of the rectum by the abdomino-perinæal excision was due to peritonitis and sepsis. The reason the mortality was low in the operation advised by Mr. Cripps was that in those cases sepsis did not much matter, as it involved chiefly the skin. In the more serious operations sepsis was a more important matter, and might cause the life of the patient to be lost.

Mr. C. H. FAGGE said that his own small experience had been entirely opposed to any form of perinæal or sacral operation, and now for nearly three years he had, except on one occasion, confined himself to the abdomino-perinæal route; his experience of Kocher's method (for he agreed with Mr. Waterhouse that if the perinæal route were chosen sufficient room could be obtained without removal of any part of the sacrum, and therefore a complete Kraske was unnecessary) had been unfortunate, for all except one of his cases, about ten in number, had died of recurrence after intervals of six months to three years. He thought the first speakers who were in favour of perinæal methods alone had entirely given their case away by admitting that this method was unsuitable when the growth had extended beyond the bowel wall, for in his experience many patients did not come under observation until the peri-rectal tissues were permeated, as one found by dissection after operation and by microscopical examination of the parts removed; and he entirely agreed with Mr. Miles that in others which had not advanced so far, to limit the excision to the bowel only was sailing too near the growth, and was entirely at variance with present surgical principles in dealing with cancer elsewhere. He believed that the abdomino-

perinæal route would give some measure of success in cases which the openers admitted were inoperable by their method, and therefore, even by those who laid much store on statistical tables, no comparison could be drawn in mortality, etc. Personally, he paid no attention to the relatively high mortality, partly because the number of cases was small and he admitted no contra-indication except metastases or fixation to the bladder, and chiefly because he did not intend in this or any other branch of surgery to base his considerations in regard to choice of operation on any question of mortality; if one were going to advance, and therefore to do what was best for the patients, one must at times risk a relatively high immediate mortality for the chance of obtaining a larger percentage of permanent cures. Some three years ago, when becoming dissatisfied with perinæal methods, he met with two cases which finally settled the question for him; in the first, Kocher's operation on a man was followed by death from inhalation pneumonia in a few days, when several unsuspected secondary deposits were found in the liver; the second case was sent to him by one of his medical colleagues, who was very pleased to have detected an early case with the sigmoidoscope, but he (Mr. Fagge), after his last experience, refused to operate from below without first exploring the abdomen, when he found cancerous peritonitis, and of course no radical operation was done. He had carried out the abdomino-perinæal operation six times on patients of his own, and had also on several occasions assisted one of his senior colleagues. In some cases they had operated together, one from the abdomen and the other from the perinæum, but they had found it unsatisfactory. He had in one of these six cases carried out the operation in two stages, but the bowel, detached and pushed down into the pelvis, sloughed, and the patient died of peritonitis; he had seen the same thing happen in the practice of another surgeon. In one of the six patients abdominal exploration for acute obstruction led to a diagnosis of cancer of the rectum to which the left tube was adherent; at a second operation these organs were removed by the abdomino-perinæal method, when the condition causing obstruction was found to be tuberculous salpingitis widely infiltrating the rectal wall. The patient got well. In the other five microscopical examination showed typical carcinoma; three died as a result of shock or peritonitis shortly after operation, and two recovered, but neither have yet passed the three years' limit. As an alternative, when the growth is situated in the upper part of the rectum, the abdomino-anal method, as described by Miss Aldrich Blake, and as practised by Mayo and others, is to be considered, but he was of opinion that for growths in the ampulla—the usual site of rectal neoplasms—it was wise to sacrifice the levator and other tissues around the bowel. An attempt to preserve the sphincteric apparatus ran two risks, of which the first was local recurrence, and the second an incontinent, functionless anus, which was probable after such an operation necessitating a perinæal dissection, unless all the muscles and nerves could be left strictly intact; he thought a patient was more comfortable with a colostomy under such circumstances, so that as a routine operation for rectal cancer he preferred the abdomino-perinæal route.

Mr. Gordon Watson said that most of the points he intended to speak on had been already mentioned. Since April, 1909, he had performed the abdomino-perinæal operation ten times, and had assisted others on nine occasions. He had also watched distinguished operators perform the operation. In the cases in which he had himself operated, he had only twice brought down the bowel to the anus. Of his ten cases four died, two of them three weeks after the operation—one of pneumonia and one of peritonitis—and two within a week of the operation. The chief objection which most people would raise to the abdomino-perinæal operation was its high mortality: but the same kind of thing occurred in connection with the operation of pan-hysterectomy by the abdominal method when first introduced. For the first few years the mortality was about 40 per cent., whereas a gynæcologist told him recently that the present mortality was probably under 10 per cent. He considered that the latter figure would be reached in the abdomino-perinæal operation if the cases were carefully selected. Patients over a certain age, with fat abdomens and thick arteries, were not suitable for such an operation. With regard to the technique of the operation, he would like to bring out certain points. No patient ought to die of shock after this operation. Each

of his patients had been given ¼ gr. of morphia an hour before the operation; most had had saline during the operation, given by means of Arbuthnot Lane's rubber bags. Some had had oxygen, passed through absolute alcohol, given with the anæsthetic. In not one of his ten cases had there been danger or grave anxiety on account of shock after the operation. He was sure shock could be eliminated. With regard to the length of the operation, the longest time he had taken was one hour forty minutes, and the shortest time was one hour. He did not think that was too severe a strain for that procedure. If the mortality could be reduced, he did not think any surgeon could object to it on pathological lines, as had been pointed out by Mr. Handley and others. He had not succeeded in discovering permeation in the portions of bowel above the growth, which Mr. Handley had demonstrated that evening, but he had kept all the specimens, with a length of bowel above and below, and would further investigate the point. He wished to emphasise another point, namely, the method which Mr. Wallis practised, and which he (Mr. Watson) had several times assisted him at, namely, bringing the bowel down through the sphincters without dividing the sphincter. But that was not suitable for cases in which there was a growth in the lower five inches of bowel, because he was convinced that later on, though the cases might do very well at first, recurrence would happen locally where the sphincters were not removed. The procedure was admirably suited for growths above the reflexion of the peritoneum from the vesiculæ seminales on the one hand, or the vagina on the other. He would like to know the opinion of Fellows with regard to prophylactic injections of *coli* vaccine. He had given it experimentally in several cases, but it was difficult to arrive at conclusions. The idea was to give it before operating so as to diminish the chances of peritonitis by raising the resistance of the patient. He believed that the great risk connected with the operation was the occurrence of peritonitis the result of sepsis. Mr. Handley mentioned that he had had three consecutive fatal cases. He (Mr. Watson) also had three consecutive successful ones, but they were followed by three consecutive fatal cases, though in two of them he felt he was partly responsible, because he gave *coli* vaccine within thirty-six hours of the operation, which was due to a misunderstanding between the pathologist and the house-surgeon. Since that time he had given the vaccine a week before operation, and during that week he had had the teeth carefully attended to. Naphthalene tetrachloride was also given every four hours, in 5-gr. catchets, and the rectum irrigated daily with a view to diminishing the intestinal flora for a week before operation.

Mr. SAMPSON HANDLEY, in reply to Mr. Gordon Watson, said he had not used anti-*coli* serum, but he had used anti-streptococcic serum in one case. That case recovered, but he could not say what part the serum played in the recovery.

Sir FREDERIC EVE said he had used anti-streptococcic serum as a routine measure before doing excision of the rectum. It seemed to do good; he could not say more than that.

Mr. GODLEE said that before calling upon Mr. Swinford Edwards to reply, he would like to mention that he knew one case in which carcinoma of the rectum did not recur until ten years after removal, and then the recurrence took place in both the lung and the rectum. He had been much struck, when doing the combined operation, with the small amount of shock which resulted. It did not seem to matter whether the operation was done quickly or slowly; in fact, sometimes the more slowly it was done in moderation the better. The real point of importance was to avoid stretching and pulling on the structures. Even in the prolonged operations there was a small amount of shock compared with that from Kraske's operation. What appeared to be the great objection to the combined operation was that he at all events could not be sure, however carefully it was performed, that there would not be sloughing of the posterior part of the bowel. He had no dread of peritonitis; that did not occur if the peritoneum were sewn up carefully. He did not succeed in any large proportion of cases in getting healing by first intention—in fact, he only remembered one genuine case. Therefore he was becoming convinced that the operation of choice in the immediate future was the removal of the rectum and lower part of the sigmoid flexure, leaving a permanent colostomy wound. An unsatisfactory anus in the right posi-

tion was less satisfactory than a satisfactory colostomy wound.

Mr. SWINFORD EDWARDS said he could confirm Mr. Godlee's remarks as to the extraordinary absence of shock after the combined operation. His last case of excision of the rectum took place at St. Mark's Hospital a fortnight before, when he did the combined operation, and occupied three hours and a quarter over it. But the patient was doing remarkably well, and had no shock. Therefore he was not entirely wedded to the para-sacral operation, and he thought that all cases should be judged on their own merits. By the operation advocated by Mr. Miles, which he called the radical abdomino-perinæal operation, he (Mr. Edwards) conceived that cases had become operable which previously would have been considered inoperable; for Miles showed, as Mr. Edwards said in his paper, that if the cases were analysed it would be found that the disease in all had passed beyond the confines of the bowel itself, having become adherent either to the organs in front, or to the sacrum behind. Most speakers had emphasised the point that, no matter how small the cancer might be or where it might be situated, the combined operation should be performed, though it was admitted that the operation entailed a mortality of 30 to 40 per cent. He very much doubted whether, if that fact were properly put to the patients, they would agree to undergo the operation, especially if they were told they would, if they recovered, have to have a colostomy opening. There were cases in which the cancer was situated low down, perhaps not larger than a shilling or a florin, and limited to one wall, which could be dealt with quite locally. He pointed out that he had had in private practice no less than five cases of partial excision for that particular condition. All those cases were till recently alive, and had passed the three years' limit, and had retained the use of their normal anus, the exception being that one died from lung trouble, but some years after the operation. With those facts before him he did not think he would be justified in recommending the radical abdomino-perinæal operation for such cases. That operation had its own field of usefulness, viz. when the growth was either somewhat adherent or was situated in the upper rectum—*i. e.* at the junction of the sigmoid and rectum. If the combined operation were done, it was well to leave a permanent colotomy opening, and remove all the bowel below it. In the case operated upon a fortnight ago he removed 18 in. of bowel, and established an artificial anus. If one attempted the procedure recommended by Mr. Mummery of bringing the sigmoid down to the anus, having excised all the bowel between those two parts, he thought there would be great risk of sloughing. He had seen it happen more than once, and of course the result might be fatal. He was very glad the opportunity had been afforded of having the discussion, especially at this time, for evidently there were two opposite camps. He hoped he did not belong entirely to one, but rather to both.

LANZ has applied the Talma operation in four cases of ascites from cirrhosis of the liver, with complete success in two cases and absolute failure in the others. In a more recent case the patient was a man, æt. 69 years, with enormous ascites and suspected cirrhosis of the liver and alcoholism. The absence of enlarged veins in the abdominal wall and the age of the patient suggested carcinomatous peritonitis. In order to divert the fluid into a new route, after removal of 12 or 15 litres of fluid, Lanz drew the right testicle upward out of the scrotum by pulling on the spermatic cord, and implanted it in the open abdominal cavity, suturing the spermatic cord its entire length around the peritoneal incision. The discovery of tuberculous nodules in the peritoneum had left little hope of permanent recovery, and the patient died five weeks later. There had been no recurrence of the ascites, but the spermatic cord and the retroperitoneal tissue were found gorged with serous infiltration, everything showing that the operation had provided a collateral circulation for the lymph with direct drainage of the peritoneal cavity along the spermatic cord with its wealth of lymphatics. The outcome might have been still better if the testicle had been wrapped in omentum. The findings at necropsy corroborated the advantages of this method of internal drainage. The results are more promising, Lanz declares, than when the omentum is merely fastened—as by the Talma operation—to the abdominal wall, which is not so rich in lymphatics.—*Journ. A.M.A.*, vol. lvi, No. 10.

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A LECTURE

ON

DYSPNŒA IN BRIGHT'S DISEASE AND DIABETES.

Delivered at the Polyclinic.

By NESTOR TIRARD, M.D., F.R.C.P.,
Senior Physician to the King's College Hospital, and Consulting Physician Evelina Hospital for Children.

LADIES AND GENTLEMEN,—First of all with regard to the choice of subject for a post-graduate lecture. I take it that the choice is always to be determined by what is at the moment interesting to oneself. It is always, I think, most likely that what is interesting to oneself may be of interest to others also; I therefore determined upon this subject of dyspnœa in Bright's disease and diabetes, because at the time I was asked to give these lectures this term two or three recent cases of urgent respiratory trouble had been under my notice, and I felt keenly interested in the subject.

And next with regard to the title. I suppose I am not wrong in thinking that the title to a certain majority of people may suggest that the lecture is to be concerned with renal asthma and with air-hunger. The title "dyspnœa in Bright's disease" suggests renal asthma; dyspnœa in diabetes suggests the condition to which the unfortunate name "air-hunger" has been given. With regard to these two terms, I am very far from satisfied with either of them. The term "renal asthma" appears to indicate that, at any rate, there is some limitation of this asthma to kidney diseases, that it separates this form from other types of asthma that occur with other diseases, such as heart disease or lung disease, and that we are dealing definitely with one simple subject. On the other hand, the term "air-hunger" has, I think, practically become limited; it is so very rarely mentioned in connection with any condition apart from diabetes,

that when we speak of air-hunger we almost necessarily think of diabetes. Before we part to-day I hope I shall have given you reasons for being as dissatisfied as I am with this limitation of either of those terms. Neither term is strictly accurate. "Renal asthma" does not include all types of difficulty in respiration which we meet with in connection with kidney disease. The term "air-hunger," on the other hand, is not strictly limited to diabetes. We meet with conditions of air-hunger, as I hope to show you in the course of this lecture, in other diseases where the association with diabetes is only a very remote one, or even where there is no association at all. For these reasons, although the difficulty of breathing was what I had in my mind in addressing you to-day, I prefer to use the term "dyspnœa," and to use that term in the broadest possible way, meaning by that term difficulty of breathing as ordinarily expressed by increased rapidity of breathing, accompanied by definite indications of distress. Apart from the rapidity, sometimes indications of distress may be shown by restlessness, the patient tossing about in the effort to get more air into the chest. There is always a sense of lack of air with dyspnœa. Sometimes, as with asthmatic conditions, we find patients fixing their thorax and grasping the chair, or sitting up in bed and grasping the sides of the bed, or sitting in an armchair and grasping the arms, so as to steady the clavicles and scapula, and give full force to the accessory muscles of respiration. With this form of dyspnœa it is scarcely necessary to say that the patient is always in a condition of orthopnœa. I do not think I shall want, for the purpose of this lecture, to consider any type of dyspnœa in which the patient is able to be prone in bed. I assume, after the condition has existed for a few hours, the occurrence of cough. There may be no cough at the beginning, but after a few hours of dyspnœa there will be cough, and there may be some scanty expectoration. To give a fair illustration of the sort of dyspnœa which I mean, I would instance the sudden distress that we sometimes see with pulmonary embolism, such as we meet with in post-partum cases, with separation of clot, after operations on the veins or after operations on the rectum. I suppose we have all had sad examples of this form of dyspnœa under observation, and you know how the picture suddenly changes: from being in a position of comparative rest and peacefulness, the patient suddenly becomes urgently troubled with dyspnœa, and unfortunately in many cases dies. Dyspnœa which is urgent, of this type, is always a dangerous symptom. None of us look on dyspnœa in its suddenness of onset as anything but a most alarming condition. Certainly, as far as the two diseases I have on my programme are concerned, that is to say, Bright's disease and diabetes, I think we have to look upon dyspnœa as one of the terminal symptoms. I had got thus far in my thoughts when I was arranging the subject-matter for this lecture, and then I began to collect illustrative cases, and wondered how I should get any sort of order into this lecture. I remembered, however, that a few years ago we had, at the University of London, in seeking for any variety of questions that might defeat the wiliness of the coaches, developed a certain question which the men who knew the answer liked very much. We merely asked for the modes of termination of certain diseases. With that question in my mind I saw that I had a clue which would help me in this lecture to-day. I have, first of all, to separate out for our consideration the types of Bright's disease, and next to consider the modes of termination of the different types; in that way we shall have order out of chaos. They are: (1) *Acute nephritis*, which we shall take as the starting-point; (2) secondly, as a sequel of it, and a common sequel, *chronic nephritis*. By the term "chronic nephritis" I mean chronic tubal nephritis, the common result of the acute nephritis which is passing on to the chronic stage. It has also other names—large white kidney and others. (3) For the third, there is a condition which stands comparatively by itself—*chronic interstitial nephritis*, meaning by that term the condition known variously as gouty kidney, or cirrhosis of the kidney, or small red kidney. These are the forms of disease of the kidneys of which I want to speak to-day. If we consider what are the modes of death to be met with in each of these three conditions, I think you will agree with me that the first, the acute nephritis, may end directly or indirectly as the result of the nephritis. By directly, I mean that death may result directly from interference with the work of the kidney, from suppression of urine, and with this suppression the development of toxic symptoms, prominent among which are

headache, convulsions, and coma. This is, I think, a fairly common mode of death for cases of acute nephritis.

For the indirect mode of death we have to consider the development of certain complications of acute nephritis, amongst these being the various troubles of organs of respiration, pneumonia, pleurisy, broncho-pneumonia, tubercle and the like —conditions which may occur in almost any case of acute nephritis, especially when the acute nephritis is following one of the acute exanthemata. In the second group, this indirect group, we meet with dyspnœa, but the dyspnœa is not necessarily the result of toxæmia. It may be in part an indication of toxæmia in connection with pneumonia for example, where the rapidity of respiration is due to the amount of the toxin developed by the micro-organism. But on the other hand, the dyspnœa will also be an expression of the extent of the lung affected. With this form of dyspnœa in Bright's disease I do not wish to trouble you to-day. The dyspnœa of acute nephritis, if due to pneumonia, or to pleurisy, or to broncho-pneumonia, is practically the dyspnœa of these diseases, and is not due especially to the kidney disease.

Let us now take the next form, chronic nephritis, and consider what are the modes of death. Taking Moxon's clue to chronic tubal nephritis, or the large white kidney, the large white man with a large amount of œdema, we know that one of the modes of death may be commonly a great increase of dropsical effusion, and with this dropsical effusion failure of heart muscle, resulting directly from the impediment of the circulation. On the other hand, in some cases of chronic nephritis we may find that in addition to the œdema there will be toxæmic symptoms. In some of these patients, as the œdema increases, we find that they develop more and more drowsiness, more and more headache, and they may perhaps have convulsions towards the end. Thus toxæmia may occur with chronic nephritis, either in connection with dropsy, or separately, independently of the dropsy.

Then with regard to chronic interstitial nephritis, I suppose the commonest mode of death—it is the one which we fear most in insurance work—is the development of arterio-sclerosis. We may regard the interstitial nephritis as part of the arterio-sclerosis, and arterio-sclerosis which is going to lead to increasing weakness or brittleness of the arteries and thus cause cerebral hæmorrhage. Another mode of death with chronic interstitial nephritis is that the increasing arterio-sclerosis may lead to dilatation of the heart, with alteration of the valves of the heart, both contraction and thickening, and death will probably ensue from cardiac failure. The heart which is dilated owing to the arterio-sclerosis with the valves acting imperfectly, will probably, as the case goes on, gradually fail, and the case to all intents and purposes assumes the type of an ordinary case of mitral failure. Then a third mode of death for chronic interstitial nephritis is the development of uræmic conditions. We know that many patients with chronic interstitial nephritis suffer from symptoms which we conveniently group under the name "uræmia," that we speak of the neuralgic troubles of these patients, the headache, the cramp, and so on, all as indications of the circulation of some poison which is conveniently called uræmic not pledging ourselves as to what the nature of the poison may be. Now, with the uræmic conditions which cause these slighter manifestations during the time that the patient is going about, able to pursue his business, we may find, as with the uræmic conditions of the first group, that the disease is suddenly brought to an end either with coma following convulsions, or possibly with a sudden attack of dyspnœa. So here we have a fair basis for classification.

Dealing in the same way with diabetes, and considering what are the modes of death, we may divide our cases of diabetes into the two classes, acute and chronic. I am speaking purely of glycosuric diabetes in which there is sugar. I am not speaking of gouty conditions; I want to leave those out of consideration. Of these two classes we see the acute mainly, but not exclusively, in comparatively young patients, children and young adults, though we may see an acute case now and again in a man aged fifty. By an acute case I mean one in which the onset of the diabetes is rapid, the downhill progress rapid, and in which there is a very high specific gravity. I shall be seeing one shortly, who has had a specific gravity of 1040 and 1042 for a week in spite of treatment, and that patient is over fifty years of age. Mostly, however, when we are speaking of acute cases we are thinking of cases in young adults. These patients die in various ways. The young child may die of

failure of respiration, or with the rapid development of tuberculosis, or with pneumonia. On the other hand, as a second mode, these patients may die with symptoms of toxæmia. Here I use the term in a wide sense, in the same sense as using the term "uræmia" in connection with kidney disease, not pledging myself as to what the exact toxic substance may be, whether it may be oxybutyric acid, or acetone, or diacetic acid. We have to deal with the fact that the case is one of a poison acting primarily on the central nervous system, which may in some cases develop increasing coma. The patients may become at first slightly drowsy, then increasingly so, irresponsive, unconscious, and the unconsciousness may end fairly rapidly. Or, as another symptom of the toxæmia, we may occasionally find this symptom of urgent dyspnœa, the air-hunger which I have mentioned. The third way in which acute cases may terminate is from increasing inanition, loss of strength and flesh, a gradual increasing weakness. For the chronic cases, the second group, we have to consider cardio-vascular changes sometimes with cardiac failure, or secondly, the development of any intercurrent malady. A patient, even with chronic diabetes, is likely to suffer from any intercurrent malady, and that may be a very serious matter to a diabetic. Thirdly, exhaustion with toxæmia, owing in all probability to some fresh complication, something, for instance, like perforating ulcer or gangrene, or even cardiac failure after operations for the relief of these conditions. These are the common modes of death with chronic diabetes.

Now, harking back somewhat, I think the greatest variety of dyspnœa to be seen in Bright's disease is in connection with chronic interstitial nephritis. I will therefore speak more fully on this condition than on the others. One of the forms of dyspnœa, which seems very alarming, but really does not matter very much, may occur in chronic interstitial nephritis in patients with extremely irregular cardiac action. I have brought down the original letter which I received from a doctor in connection with one of these cases. The case was that of a patient, æt. 66 years, suffering from albuminuria. "The condition has been known for eighteen months to two years. During that time there has been found a slight amount of albuminuria and occasionally no albumen has been present. The specific gravity has varied from 1010 to 1015, and I have found no indications of casts, though it is a good while since I examined the urine. There is polyuria, general fatigue, physical and mental, and some shortness of breath." That does not, as I read it imply that this man is fit to come into this category of dyspnœa. "No dropsy: his heart is no doubt somewhat hypertrophied and pulse somewhat slow and laboured. I supposed him to have granular kidney. He is certainly not getting at all quickly worse, has not lost flesh. Has been for the last two summers to Marienbad and had a good deal of fat taken off him. I proposed him going to Egypt or Algiers," and so on. When this patient came into my room I was uneasy, and for this reason: he had great shortness of breath and was puffing and blowing. He was a big man, 5 ft. 8 in. high and weighing nearly 16 st., and his pulse was 42. When I listened to the heart I found the cardiac beats were 84 to the minute. It was one of these curious cases which I have no doubt you have all come across, where every alternate beat failed to reach the wrist and failed to open the aortic valves. That was the cause of the dyspnœa in that man. There was very little lung trouble: a few moist râles, but nothing very much. The dyspnœa in this case was entirely due to this intensely irregular contraction of the heart. I may say, in continuation of this case, that I had pulse-tracings of the man taken, which were very interesting. They showed a "fortification" appearance at the top; there was then a very long-drawn-down line with marked oscillation in the centre of the down stroke. When seen three weeks later there was no dyspnœa, the pulse was 75, and this little oscillation was found to have been the sole representative of the abortive systole. I published that case a little time afterwards as an illustration of the benefit that could be obtained in such cases from the use of digitalis: 10 ♏ doses of tincture of digitalis steadied the heart's action completely, and all went well with that man for a time. The sequel I will tell you presently. I have said enough to show you one type of dyspnœa; but certainly in this case the weight of the man, the irregular cardiac action, which cardiac action was due, I think, to the albuminuria, and the conditions generally, made me glad when he was safely out of my house.

Now for another type of dyspnœa, which we may meet with in association with chronic interstitial

nephritis. I refer to the engorgement of pulmonary vessels and dilatation of the heart. Of this, of course, I could give you unnumbered examples. For instance, in regard to the foregoing patient, I have letters later, when I was following up the case. The practitioner who had attended him in Torquay was good enough to send me up many notes about him. In January, 1909, the man died. Between October and January he had shown signs of pulmonary engorgement, the ordinary œdema of the lungs, orthopnœa, distress in breathing—in a word, dyspnœa due to failing action of the mitral valve, and dilatation of the right side of the heart.

I will mention another man, æt. 56 years, who had had a long history of many attacks of what was termed "ptomaine poisoning," which meant attacks of abdominal pain with diarrhœa, and many attacks of shortness of breath, with a noisy, dry cough. In that man there was a systolic bruit at the apex outside the nipple line, and the condition affords another example of this group of dyspnœa from failure of circulation. Another type which our first man exhibited was the Cheyne-Stokes' respiration, when the dyspnœa is not continuous—where there are periods of intense dyspnœa interrupted by periods of comparatively ordinary breathing. In that case the Cheyne-Stokes' respiration began very late in the case and continued to the end.

To give you another example of a different type of dyspnœa, I will mention the case of a boy, æt. 11½ years, whom I saw first in 1903. I will give you a few of the notes of the case. I only saw this boy at long intervals in connection with a difficulty with his water. When I first saw him in May, 1903, there was much pus in the water, and the specific gravity was 1010. X-ray examination was advised, but refused, I am afraid, on account of the expense. It was just one of those unfortunate cases in which the patient was not rich enough nor poor enough to have the best advice, for if he had been very rich there would have been no question of refusal of X rays, and a very poor man would have come into the hospital and been treated straight away. In July, 1903, there was no pus, no casts, there was a very slight trace of albumen, and the specific gravity still remained low—1008. In August, 1903, he began to show signs of vascular changes—re-duplication of the second sound at the base of the heart. From that time I did not see him until November, 1908, and I was told that six months before that he had a high temperature. Albumen was again found in the urine, and when I examined it on that occasion there were a few large oily casts. With regard to the heart-sounds, at the impulse, which was nearly in the nipple line, the systolic sound was loud and booming, and the sounds at the base were exaggerated. I saw nothing more and heard nothing more of the boy until November, 1910, when I was called to see him in one of the suburbs. He was suffering with sudden urgent dyspnœa, without any further signs of the cause of this symptom. There was plenty of air entry into the chest, the exaggerated sounds of the heart were as before, and the pulse was of high tension. Owing to the intensity of the dyspnœa one had to give a fatal prognosis, and the boy died within twenty-four hours from the onset. If that boy had had any trace of sugar, or of acetone, or of diacetic acid, that case might have been classified as one of acidosis. As it is, I am afraid it must be considered as one of the unusual types of uræmia.

Now, turning to chronic nephritis, I mentioned just now that dropsy might be a cause of death, or that dropsy might be associated with toxæmia. The evidences in this case one has to judge of largely as a result of treatment. You will find, as probably many have, indeed, found, that all cases of dropsy are not to be treated with the same satisfaction by the same methods even when they are not associated with chronic nephritis. Sometimes you will find cases of dropsy where good results are obtained by the ordinary methods—hot-air baths, vapour baths, wet pack, purgatives, and the like; and at other times you will find that cases which appear almost identical do not do well under this form of treatment, not so well as if you boldly make incisions over the malleoli, or remove fluid from the abdominal cavity. I remember very well one patient in hospital in whom we had just this sort of difficulty. Again and again we were able to relieve the dropsy by medicinal measures, and yet the man remained ill, and weary, and dull, and complained of headache. We then proceeded to incise the ankles, although there was not very much dropsy at the time they were incised, and next morning the man was bright and clear. The meaning of that sort of experience is that in both forms of treatment we were removing fluid, but by the first

method, though the fluid was removed, the nitrogenous materials which were carried out into the subcutaneous tissues had to be re-absorbed into the system and slowly eliminated. On the other hand, when we are making punctures and removing the fluid quickly there is no surcharging of the system with the results of nitrogenous waste.

There are two chief forms of dyspnœa seen with *diabetes*. Both are of great importance with regard to the prognosis, as they indicate respectively, first, the rapid extension of one of the grave accompaniments of this disease: or, secondly, the sudden development of one of the most dangerous and most hopeless of the symptoms of acidosis.

Obviously the two forms of dyspnœa to which I am now referring are the dyspnœa of acute tuberculosis and the dyspnœa of air-hunger.

Of the two, the dyspnœa due to tuberculosis appears much more gradually; it is ordinarily accompanied by the familiar features of consumption—the rapid pulse, the oscillations of temperature, and the existence of cough and of expectoration. In addition, there are usually unmistakable physical signs, but it must be remembered that occasionally the signs may be slight and indefinite if the case is one of acute miliary tuberculosis. There is generally no difficulty in recognising this form of dyspnœa, especially in patients who have previously been under observation on account of diabetes; in these the onset of the dyspnœa is noted as a gradually increasing feature of the case. Difficulty may, however, be experienced when a patient is seen for the first time, and found to be both dyspnœic and glycosuric; this difficulty, however, is more likely to be experienced by consultants than by general practitioners, since the severity of either the diabetes or the tuberculosis would, in all probability, have led the patient to seek advice long before the dyspnœa had reached a stage which might be misleading.

The onset of the dyspnœa of air-hunger is far more acute; in the cases which I have observed the patients, from a condition of comparative comfort, suddenly passed into a state of urgent dyspnœa, the rapidity of the breathing and the violent working of the accessory muscles of respiration suggesting the onset of an asthmatic attack.

The dyspnœa of diabetic conditions seems always to be connected with some error in the feeding of the patient. It has been pointed out, I think by Dr. Spriggs, that air-hunger might arise, or acidosis might arise, when diabetic patients were either taking an excess of carbohydrates, or, on the other hand, when the carbohydrate was too suddenly removed from the dietary. That is also my own experience. The cases of air-hunger I have seen have usually been cases where, in spite of the diabetes, the patients were going on living the ordinary life. In one such case the onset of the dyspnœa occurred without any previous serious interference with health or power of work; the patient had known that he had been passing sugar for some months, but there had been so few symptoms that he had attached but little importance to the condition, and had postponed all thought of alterations of habit until the completion of some important work. Then, while at work, without warning, the dyspnœa commenced and rapidly increased in severity. When I saw him within a few hours, the distress was so great that he could scarcely speak, but on examining the chest it was found that the movements, though very rapid, were deep, and there was full entry of air to all parts of the lung, with no adventitious sounds, merely the harsh sounds of the rapid entrance and exit of an unwonted volume of air. In spite of treatment the patient rapidly became lethargic and comatose, and died within some twenty-four hours of the development of the urgent symptoms. I have seen others where, with excess of energy, an alteration of diet was made too suddenly, the carbohydrates and sugars were excluded from the food too rigorously and too suddenly, and the patient developed first air-hunger and then coma. I mentioned another case when I was lecturing here lately on diabetes. This patient was the wife of a chauffeur, and was unsatisfactory from the beginning. She began with a very large amount of sugar in the urine, which was of very high specific gravity. She protested all through against the limitations of diet which were imposed upon her. She went out of the hospital, and insisted on taking any kind of food, and then one night I was called to see her and found her in this condition of air-hunger, from which she died.

Air-hunger is not identical with renal asthma. With true renal asthma you probably find that although there is plenty of air entering, it enters with difficulty. When it is a case of air-hunger,

on the other hand, the patient is taking a great deal of trouble to get a large amount of air quickly in and out of the chest, he is tossing about as though unable to get in sufficient air, and you may find at that time that there are no physical signs at all, only harsh, noisy breathing and a large volume of air passing in and out.

I said at the beginning that air-hunger is not limited to diabetes, and in this connection I want to mention briefly one case which occurred towards the end of last November. The patient died very suddenly with symptoms of air-hunger. He was æt. 56 years, and had been in the hospital with severe abdominal pain after food. The abdominal pain had commenced rather gradually six months before admission. During the six months he had lost flesh rapidly—two stones—and there had been no vomiting and no hæmatemesis. The drawn, anxious appearance led to immediate examination of the urine, as we thought from the appearance that there might be diabetes, but the urine was found to give no reactions for sugar or for albumen. A test meal showed absence of hydrochloric acid, and the provisional diagnosis made was malignant disease of the stomach, and a colleague performed an operation. It was found possible to remove from the stomach a malignant growth, which involved the greater curvature. All went well for nearly forty-eight hours, and when I saw the man forty-eight hours after the operation he was weak, but was talking quite composedly and happily. He was being fed with nutrient enemata. Suddenly urgent dyspnœa commenced, there were all the symptoms of air-hunger, and the man died in a few hours, before there was any opportunity of examining the urine. At the post-mortem examination, which was very carefully made, due search was made for pulmonary embolism, which might have explained the dyspnœa, but none was found. A few drops of urine were removed from the bladder with great caution, and were examined, and diacetic acid was found to be present, which, you know, is the substance found when coma and air-hunger develop in the course of diabetes. With that case in mind I wish to speak of another case we had in the hospital at the same time. This patient, who was not diabetic, had distinct symptoms of acidosis, and acetone and diacetic acid were found in the urine. Though the patient did not develop air-hunger she developed all the drowsiness, headache and weariness and lassitude of a patient with impending diabetic coma. She was also being fed by means of nutrient enemata on account of ulceration of the stomach, or, at all events, of profuse hæmatemesis, and when acidosis was found we proceeded at once to feed her with carbohydrates, in the same way as we should feed a patient with diabetes with acidosis. That patient recovered.

The conclusions I want to leave with you are these: that urgent dyspnœa is always of grave import in these two diseases—Bright's disease and diabetes. That in Bright's disease, when dyspnœa is found, you must determine the cause, and to which of these categories of dyspnœa the case belongs; that this is essential for the purpose of treatment. That the treatment should be hot air baths and purgatives. Diaphoretics may do good in one group, and in another group you will have to withdraw the fluid from the legs and the abdomen and the chest. And in yet another group of cases you will be successful in combating the symptoms by means of digitalis, or of ether, by stimulants, and by expectorants. That in diabetes, when dyspnœa develops, you must once more examine the chest very closely for evidence of tubercle or other pulmonary trouble. You must also examine the urine very closely for evidence of acetone or diacetic acid, and remember that this may be present even when, or particularly when, the sugar has shown signs of diminishing. Sometimes, before we have had much experience, we may be a little puzzled by the fact that the sugar diminishes and yet the patient is suddenly worse. When the sugar diminishes and dyspnœa is worse, it is almost always certain that acetone or oxybutyric acid or diacetic acid will be present. Another practical conclusion is that when this symptom of air-hunger develops in patients who are not known to be diabetic, or who are known not to be diabetic, or when there is marked apathy or drowsiness in patients who are being fed by the rectum, it is advisable to examine for acetone and diacetic acid, and when these substances are present, whether we are dealing with diabetics or with non-diabetics, it is always a safe rule to give alkalies and to give carbohydrates.

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ACUTE INTUSSUSCEPTION.*

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LADIES AND GENTLEMEN,—I propose to-day to call your attention to a condition which, though not common, may yet confront you at any time without warning. I refer to acute intussusception. As you will readily understand, I have no actual cases to show you and must confine myself to simple description; but even with that drawback I hope you will find the time not ill spent.

One is called in a great hurry to see a young child who has been suddenly taken ill. On arrival one finds the child screaming violently, with legs drawn up and evidently in great pain; or if at the time it has gone to sleep exhausted, that is the history that is given; the mother will declare that up to the time of the attack the youngster was apparently in good health; it is often the case that the child has been awakened out of sleep during the night by the pain without any apparent cause; if at the time it is seen it is in pain, the abdominal muscles are strongly contracted and the child strongly resents any attempt at examination; as the pain passes off the muscles relax, and it is easy to see that there is no true rigidity of any part of the abdominal wall, but even with a perfectly supple abdomen it may be impossible to feel anything abnormal; the pulse is increased in frequency during the attack, but returns to normal as soon as the pain is gone; the temperature is not raised at all. The mother has naturally been trying to remember everything the child has had during the past week which might possibly account for the intestinal disturbance, and it will be odd if she does not find something—some milk which might have been sour; some family disturbance which might have altered her own secretion, or somebody who held the child for a few minutes, and who must have slyly run a pin into it. She will almost certainly suggest—and the temptation is very great to agree, even if one does not originate the suggestion—that a dose of castor oil and patience will get rid of whatever is irritating the intestine and peace be once more restored. But if this is really a commencing intussusception, which it may very well be, delay and stimulation of peristalsis are the two things most to be avoided. What is the medical attendant to do?

The practitioner in question will probably take advantage of a quiet interval to consult any books on medicine which are in his library; will he find in them anything which will help him in his difficulty? My own experience is that he may, but that more probably he will not. Books at a time like this are peculiarly exasperating. He will find great stress laid upon the relative frequency of ileo-cæcal, ileo-colic, enteric, and colic forms, the relative incidence of sex and age; in the section on symptoms, to which he will naturally turn, he will find that the presence of a sausage-shaped tumour and the discharge of blood and mucus from the bowel are emphasised as characteristic, whilst in that on treatment, the relative advantages of Maunsel's operation as compared with inflation, enemata of oil, or soap and water, and the position of the child whilst these are administered, will be duly impressed upon him. Early and late symptoms are all given together with but slight hint as to what may be first expected or to which he is to attach the greater importance.

And yet acute intussusception is a condition which is not difficult to diagnose, and which it is possible to present in a practical form, and ill-equipped as I feel that I am, I propose to attempt it.

Someone, I think it is Lockwood, has said that definitions are like scalpels, necessary for the clean dissection and clear presentation of any subject; that such definitions, to be satisfactory, must combine an objective with its cause. As far as definitions do this they are useful, and, indeed, imperatively necessary. So I start with a definition.

Acute intussusception is the sudden entry of one segment of intestine into another, and is due to imperfect correlation of their respective nerve impulses.

It may be said that this is more than can be proved, but at least it forms a safe working hypothesis, one upon which the medical attendant may safely rely, and presently I hope that it will be evident that it is probably as near the truth as is possible in our present state of knowledge.

If that definition and all that it implies is duly

* Post-Graduate Lecture delivered at Ancoats Hospital, Manchester.

considered, many things will be seen to naturally follow which have the power of making the picture of intussusception stand out clear and distinct in the mind.

First of all, this condition is due to imperfect correlation of the respective nerve impulses of different segments of gut. Such a condition would be most likely to occur during the development of those nerves, and implies also a general defectiveness in development, not only of the nerves, but of the organs they supply. Naturally, then, it will happen in very early life, and one finds accordingly that the great majority of such cases occur during the first twelve months. Out of 28 cases collected by McAdam Eccles 18 were under one year of age and four more under two years. Out of 100 cases collected by J. F. Adams, of St. Thomas's Hospital, 70 were under one year. D'Arcy Power's cases were all under seven months, and my own eight cases were all but one, and that an exceptional one, under one year.

But not only is the age of the patient thus emphasised, but the location of the disorder. At birth, says D'Arcy Power, who has made an exhaustive study of this subject, the diameter of the colon is only a few millimetres greater than that of the small intestine. During the first month of life the disproportion rapidly increases, and this steadily goes on during the ensuing months. In six to seven months the colon may be four or five times as large as at birth, whilst the small intestine is but slightly increased in calibre. Such a disproportion in growth is to be found nowhere else in the whole alimentary tract below the stomach, and at least suggests, if it does not imply, a corresponding difference in the development of the trophic nerves which control its growth; and although in the great majority the proper relation is maintained, one can understand how very easily it might be disturbed. Such disturbance would also be much more probable in cases in which the whole process of development had been delayed, since such delay pre-supposes the action of some unusual factor.

That the sympathetic nerves which control, though chiefly by inhibition, the action of both ileum and colon are originally distinct, and not only these, but their intrinsic nervous mechanism, the plexuses of Auerbach and Meissner, is strongly suggested by the fact that waves of peristalsis passing along the small intestine cease at the ileo-cæcal valve, to be succeeded by others originating at this point, whilst Cannon's experiments appear to indicate that there is a radical difference between them, for whilst the ordinary movements in the small intestine are steadily from above downwards, those in the ascending colon and cæcum pass as often in the one as in the opposite direction.

Another point which almost necessarily follows from our definition. During the development of the intestinal tract the relative position of various portions of that tract alter considerably. At first the whole tube, small and large intestine alike, lies free and unattached except by its mesentery in the cœlomic cavity. When development is complete the mesentery supplying the cæcum and ascending colon becomes attached to, and blended with, the parietal layer covering the posterior wall and right iliac fossa, so giving stability to the commencement of the large intestine. In these cases of arrested development this blending has not yet taken place, and the cæcum and ascending colon are therefore free to move, being in fact one continuous tube with a continuous and free mesentery. In every instance that I have seen this was the case, and it is hardly possible that it should be otherwise if we remember how these intussusceptions progress. In the immense majority of intussusceptions the increase is at the expense of the ensheathing layer. It is not that the proximal portion passes further into the part it originally entered; the relative positions of the two segments of bowel at the point of commencement remain the same, but the ensheathing colon folds in upon itself. In order to do this it must itself be free to move.

Many varieties of acute intussusception are to be found in text-books; the only one worth remembering at this stage is the ileo-cæcal, the entrance of the ileum preceded by the ileo-cæcal valve into the colon, which treats it as a foreign body—as, indeed, it is when it takes this position—and endeavours to get rid of it in the usual way by forcing it onwards. This form constitutes 90 per cent. of all cases of acute intussusception.

The entry of this particular segment of bowel into that below it naturally has three results:

(1) The region in which the cæcum usually lies becomes flat because no longer supported by the

air-cushion-like bowel beneath, not only giving a flatter resonance on percussion, but actually flatter on inspection. This is called the sign of Danz.

(2) A tumour is formed. The thin coats of the intestine are not normally evident to palpation, but when doubled upon one another, and containing as well the thicker ileo-cæcal valve, they become much more recognisable; moreover, this altered position cannot be maintained for long before the compressed internal gut becomes œdematous, thus adding to the volume of the substance felt. In eighteen of the twenty-eight cases noted by Eccles a tumour was felt; it was also found in 87 per cent. of Adams's cases. But it is not *always* felt, and it will be noted that it is best recognised in the later stages, and not in the earlier; so that its recognition, though useful, is not indispensable. Various reasons for its non-discovery may be given; it may be hidden beneath the liver, having progressed only as far as the hepatic flexure; the child may be, and often is, fat, and the mass to be felt small; the mass is most marked during a paroxysm, when the peristaltic wave thickens and stiffens the muscular coats. At such a time the child is suffering the greatest pain, it is most resentful of any attempt at examination, whilst its abdominal muscles are contracted, and so tactile examination becomes most difficult.

(3) All intestine derives its vitality through the mesentery to which it is attached. This membrane supports the arteries which carry the blood to, and the veins which return it from, that portion of the gut to which it is attached; it also contains the nerves which, at least to some extent, control the action of that gut. One segment of bowel cannot enter another without carrying with it this membrane, and at the same time these vessels and nerves. As it enters they are compressed along with the intussusceptum by the intussuscipiens, which is excited to abnormal contraction by the presence of the foreign body within it; the pressure so exerted will be first and mostly felt by the thinner-walled, and therefore more compressible, veins. The result will be, and must necessarily be, that whilst blood still enters the intussusceptum through the more resistant arteries, it finds great difficulty in returning. The inevitable consequence is that the entering bowel becomes gorged with blood. Some relief to the over-full vessels and capillaries is obtained by the transudation of serum through the capillary walls into the connective tissue of the mucosa and submucosa of the constricted intussusceptum, which becomes œdematous and therefore much more swollen, *but also by the giving way of the delicate vessels of the mucosa*, and the escape of blood into the lumen of the distal gut. The mucous glands of the bowel are also stimulated by the continued retention of blood in the part to increased action, and a greater flow of mucus is the result. Thus inevitably in every case two characteristic symptoms are produced—blood and mucus in the distal bowel. This may be expelled *per anum*, but since constipation is usually caused by the obstruction above, they may not. But these two, blood and mucus, are always there, and trickle down to, and collect above, the sphincter, so that a finger introduced within this will return blood-stained.

And it will be noted that in children of this age, who have been previously in good health, nothing but some mechanical obstruction to the circulation, such as either this condition, or the pressure of a band, or the constriction due to an internal hernia, could bring about the production of this symptom—the presence of bloody mucus inside the sphincter. All these conditions require similar prompt and effective surgical treatment.

From what other conditions does this require differentiation? Most especially, as most commonly, from simple colic. In both there is paroxysmal pain, whilst usually in both there will be a history of some error in diet; the mother will search her memory for all manner of possibilities, and it will be odd if she cannot find something—and somebody—to blame (I suggest that very little weight should be attached to her reminiscences). In both there may be vomiting, in both the temperature is normal or but little altered; so also is the pulse; this may be quickened in both during a paroxysm, to return to normal as soon as the child is once more quiet. In both, when the child is at rest (the only time when any real examination can be made), no lump may be detected. How are two such similar (so far as symptoms go) conditions to be distinguished during the short time which, as I shall try to show later, it is safe to wait before interference?

In simple colic there is no blood in the rectum. In intussusception there must be within a short time—four to six hours at the most—a time during

which it is safe to wait, during which simple coloured dill water may be given and the case watched. No preparation of morphia or belladonna should be allowed until the diagnosis is made, but heat in the shape of hot cotton-wool may be applied to the abdomen.

Other symptoms are given in books, but they are *late* symptoms. They occur too late and only after so much damage has been done as to be all but irreparable. Amongst these are:

The formation of a sausage-shaped tumour.—The early shape of an intussusception is round. It is only when it has existed for some time—an unsafe time—and has extended for some distance—an unsafe distance—that the pull upon the mesentery causes sufficient curvature as to be recognisable through the abdominal wall.

The recognition of the apex of the intussusceptum in the rectum.—It is hardly necessary to point out how far the ileo-cæcal valve must have travelled before it could have reached this point. The whole ascending colon and at least half of the transverse must have doubled upon itself before the examining finger of the surgeon would be able to reach its apex in the rectum. How long must it have taken to reach this stage, and what an immense amount of dragging and compression must the meso-colon have undergone during the process? Why should the surgeon wait until such a sign is present, especially as it will be evident when treatment is discussed that almost all chance of recovery will have been lost before matters have progressed so far?

The expulsion of bloody and mucoid stools.—A frequent result—so frequent as almost to be the rule—of any intestinal obstruction, is cessation of peristalsis in the distal section. It could only be in unusual cases or when an unusually large quantity had been poured out that such stools would be obtained, but the mere presence of undoubted blood within the sphincter is sufficient for certainty. What object is gained by waiting for its accumulation in quantity?

All these things are practically useless for early recognition, and the futility of waiting for them needs emphasising, since the time thus lost carries the little patient beyond the stage in which the only safe method of treatment can be carried out.

There are certain other conditions in which blood and mucus may be found in the way indicated; they are seen when an intra-intestinal growth is present—a condition at such an age extremely rare, but producing, when it does exist, almost the identical condition now described and requiring similar treatment; occasionally in tuberculous disease, especially if a band has been formed, compressing the bowel, also requiring similar surgical action; and in Henoch's purpura, from which it is easily distinguished by the more gradual onset of the latter, the characteristic spots on arms and legs—absent in the condition under review—and the fact that the exudation of blood is not confined to the bowel, which alone contains it in intussusception, but exudes from all mucous surfaces indiscriminately, the urine and vomit being also blood-stained.

When, therefore, sudden wave-like pain has occurred and recurred in a child previously healthy, especially if under two years of age, and blood and mucus can be demonstrated as present in the rectum above the sphincter and nowhere else, there is no need for further hesitation—the abdomen must be opened.

The abdomen must be opened, but is it necessary to proceed to a cutting operation upon the delicate structures within? No! The main object of such an early opening is to avoid that very thing. Just as in strangulated hernia the operation for relief becomes a mild and innocuous or a severe and dangerous proceeding, according to the length of time the compression has been allowed to continue, so it is here. Within the first six to twelve hours reduction in full sight, and without the hampering interposition of the abdominal wall, is easy, harmless, and effective. Much has been said and written about adhesions between intussusceptum and intussuscipiens, of the swelling of the intussusceptum impeding its return, of the danger of rupturing the serous coat of the distended sheath, but these are late changes, and are never present in the stage at which exploration ought to be made. The bowel in the early stage is not adherent, is not greatly swollen, and reduction may readily be carried out by gentle compression from below upwards, assisted, if necessary, by equally gentle traction from above, *and this reduction is the only safe method of treatment.*

All other methods of treatment, if they are intended to take the place of this, are mere waste of time; not only so, but they are often extremely

dangerous. Inflation by air or hydrogen succeeds so rarely that it is not worth consideration. Injection of fluids has had some successes, but they have also directly caused death. R. W. Parker recorded a case in a child, æt. 3 months, in which the gut burst under the pressure of simple soap and water, and others are reported in the literature; inversion of the child or tossing in a blanket, as suggested by Jonathan Hutchinson, need only mention; attempts at reduction through the abdominal walls is practically impossible. To all these methods three objections must be urged, which, to my mind, are fatal: First, that in all the surgeon is acting blindly; the force he applies will almost certainly be exerted in a wrong direction, whilst he may easily produce irreparable damage; and secondly, any reduction he may by chance produce will not be complete; whilst thirdly, none of them do anything to ensure non-recurrence. Moreover, the time consumed by such efforts wastes that during which the open method of reduction is likely to succeed, and such reduction may thereby have been rendered impossible.

And this consideration is of the greatest importance. The results of all cutting operations upon the bowel in children of this tender age are lamentable. In the later stages, when the bowel is greatly distended and swollen, when adhesions have formed between the entering bowel and its sheath, when the child's strength is exhausted by pain and perpetual vomiting and straining, such operations may present the only possible course open to the surgeon, but what chances are then left for success? As a matter of fact the conclusion is more and more forced upon all operators that they are all but foredoomed to failure. Lockwood has seen no case operated upon in this way recover. Childe, of Norwich, says the same thing. D'Arcy Power speaks in the same way. In the summary of foreign literature in the 'British Medical Journal' for November 5th of last year two cases were reported by Viarmay; one recovered, being reduced, the other, operated upon by Maunsell's method—the best of all—died. I myself have never seen a case operated upon by removal or division of the gut recover. I have twice opened the intussuscipiens and excised the intussusceptum as Maunsell advised, but both died. In another case I excised the whole mass and reunited the distal and proximal segments by lateral anastomosis, with an equally fatal result; in five cases, on the other hand, I have been fortunate enough to see the case within the first twelve hours, have opened the abdomen and reduced the intussusception; four recovered completely, and one appeared likely to recover, but died ten days later of pneumonia. The fatal cases all died from shock with this exception. The only chance for these children lies in early diagnosis, early laparotomy, and reduction in full sight.

This, of course, applies only to these very young children; in older ones Maunsell's operation is often successful, and is, as I have said, the best of all; but such cases are not due to defective correlation of nerve impulses, but to the presence of some pathological cause, as the growth of a small adenoma, or fibroma, which acts as a foreign body. In 1905 I reported such a case:

A child, æt. 8 years, was admitted into Ancoats Hospital complaining of severe abdominal pain occurring in paroxysms which had commenced the same day at 8 a.m. The bowels had been moved at 8 30; the operation took place at 11.30. The abdomen generally was supple; a mass, the size of a tangerine orange, could be felt during a paroxysm in the right umbilical region; this mass was smooth and mobile, with rounded outlines and surfaces; it was tender on touch. The finger introduced into the rectum was stained with blood on withdrawal.

In this case, though seen early, reduction was impossible because of a pedunculated fibroma springing from the internal wall of the gut. Maunsell's operation was done, and the patient recovered, but it is to be noted that the child was eight years old, and that we were fortunate in being able to open the abdomen within four hours of the occurrence.

When the abdomen is open, and not before, is the time to consider varieties of intussusception. Then it is that the surgeon must decide whether he has to deal with the common ileo-cæcal or with one of the rarer varieties—ileo-colic, enteric, or colic: they are all amenable to the same treatment, and merely differ in position, or, in the case of the ileo-colic, require some little extra attention in order to be certain of their complete reduction. But any difficulty encountered does not justify waste of time before operation in order to attain certainty, which, indeed, is almost impossible with

an intact abdomen. If this is only thoroughly realised, much of the medical man's perplexity in these cases will disappear. Time taken up in discussing these points before operation is simply wasted. And the same is the case with regard to possible tuberculous bands, the chance of finding an internal hernia, etc.; the distinction is easily and rapidly made when the abdomen is once open; it may be impossible before. Of course, the surgeon must be competent to deal with whatever he may find.

But if the intussusception is reduced, may it not recur again? D'Arcy Power relates one case in which a child was operated on twice during the first seven months of its life. Reduction simply leaves the bowel in the state and under the same conditions which obtained previous to the occurrence of the lesion. Can we do nothing to prevent such recurrence?

Certainly we can! and it is surprising that more stress is not laid upon this fact. Returning to our definition, it will be noticed that much importance is attached to the element of imperfect development. What is the particular failure in development in these cases which permits of this malplacement in the first instance and allows of its return in the second? It is the fact that the right lamella of the meso-colon has not yet blended with the parietal peritoneum lining the posterior wall and right flank of the abdominal cavity.

Before the colon can fold upon itself it must be free from any attachment except that of its own mesentery, which in such cases is long and loose; it should also be remembered that its mesentery is originally in the same plane as—is, indeed, a continuation of—that of the small intestine, and it is only after its adhesion to the parietal peritoneum below it that it becomes the short, almost absent meso found in well-developed adults. When once the colon is thus anchored, and still more if the cæcum is also adherent, intussusception of that gut becomes, if not impossible, at least tremendously hampered. It is not necessary to imitate the natural process to the full extent by attaching the whole peritoneal surfaces; it is quite sufficient to fix the cæcum to the right iliac fossa by two or three sutures, the rest being brought about by the weight of the superposed small intestines. The abdomen may then be closed completely.

In conclusion, one may briefly summarise the points to which attention is desired; they are—

(1) The sudden onset of attacks of peristaltic pain in a young child, previously in good health, associated with the presence of bloody mucus above the sphincter within a few hours after commencement, justifies immediate operation.

(2) The immense probability under such circumstances is that acute intussusception is present, but time is of so much importance that it is merely wasted in attempting to arrive at a complete diagnosis before operation.

(3) The proper time for this is during, and not before, exploration.

(4) The only safe and efficient treatment of acute intussusception consists in reduction of the intussusception after the abdominal wall has been divided, followed by fixation of the cæcum. For this to be successful it should be performed within twenty-four and preferably twelve hours of its duration.

(5) All cutting operations which involve division and suturing of intestine at the age at which acute intussusception occurs are to be avoided if possible; they only become necessary if interference has been too long delayed, and are usually fatal.

(6) Other conditions yielding similar symptoms in which abdominal section is contra-indicated, as, for example, Henoch's purpura, are differentiated by previous ill-health and a slow onset, besides other easily recognisable signs.

April 3rd, 1911.

THE preparation of bacteriological culture media on a small scale has always been tedious and unsatisfactory. Experiments in the Wellcome Physiological Research Laboratories have shown that many of the ordinary culture media can be dried and kept in this condition for long periods, regaining their original consistency and appearance on being re-dissolved in the requisite amount of water. Chiefly to meet the needs of the physician or bacteriologist who requires to use a few culture tubes only, and at uncertain intervals, the "Soloid" nutrient media have been introduced. These products are dry and portable, will keep indefinitely under proper conditions, and render it possible to prepare a tube or plate of the required medium within an hour, without laboratory or apparatus. "Soloid" bile-salt agar-agar (MacConkey), "Soloid" bile-salt (MacConkey), "Soloid" nutrient broth, "Soloid" nutrient gelatin, and "Soloid" (nutrient) agar-agar are useful examples of this class.

WITH DR. GUTHRIE RANKIN AT THE SEAMEN'S HOSPITAL, GREENWICH.

GENTLEMEN,—We saw this man a fortnight ago, and there was a great deal about him to suggest that he was suffering from myxœdema; his appearance, his speech, the texture of his skin, the whole condition of the man justified that idea. But you will remember I pointed out that the facts against it were his age; secondly, his sex; and thirdly, that after he had come in and been observed more carefully, we found he had albumen and blood in his urine. We put him upon thyroid gland, and thought that if there was a myxœdematous element in the case the physiological test would prove it for us. But meanwhile, during the last week, he has developed a left hemiplegia, which confirms the suspicion that the myxœdema, if it exists at all, is only part of his pathological condition and not the primary ailment. His age is seventy-two, his vessels are thickened and tortuous, his pulse is irregular, and although it may be that some of the blood which appears in his urine is from the prostate, there is kidney mischief as well, and the facies which so closely simulates myxœdema is probably mostly caused by his kidney disease, the arterial changes being secondary to interstitial nephritis. In cases where there is hemiplegia in a man who is practically unconscious from it, you get help very often from an examination of the knee-jerks, because on the side of the hemiplegia you will have the reflexes increased and Babinski's sign markedly present. This is well exemplified in the case of this patient, as I now show you. When the unconsciousness is so deep that you cannot tell whether the patient is hemiplegic or not, you get a guide from these signs as to what is the true cause of the coma.

The next patient is one who has a double aneurysm, one of them in the transverse arch of the aorta, and the other in his axilla. He has had eight injections of 100 c.c. of a 2 per cent. solution of gelatine in saline solution, injected twice a week into the inner side of first one thigh and then the other. No serious discomfort has been produced by the injections, and there has been no disturbance of temperature, as you will see from his charts. The man says he feels a good deal better after them in regard to his subjective symptoms; he can now move his arm freely, and the œdema is disappearing from his hand, so I think we may conclude there is obviously some diminution in the pressure. A fortnight ago we were agreed that the axillary swelling was diminished, and I think to-day it is less still. There is still some pulsation, but the tumour is less expansile. The thrill which was so noticeable before can only be elicited to-day by deep pressure. He is sleeping well now, whereas his nights were very seriously disturbed prior to the gelatine administration, and the pain, from which he suffered intensely, has almost entirely disappeared. I beg of you to keep this plan of treatment for aneurysm present in the back of your minds, for it seems to be a valuable addition to other methods. Our previous experience has been that something like twenty injections are required to produce real benefit. The improvement in this patient may not be altogether due to the gelatine, for he is also having his liquids restricted to two pints *per diem*, and is taking 10 gr. of iodide of potassium thrice daily. It is to be noted, however, that these measures were in operation for a considerable time before gelatine was resorted to without producing any effect upon either the pain or sleeplessness. If due care is taken in the proper sterilisation of the gelatine no untoward results need be feared from its administration. The injections should be very slowly delivered intramuscularly, and not subcutaneously; by these precautions all pain is avoided, except that momentarily due to the plunging of the needle into the tissues. I show you the simple apparatus which we employ in these wards for carrying out the treatment, and those of you who care to see its practical application can do so by returning with me to this patient at the end of our present demonstration.

Our next patient is a pensioner who has been in the hospital many years with locomotor ataxy, and it is useful to contrast him with the man we saw last week suffering from general paralysis of the tabetic type. This patient has no symptoms of general paralysis of the insane. He is in the third stage of tabes and is practically confined to bed altogether. He presents all the characteristic symptoms of the disease, and has reached that period of his illness when very little in the way of treatment is of any avail except to relieve sym-

ptoms. One remedy after another was tried in the earlier stages in the hope of producing some beneficial influence on his spinal cord lesions, but without any good result. When first admitted, his disease was so advanced that he had completely lost the control of his legs and was quite unable to walk. It is worth recording that after a prolonged course of Fraenkel's exercises this disability was so far overcome that for a time he regained sufficient co-ordinate power over his muscles to be able to get about both in the hospital and out of doors with the aid only of a stick. The improvement, though not of long duration, was sufficiently marked to demonstrate the possibility of re-educating the power of balance by such methods. In the case we investigated last week the brunt of the disease had fallen upon the brain; in this patient the same disorder is present, but its disastrous effects are manifested entirely by degenerative changes in the spinal cord. There is one point more in the history of locomotor ataxia which this patient illustrates vividly. Despite his extreme ataxia, which may be said to have been throughout his illness the leading symptom, he has escaped all trouble in his optic nerves. He can see to read as well to-day as ever he could, and his discs are, to all intents and purposes, normal.

We will now look at a patient who was admitted this morning complaining of pains in the joints and swelling in the feet and legs. He contracted gonorrhœa in the beginning of last month, and this is the start, probably, of his present illness. The gonorrhœa became better, but at the end of last month he had pains and swelling in the right wrist, followed by pain in his left hip, and during the last week by pain in the left ankle. The swelling in the legs began three days ago and there has also been swelling of the face, especially in the morning. In the middle of last month there was blood in his urine, which lasted only a few days. He has not had any other illness for many years, but he has in bygone times suffered from yellow fever and malaria. He has also had syphilis. He drinks beer moderately and smokes about two ounces a week. There is nothing particular in the family history so far as we can ascertain it. His face has that pale and pasty aspect which is always suggestive of renal disease. He has some decayed teeth, his bowels are constipated, his pulse is small, hard, and of high tension, his arteries are atheromatous, and his skin is glossy over the œdematous parts. His apex-beat is not visible or palpable, and the cardiac sounds are faint and of a "tic-tac" character, the first sound being short, sharp, and closely resembling the second. At the aortic area the first sound is hardly audible; the second apparently has a relative accentuation. The lungs are emphysematous; at both apices the note is impaired in the supra-spinous fossæ and there are some moist rales at both bases. There is a slight cough at night. Liver dulness is diminished, reaching from the sixth to the eighth ribs only; the spleen is not enlarged; the urine is pale and of low specific gravity, and contains 1·8 per cent. of albumen. There is dilatation of the left pupil, and cataractous changes in the lens, the result of an accident some years ago. The microscopical appearance of the urine is recorded as follows: "There are large numbers of casts, both coarsely and finely granular, and a fair number of hyaline casts: numerous white blood-cells, some reds, many epithelial cells, some of which are derived from the kidneys. No crystals present in the films."

The case, as you will gather from the report which Dr. Gilchrist has read, is somewhat complex. It is not a straightforward text-book story. His arterial tension is only 110, which is much less than we would expect to find in a man with renal disease of any duration. In passing it may be mentioned that tension is a variable quantity and is always difficult to estimate, but there are two or three guides which keep us in the right track, apart from mechanical measurement such as is recorded by the sphygmomanometer. The pulse of high tension is small, hard, incompressible, of the whip-cord description, and the difference in its ratio in the recumbent as compared with the erect position is markedly greater than in a normal state of the circulation. It is usually associated with more or less left ventricle hypertrophy and with an accentuated second aortic sound. There is no doubt of the existence of kidney mischief here; the casts and albumen are sufficient evidence of themselves, and their testimony is confirmed by the existence of dropsy. Admitting that he is suffering from a subacute nephritis of recent onset, and further allowing that his arthritis is in all probability of the gonorrhœal type, the difficulty which arises is to decide whether the two conditions are merely an

accidental coincidence, or whether the kidney condition is, like the joint condition, a sequel to his recent venereal disorder. The urethral discharge has ceased, so that it is impossible to demonstrate the existence of the gonococcus from that source. We will, however, have his blood examined in the hope of finding the organism, but meantime the distribution and type of his joint lesions is strongly suggestive. Such an early implication of his joints makes it likely that the infection, if gonococcal, is virulent, and points to the theory of his kidney disturbance being toxic as reasonably probable. If the gonococcus is found in his blood, this patient is one in whom pre-eminently treatment by vaccination ought to prove serviceable, and though our experience hitherto in these wards of gonococcal injections has been extremely disappointing, we will again endeavour to prove its value in this case if the bacteriological examination of the blood justifies such a course of management. Meantime the ineficient action of the kidneys demands primary attention. His immediate treatment must be active, and I suggest that he should have a hot pack once every day lasting an hour, so as to get the skin to act freely; that he be put upon a simple diuretic mixture; that his diet should consist exclusively of milk; that he be given "imperial drink" freely; and that each morning he should have an active purgative.

The last case I shall ask you to look at is one of rupia. The sores on the buttocks and legs are, as you see, very characteristic and very nasty. Last week when this patient was first seen it was decided to treat him on ordinary lines with perchloride of mercury and iodide of potassium. For the past week he has had twenty grains of iodide of potassium and a drachm of liquor hydrargyri perchloridi three times a day, and his sores are already both cleaner and smaller. The eruption is undoubtedly specific, and the patient fully admits infection about two years ago. A Wassermann reaction has been done and is positive. As regards this reaction, however, about which so much has been heard recently, I would remind you that it is met with in other conditions besides syphilis, so that we must not attach to it more importance than it deserves.

The tendency of the present day is to give undue prominence to laboratory research. No one will deny the enormous help which is now given us by bacteriological investigation, but it ought to be regarded as an addendum to, and not a substitute for, clinical evidence. We all remember how the Widal reaction in enteric fever came almost to be regarded as the diagnostic criterion of the disease, until it was ascertained that it was not pathognomonic but occurred in other conditions with which enteric fever had nothing to do. It is not improbable that the Wassermann test will by-and-bye, in the same way as the Widal test, be regarded as useful confirmatory evidence in doubtful cases, and no more.

This case is one of the best possible examples we could have of specific disease for proving the value of Erhlich's new remedy "606," about which so much is being spoken at the present time. Fortunately we have a lucky opportunity of doing this, because one of our number—Fleet-Surgeon Holme—is possessed of a small quantity of this priceless remedy, and he has very kindly consented to inject this patient to-morrow or next day. The result of this new form of treatment is said to declare itself rapidly, so that when we look at this man together again next week, we shall hope to find that the results which have ensued from the old method of drug administration have been brilliantly eclipsed by this new and much-praised preparation. All other treatment will, of course, be temporarily suspended. Mr. Russell Howard has been lecturing recently on this subject, and he tells me he has seen many cases injected with "606" at the London Hospital, but that he is still undecided in his conclusions about the permanence of its curative value. The most striking point about the treatment by this new arsenic compound is, he says, the rapid disappearance of the spirochætes from the blood. We do not know yet whether they will come back subsequently, but there seems to be no doubt that "606" has a very definite effect in chasing them away.

April 3rd, 1911.

In answer to "Enquirer," the intensive irradiation and wave-current method of Dr. L. E. Creasy in the after-treatment of fractures and in the alleviation of pain in and about joints was commented on by Mr. F. C. Wallis, Surgeon to Charing Cross Hospital, in a letter published lately in the correspondence columns of the 'British Medical Journal.'

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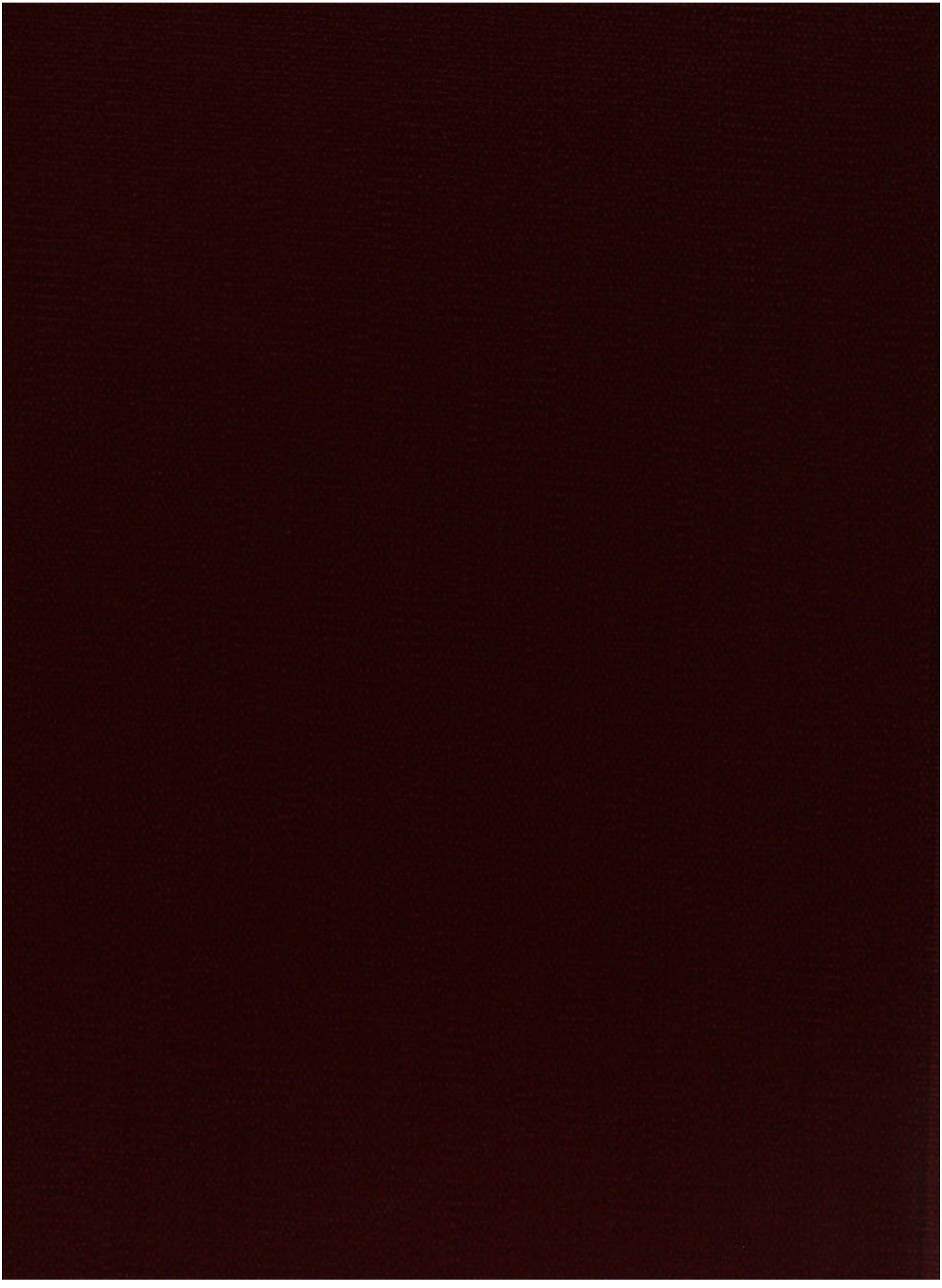
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